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1. *Parturition, and the Principles and Practice of Obstetrics.* By W. TYLER SMITH, M.D. Lond., Lecturer on Obstetrics in the Hunterian School of Medicine.—*London,* 1849. Fcap. 8vo, pp. 396.
2. *Obstetrics: the Science and the Art.* By CHARLES D. MEIGS, M.D., Professor of Midwifery in the Jefferson Medical College at Philadelphia, &c. &c. With 121 Illustrations on Wood.—*Philadelphia,* 1849. 8vo, pp. 686.

IT seems to us not a little strange that writers who show a considerable amount of acuteness on other topics, should continue to entertain such vague and confused ideas with respect to the relation between Science and Art, as are presented by the authors of the two works before us, in the opening sentences of their respective treatises. According to Dr. Tyler Smith,—

> "Labour: the study of the act of parturition itself, and of all that relates to the prevention or alleviation of the pangs and dangers in which women bring forth children, and to the preservation of their offspring, are the principal aims of the Obstetric Art." (p. 1.)

We do not precisely understand the relation of the first word in the above quotation to the rest of the sentence from which it is separated by a colon; according to all ordinary rules of punctuation, it is entirely isolated; and although certain clever writers of fictitious or imaginative works have lately broken through all these for the sake of *effect,* we shall set our face against the introduction of any such method, or rather want of method, into treatises in which the clear and sober exposition of *truth* is the only object. Leaving the word Labour, then, to stand alone, like the monosyllabic noun-verb which in the Chinese language forms a sentence by itself, we shall inquire into the meaning of the remainder of the passage. Inverting the sentence, we find Dr. Tyler Smith asserting that "the principal aims of the Obstetric Art" are "the study of the act of parturition itself, and of all that relates to the prevention or alleviation of the pangs and dangers in which women bring forth children, and to the preservation of their offspring." In our apprehension, this is rather a definition of the *science*, than of the *art*, of obstetrics. The object of the art is to alleviate the sufferings, and preserve from the dangers, incident to the performance of the function in question; that of the science is to *know how* to alleviate. The art consists of a set of rules for practice, deduced from the scientific study of the function in its normal and abnormal conditions; and these may be successfully applied by a practitioner, who knows nothing of their rationale. On the other hand, the science is evolved from the philosophic contemplation of the phenomena presented by clinical observation, guided by an acquaintance with the general principles of physiology; and this may be successfully pursued by a man who has never conducted a case of labour for himself. Although we are far from thinking that a mere observer is the man best qualified for such a pursuit, yet, on the other hand, we are confident that he would be much more likely to contribute towards the advancement of the science, and the consequent improvement of the art in precision and certainty, than the mere practitioner who isolates this department of biology from every other, and looks at the parturient woman as a being with whom no one but himself has anything to do.

Let us see whether Dr. Meigs is more successful in his definitions. The following are *his* opening sentences:

> "Midwifery is the art of assisting women in labour.
>
> "Obstetricy comprises the sciences of anatomy, physiology, and pathology, as relates to the reproductive organs, and the arts of therapeutics and surgery, as applied to sexual affections in women.
>
> "Midwifery is an Art.
>
> "Obstetricy is a Science.
>
> "A Midwife or Accoucheur is one who assumes the conduct of cases of labour.
>
> "An Obstetrician is a physician, who, in addition to a general knowledge of physic and surgery, adds the special information that is necessary for one having the peculiar charge of all sexual affections, whether in the department of midwifery proper, or in other complaints of the sex.
>
> "Notwithstanding obstetricy is composed of several different branches or sorts of knowledge, it claims to be considered as a distinct science." (p. 17.)

In these oracular dicta, we have an attempt at greater precision of statement; and we believe that, by a little amendment, they may be made to express the true view of the subject. We will take Dr. Meigs's definitions of *Obstetricy* as the *Science*, and *Midwifery* as the *Art;* and inquire what are their respective provinces. Obstetricy, strictly speaking, consists of those departments of the sciences of anatomy, physiology, and pathology, which relate to the reproductive function; but as the obstetric practitioner is the one generally consulted upon the diseases and injuries to which the female sexual organs are liable (unless these diseases, like syphilis, be purely constitutional in their nature), the whole pathology of these organs (with the exception we have named) must be considered as included under the designation of obstetrics. Now so far as obstetrics is a science,—in other words, to use the definition of Mr. John Mill, as it consists of a collection of truths, whose language is "this is, or this is not; this does, or does not, happen,"—it is perfectly independent of any art; and we cannot, therefore, admit the justice of Dr. Meigs's assertion that the science of obstetricy comprises the arts of therapeutics and surgery as applied to sexual affections in women. These arts, again to use Mr. Mill's definition, are collections of rules, whose language is "do this, avoid that;" and therefore they cannot constitute a department of obstetric *science*, but belong to the *art* which is founded upon it. To include the whole of this art under the designation midwifery, would be to make a wide extension in the application of that term; which is usually restricted to the care of the act of parturition, and its antecedent and consequent phenomena. To such an extension there are many obvious objections; and we do not see what else is to be done, than to distinguish carefully between obstetric *science* and obstetric *art*,—just as we distinguish between the science and the art of medicine or of surgery,—and to let midwifery continue to relate to that division of the art, which the term has hitherto been employed to designate.

Although we have not considered either of our authors very successful in the formal definitions, with which their treatises respectively commence, yet the distinction between *principles* and *practice* is most fully recognised by both—in their teachings, as well as in their titles. In fact, Dr. Tyler Smith's object is to give to Obstetric Science a far higher elevation than it had previously attained, by connecting it with those general principles of Physiology of which a large proportion of obstetric practitioners, and too many obstetric writers, are profoundly ignorant. For his labours in this vocation he deserves great credit, whatever we may think of his success; and if we shall find it our duty, in the discharge of our critical office, to express our dissent from several of the doctrines which he has propounded in the treatise before us, we do not the less feel that he deserves the gratitude of the profession, for having pursued the study of obstetric science in a more enlarged spirit than any preceding writer, and for having diligently attempted to improve and extend it.—We quote his preface in full, as expressing what he conceives to be his claims as a discoverer, and as justifying the careful examination of these claims upon which we shall presently enter.

> "I began to study Reflex Obstetrics in 1842; and the present work is the result of seven years' close and earnest attention to the subject. I may say truly, that during this time, though much occupied by other matters, it has scarcely ever been absent from my waking thoughts.
>
> "I have no wish to deprecate criticism; but I trust I shall not be considered in the light of one who applies facts and principles already known to his own department of practice. I believe every candid person conversant with the current knowledge of the reflex function, and of obstetrics, when I began to write, must admit that I have both added to reflex physiology, and made extensive applications in practice, which had eluded previous observers. Indeed, reflex obstetrics is a new department of the reflex function and its applications. Taking the whole range of reflex physiology, the cause of labour is only second in importance to the cause of respiration; and no one had perceived that the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration; while, in the investigation of the causes of the Genesial Cycles, in the Twelfth Lecture, I have entered upon a new field, altogether distinct from the reflex motor function.
>
> "When I published my first 'Observations,' reflex physiology had not found

even a verbal home in any work on Obstetrics; but I do not think it will be possible to say the same of future works in this department of medicine." (p. viii.)

Dr. Tyler Smith's treatise, like the lectures already published in the 'Lancet,' of which it is almost entirely always made up, is much less an introduction to the study of obstetrics, than an exposition of the author's peculiar views; and is consequently less fitted for the pupil who needs a comprehensive text-book of the science, than it is for the advanced student, who is prepared by his previous acquirements to eliminate its truths from its errors, and to profit by the former without being led astray by the latter. As a treatise on the "Science and Art of Obstetrics," it is deficient in much that the pupil needs to be taught; as a dissertation upon "Reflex Obstetricy," it is replete with interest. The latter title would therefore, in our estimation, have been more appropriate than the former; and we shall discuss the subject-matter of the treatise as if it had been thus designated. This, in fact, will be the fairest method of procedure in regard to the author; for we are sure that he will be best satisfied by our bringing into prominent relief those portions only of his treatise upon which he bases his claims as a discoverer, passing by with little or no mention those which contain no distinct novelty. We shall find that the former will afford such ample food for critical discussion, as to necessitate our restricting ourselves in the present article to the theoretical portion only of Dr. Tyler Smith's work.

The corresponding department of Dr. Meigs's treatise presents so little that varies from the beaten track, that a few words will suffice to dismiss it, especially as we had occasion not long since to criticise some of Dr. Meigs's opinions on these subjects, as well as his manner of expressing them. With regard to the style of the present treatise, we have the satisfaction of informing our readers that whilst it presents many of the peculiarities which characterised Dr. Meigs's "Letters to his Class on Females and their Diseases," it is free from the most offensive faults which disfigured that work. The colouring is in fact considerably 'toned down;' and would perhaps be altogether not more lively than is sufficient to make the book very agreeable reading, were it not that the old affectations every now and then force themselves in, with somewhat unpleasant obtrusion.—We noticed on a former occasion (Vol. III, p. 142,) Dr. Meigs's notion that the "corpus luteum" is a vitellary body. On this subject he expatiates somewhat fully in the present treatise; and informs us that, when boiled, the corpus luteum becomes hardened like yelk boiled hard, and that, when roasted, it gives out a strong odour of roasted eggs! From the result of this truly refined and trustworthy procedure, in addition to the microscopic observations formerly referred to, Dr. Meigs deduces what we must regard as a most loose and unphilosophical analogy between the corpus luteum and the vitellus. Their similarity in composition, histological and chemical, however close it may be, does not indicate any real analogy, their position and functions being so essentially different. A vitellus is *not* a vitellus because it is composed of albumen, oily matter, sulphur, &c., which the chemist finds in it; or because of the floating corpuscles which the microscopist detects; but because it is so placed *within* the ovum as to afford a supply of nourishment to the developing embryo, which shall be converted into its own textures. This is the essential *idea* of a yelk or vitellary body; and to give the designation of vitellary body, therefore, to a substance which is only developed after the ovum has escaped from the ovisac, and even on the exterior of the ovisac which enveloped the ovum, and having no function whatever as regards the embryo, appears to us a complete perversion of language.

We are surprised to find Dr. Meigs greatly behindhand in his knowledge of the structure of the placenta. For anything that his account of it contains, it might have been written ten or twelve years ago, when as yet neither Professors Weber, John Reid, or John Goodsir had unravelled its complicated arrangement. The old questions about the fœtal and maternal portions of the placenta, are discussed in the old style; and the author gravely comes to the conclusion that the placenta is entirely a fœtal organ, and that the notion of the passage of vessels, or of maternal blood, from the uterus to the placenta, is all a delusion. He rests his convictions upon the evidence of his own senses, having on more than one occasion seen nothing but "mucous tractus" passing between the two surfaces when torn apart from each other, and having thus satisfied himself of the total absence of vascular communication. When he shall have prosecuted a dissection of the parts in question, under water, and with the requisite slowness and care (that of Dr. J. Reid occupied him several hours a day, for some weeks, we believe months); when he shall have followed, with Professor Goodsir, the development of the placenta, and shall have seen how it is made up of uterine tufts and decidual growth; and when he shall have microscopically examined the placental tissues, injected and uninjected, and shall have accounted for the presence of maternal blood in the placental cells, whilst the fœtal blood is restricted to the tufts which constitute the ultimate subdivisions of the umbilical vessels; we will allow him to have an opinion of his own. Until then his assertion of the entirely fœtal nature of the placenta is altogether valueless, and he is bound to teach the opinions of those who *have* made such examinations. For ourselves we can say, that having witnessed a considerable part of Dr. J. Reid's dissection, we can bear unequivocal testimony to the correctness of his results; and that having also gone carefully through the microscopical examination of the placenta, we can equally confirm the statements of Professor Goodsir in regard to the combination of fœtal and maternal structures which it contains. Dr. Meigs lays great stress on the fact that the whole placenta is thrown off in the human female, instead of the fœtal portion only, as in the lower mammalia; but he takes no account of the circumstance that the decidua also, of which the maternal portion of the placenta is only a peculiar development, undergoes the same detachment. In regard to the real nature of the decidua, too, we are surprised to find Dr. Meigs apparently ignorant of the satisfactory elucidation which it has received in this country; he quotes M. Coste and other continental authorities glibly enough; but for him Professors Sharpey and Goodsir have observed and described in vain, and the decidua reflexa is still pushed before the advancing ovum as in old times.

We are sorry to be obliged to speak in these terms of Dr. Meigs's sins of omission. He takes up many other novelties with such delight and earnestness, that we are sure that he cannot be unwilling to receive information or correction; and we should therefore recommend him to work up his physiology a little better from the later general treatises in which these discoveries are embodied. We shall now quit him for the present, that we may devote ourselves without hinderance to the examination of Dr. Tyler Smith's production; in doing which we shall take leave to follow the order which best suits our purpose, not that which the author has adopted. This will lead us in the first instance to inquire into the muscular structure and nervous endowments of the uterus.

Muscular structure of the uterus.—It is not long since the muscularity of the uterus was a subject of grave discussion; the affirmative being maintained by some eminent authorities, and the negative by others. The microscope has now settled the question, by showing that the uterus is made up of fibres which belong to the non-striated kind of muscular structure, corresponding in this respect to the muscular coat of the alimentary canal. It has been affirmed that, towards the completion of the term of gestation, fibres of a faintly striped character, resembling those of the heart, are to be seen in the uterus; but in the unimpregnated state, the fibres bear a close resemblance to those of less elevated development.

Nervous supply of the uterus.—Notwithstanding the difference of opinion which exists in regard to amount of the nervous matter of the uterus, all are agreed, we believe, that its nerves are *immediately* derived altogether from the sympathetic system. Dr. Tyler Smith, indeed, speaks of the uterus as "principally supplied with nerves, by the hypogastric and sacral nerves, and by branches from the spermatic plexus;" but this is a very loose method of stating the fact. The nerves of the uterus are all traceable —as Dr. Smith subsequently points out—either (according to Dr. Lee's views) into ganglia of its own, which form part of the sympathetic system, or into the ganglia and plexuses of that system from which the other pelvic viscera are supplied. No anatomist, so far as we are aware, has traced any branches of the sacral nerves into the uterus itself; although it is unquestionable that branches of several nerves of the cerebro-spinal system pass into the plexuses of the sympathetic, and are in that way distributed to the organ. It may be said that the distinction is unimportant; but we cannot regard it as being so; for it is obvious that the endowments of the cerebro-spinal fibres which enter other parts of the sympathetic system are greatly affected by their association with it, neither sensory impressions nor motor impulses being as readily conveyed by these fibres as they are by the ordinary cerebro-spinal nerves. So struck were the older physiologists with this fact, that (as is well known) they assigned to the ganglia the office of "cutting off sensation."

We are not about to offer any decided opinion upon the controversy which has arisen respecting the validity of Dr. R. Lee's claims to the discovery of additional nerves and ganglia in the virgin uterus, and of a great increase in the nervous supply during the enlargement of the organ in gestation. These claims are strongly advocated by Dr. T. Smith, who gives the following statement of them:

> "Below the bifurcation of the aorta, the aortic plexus divides into the two hypogastric nerves. Dr. Lee describes the hypogastric nerve as forming, in its descent to the cervix uteri, the hypogastric plexus. This plexus, when it reaches the cervix, terminates in a large ganglion, which Dr. Lee has called the hypogastric ganglion. The hypogastric ganglion is, in the unimpregnated state, on the authority of the same anatomist, from half an inch to three quarters of an inch in diameter; and is made up of numerous lesser ganglia with their rami of communication. Into the outer and lower surface of the hypogastric ganglion, numerous branches

enter from the third, and sometimes from the second and fourth cervical nerves. This ganglion, thus composed and reinforced, is regarded by Dr. Lee as the centre from which each lateral half of the uterus is supplied; from the hypogastric ganglia, nerves pass in various directions to the os, cervix, body, and fundus, and are distributed extensively to the muscular structure and the internal surface of the uterus. In the course of their ramifications over and in the substance of the uterus, numerous ganglionic enlargements occur. In the virgin uterus, Dr. Lee has specially directed attention to a beautiful ganglion, in front of the hypogastric ganglion, extensively connected with the surrounding nerves, which he has called the Laurentian ganglion, in honour of Mr. Lawrence; and in the gravid organ, Dr. Lee has described numerous *sub-peritoneal ganglia and plexuses* on the anterior and posterior surfaces of the organ. These ganglia and plexuses maintain extensive communications with each other and with the hypogastric ganglion below, and the spermatic plexuses and ganglia above." (p. 63.)

According to this description it would be supposed that Dr. Lee was the first to call attention to the "hypogastric ganglion;" whereas Tiedemann and other anatomists, who had carefully dissected the inferior hypogastric plexus, had described the existence of numerous small ganglia united by connecting laminæ, some of them containing ganglionic matter, forming a *ganglionic plexus* opposite the neck of the uterus. And on Dr. Lee's own showing, such a designation is far more appropriate than one which brings this collection of small ganglia, with their rami of communication, into the category of the large simple ganglia. We are not aware that any satisfactory proof has been given that the so-called "Laurentian ganglion" is anything else than a portion of the same plexus, with minute collections of ganglionic matter possibly interspersed among its rami.

It is, however, upon the question of the augmentation of the uterine nerves and ganglia during pregnancy, that the hottest discussion has taken place. Now for ourselves, we have no hesitation in saying that we have not the slightest *a priori* objection to believe in such an enlargement, with respect at least to the ganglia and fibres of the true sympathetic system; and no one has asserted, so far as we are aware, that the filaments derived from the cerebro-spinal axis undergo any increase. We do not pretend to say what are the functions of these ganglia and nerves. We do not believe them to be independent centres of reflex action; the whole analogy of the sympathetic system is against this view. On the hypothesis of Dr. Radclyffe Hall as to the general functions of that system, their office would be to regulate, through their action on the arterial coats, the afflux of blood to the organ, in accordance with its functional activity. Whatever be their office, it certainly does seem to us anything but unreasonable to suppose that, with the enormous development of the muscular and other structures, the vast increase in the supply of blood, and the elevation of the entire functional activity of this wonderful organ, there should be a necessity for an augmentation of its nervous supply. To assert the contrary, except upon positive evidence, appears to us almost equivalent to asserting that the nerves and ganglia, whose existence is admitted by every one, are of no use whatever.—It is maintained, however, by many excellent anatomists, that Dr. Lee's sub-peritoneal nerves and ganglia are *not* nerves and ganglia at all, but are carved out of the fibrous texture of the uterus; and that no essential difference exists between the nervous supply of the gravid and that of the virgin uterus. We cannot see that any real obstacle can exist to the determination of this question, if Dr. Lee will only deign to afford the materials for its settlement: for every histologist well knows that the microscope now presents an unfailing test as to the presence or absence of nerve-fibres and ganglionic corpuscles in supposed nerves and ganglia, provided these be examined in the recent state. From this test we must say, on our own knowledge, that Dr. Lee has held back; alleging precisely the same grounds for his unwillingness, as are set forth in the following extract from Dr. Tyler Smith's discussion of the subject:

> "The opponents of Dr. Lee confidently appeal to the microscope to prove the justice of their rejection of his labours; but the fact of such an appeal would seem a prejudice against him, and a willingness to deny the fidelity of his dissections. The microscope is used to ascertain the intimate physical appearances of nervous and other tissues, but hardly, on ordinary occasions, to distinguish grossly as between nerve, muscle, and cellular tissue. It must be borne in mind that the nerves of the body generally were dissected, and held to be nerves upon other evidence than such as that furnished by the microscope, long before this valuable instrument was invented. If, in the case of the uterine nerves, we deny that the tracing of the scalpel or dissecting-needle, continuity with other nervous structures, and careful ocular examination—for the debatable structures are quite large enough to be inspected by sight and touch,—if, I say, these are not sufficient to help us to a decision in the case of the uterine nerves, neurological anatomy should begin again, and we ought to question the reality of every nerve in the body; for they hold their title to be considered nerves only on evidence which, if applied to the uterine nerves, answers directly in the affirmative." (p. 47.)

Nothing can be more unsatisfactory than such arguments. There is not a single point of view, from which their irrelevancy is not obviously discernible. Dr. Lee finds nerves and ganglia, where accomplished anatomists, accustomed to minute dissection, and carefully prosecuting similar researches, had failed to discover them. His dissections are scrutinised by many other anatomists, several of whom deny *in toto* their claim to the character of nerves. Yet every one of these would say,—if Dr. Lee will *prove* these to be nerves, by that which every histologist now recognises as the only legitimate test, we will readily accord to him all the credit he demands. Will Dr. Lee or his partisans continue to affirm the existence of these nerves and ganglia, if microscopic research proves that neither nerve-tubes nor ganglionic cells exist in them?

A case of suspected poisoning occurs. One chemist examines the contents of the stomach, and can find by the liquid tests no indications of arsenic. Another, by some variation in the mode of using the same tests, discovers what he considers sufficient evidence of the presence of the metal. Is not the court of justice, in such a case, fairly entitled to demand *all* the evidence that the case admits of, and to require that the reduction of the arsenic to the metallic state should furnish the only unmistakeable proof of its previous existence in the stomach?

Even supposing, then, that the uterine nerves were as clearly displayed by Dr. Lee as the nerves of the face, or of any other part about whose nervous supply there is no question, the very fact that others have failed and do fail to bring out the same results, is of itself a sufficient reason for requiring more evidence. But there is ample ground for the assertion that this evidence is more required in the case before us than in any other.

For, on the one hand, the nerves of the sympathetic system in general contain a much larger amount of ordinary fibrous tissue mixed up in their trunks, than do those of the cerebro-spinal system; whilst, on the other, the tissue of the uterus itself is so indistinctly muscular, so little different —either to the naked eye, or under a low magnifying power,—from ordinary fibrous textures, that there is not the same facility of isolating the supposed nerve-trunks from it, as there is in the case of ordinary nerves and muscles. When, moreover, we look at the supposed *centres* of the sympathetic system, and see how small a part of the ganglionic enlargement is frequently due to the presence of vesicular matter, the greater portion arising from the junction of nerve-trunks, and from the introduction of a considerable amount of fibrous tissue, we feel that we have a right to demand that any *new* ganglia should be submitted to the test which has been applied to most of the old ones, and that the presence of the characteristic ganglionic corpuscles should be demonstrated in them, especially when a doubt exists as to their connexion with the nervous systèm at all. Dr. Lee, we have understood, affirms that he takes his stand upon the definitions of nerves and ganglia which have been in vogue amongst anatomists for the last two thousand years, and refuses to admit that the microscopic test *can* demonstrate a fallacy in his researches. If this method be adopted by other anatomists, we may bid adieu to all progress in our science. The wisdom of our ancestors is to be our only guide. Of that which *their* eyes saw and their hands touched, we are forbidden to learn anything more. Everything must remain exactly as they left it, except when the extension of our knowledge is obtained by their own methods.

It must be specially remembered in the present case, that the term "ganglion" has acquired of late years a definite *physiological* signification, very different from its original sense, which was purely *anatomical.* It formerly implied, as its name imports, merely a knot-like enlargement upon a nervous cord; and of the uses of such enlargements no definite idea could be entertained, when it was believed that the brain was the sole source of nervous power in the body. *Now,* on the other hand, we mean by a ganglion a collection of vesicular matter, which serves as a centre of nervous power to certain fibres connected with it; such a ganglionic centre may exist without any obvious external enlargement; and enlargements may exist on nervous cords without the presence of ganglionic corpuscles in their interior. Hence when Dr. Lee justifies his statement by appeals to the usage of the older anatomists, he entirely leaves out of view the complete change which, by the tacit consent of the whole scientific world, has taken place in the acceptation of the term ganglion. His assertions only go to the extent of proving the existence of knot-like enlargements upon the nervous cords,—supposing these last *to be* really nerves; the physiologist cannot accept the statement, as proving the development of new centres of nervous power during the enlargement of the uterus, until it shall have been shown that these enlargements contain vesicular neurine. If Dr. Lee had accepted the offer of experienced histologists, to test his assertions by the microscopic examination of portions of his supposed nerves and ganglia dissected from a recent uterus, the question would not have remained in the uncertainty which still clings around it. No such examination of preparations preserved in spirit can be at all satisfactory; since the "gelatinous" nerve-fibres, when hardened by alcohol, lose their distinctive characters to such a degree as to be nearly if not quite unrecognisable.

Our own opinion, then, is decidedly in favour of the *à priori* probability of Dr. Lee's assertions as to the increase of the nervous structures of the uterus during pregnancy; whilst, on the other hand, we feel ourselves called upon to refuse our assent to the doctrine, until Dr. Lee shall have furnished the evidence in its support, which is so decidedly called for, in our opinion, by the peculiarities of the case.

A considerable stress is laid by Dr. Tyler Smith on Dr. R. Lee's alleged discoveries, as bearing out his own views. We do not see, however, that the two have much connexion, since the novelty of Dr. T. Smith's doctrines chiefly consists in the stress he lays on the *reflex* actions of the uterus and other parts of the genital system, which, as he says over and over again, are dependent upon the nervous connexions of the organs with the spinal cord, the proper ganglia of the uterus (if such exist) not taking part in them. Hence we shall discuss the questions which he raises, as to the movements of the uterus, on their own merits alone.

Movements of the uterus. The chief novelty which we find under this head is in regard to the relative importance of the "reflex" and the "peristaltic or immediate" actions of the uterus. The latter are thus described by our author:

> "All muscles contract when subjected to immediate irritation, after they have been cut off from the influence of the cerebral and spinal centres, and when they have been as far as possible deprived of their nerves. In the case of muscles supplied by cerebral or spinal nerves, the contraction is limited to the spot irritated, and ceases with the removal of the irritation; but, in organs partly or wholly supplied by ganglionic nerves, as the heart, bladder, intestines, œsophagus, &c., the motion produced is of a peristaltic kind, spreading, generally in a vermicular manner, to a distance from the point of irritation, and continuing for some time after the irritation has ceased. The uterus is eminently endowed with this form of contraction. When one point of the uterus is stimulated through the abdominal parietes, the contraction excited extends with the utmost rapidity to the whole organ; the same occurs when the fingers are made to irritate any point of the internal surface of the uterus. Harvey beautifully described this peristaltic form of action in the uterus of the doe; William Hunter saw it in the cat and rabbit; Professor Müller in the uterus of the rat and the oviduct of the turtle; I have seen the same thing in the uterus of the guinea-pig and other animals. *This form of motor action is the basis of the other uterine actions.* In natural labour in the human female, it is, so to say, disguised by the reflex actions; but, under certain circumstances, it is able to effect, unaided, the expulsion of the child." (p. 39.)

All this is perfectly correct; and the sentence which we have italicised most accurately expresses a truth, some of whose applications we consider that Dr. T. Smith has overlooked, in his zeal to make the most of the reflex actions of the uterus. In fact, the sentence which concludes the foregoing extract, contains, in our apprehension, an essential fallacy. The peristaltic contractions of the uterus in ordinary labour are in no respect "disguised" by the reflex actions; for the whole character of the uterine action is as completely peristaltic in the spontaneous "pains" of labour, as it is when it responds to mechanical stimulation; and, though we are far from denying the participation of the nervous influence radiating from the spinal cord in exciting this contraction, yet we believe that we shall be able to show that its operation is very different from that which is exerted upon the muscles usually styled "voluntary," and consisting of striated fibre.

That the spontaneous contractions of the uterus may, of themselves, effect the expulsion of the child, is proved by instances in which delivery has taken place by this means alone; as in cases of paraplegia from disease of the lower part of the spinal cord, and of post-mortem parturition. Yet, as to the former, Dr. T. Smith remarks, that "patients in this state, and animals reduced to this condition by experiment, have been delivered by the stimulus of galvanism applied to the uterus itself;" and then goes on to say, that, "when cut off from the spinal centre, the uterus *requires* the stimulus of galvanism or some external irritation to excite it to contraction." He does not refer to any other experiments than those of Brachet in support of this last statement; and the testimony of that rather doubtful authority is as follows. In his first four experiments, the spinal cord was divided high up in the dorsal region, at the commencement of labour; the uterine contractions were nearly or entirely suspended, and the animals died undelivered. In the fifth experiment, labour having been fully established, the spinal cord was divided at the last dorsal vertebra, and all contraction was at once suspended; upon the application of a voltaic current, however, the uterine action recommenced, and three young ones were brought into the world; but the animal died, after five hours, with five young ones still in its womb. In the sixth experiment, after a poor guinea-pig had been in labour for an hour, the spinal cord in the sacral region was entirely destroyed, and severe flesh wounds were inflicted over the vertebral column; yet the uterine contractions continued, and five young were delivered within two hours.—Now it is perfectly obvious to us, that in the former of these cases, supposing the results to have been as described by M. Brachet, the suspension of the uterine action was due rather to the *shock* of the operation, than to any other cause. In fact, the severance of the spinal cord would by no means cut off the uterus from reflex influence, although it would prevent the agency of volition or emotion. This point does not seem to have occurred to Dr. T. Smith. The influence of the shock produced by severe injuries of the cerebro-spinal nervous system, upon organs whose habitual actions are independent of it, is abundantly manifest in the case of the heart; and every accoucheur is well aware how liable is the action of the uterus to suspension from remote causes of this nature. The speedy death of the animals which had sustained this injury, is an indication of the severity of its general effect upon their systems. The influence of galvanism, in the one case in which it was applied by M. Brachet, seems to have been exerted simply in *recalling* the uterine action, which the shock had suspended; the animal died notwithstanding. This, so far as we are aware, is the only case of the kind on record, in which galvanism has been employed; and it by no means bears out Dr. T. Smith's assertion. In the cases of uterine inertia in the human parturient female, in which it has been applied by Dr. Simpson and others, we believe that its results have been by no means satisfactory.—The sixth of M. Brachet's experiments shows that the uterine contractions are independent of the *sacral* portion of the cord; but this is obviously not sufficient to prove their complete independence of the cerebro-spinal axis.—To these he adds a case of paraplegia, apparently occasioned by disease of the lower part of the spinal cord, which paralysed everything below the pubes, so that coitus took place without any sensation of pleasure. After the usual term of gestation, however, labour commenced, but in a very imperfect manner; the uterine contractions were extremely slight, but they were sufficient to dilate the os uteri and to expel the liquor amnii when the membranes were ruptured; and at last the delivery was effected by the forceps. It is evident that, in this case, the contractility of the uterus itself was very imperfect; for it could not be called into existence by the usual stimuli applied to itself; this might be owing to that imperfection in the nutrition and muscular development of the uterus, which we usually see in paralysed parts. Here, we suspect, galvanism would have been of no use whatever.

The most complete proof of the independent contractility of the uterus is afforded by the occasional occurrence of post-mortem parturition. On this point Dr. T. Smith observes:

> "A slow reflex action of the uterus may possibly continue long after the rhythmic respiratory actions have ceased, as long, indeed, as the body retains its warmth. But we know that the heart, œsophagus, and intestines may be excited to peristaltic action after death; violent peristaltic action is often a part of the act of dying; and I am of opinion that, in the human uterus, it is peristaltic action chiefly which expels the child when the mother has died during labour, undelivered. There is, however, another source of post-mortem muscular contraction, which I believe has never been referred to the case of the uterus,—this is, the post-mortem muscular spasm. We know that, in the case of an analogous organ,—the heart,—the *rigor mortis* occurs to such an extent as to empty the ventricles of blood, and even to simulate concentric hypertrophy............The *rigor mortis*, therefore, ought to be considered as another cause of post-mortem delivery." (p. 40.)

Now, in the foregoing extract, three distinct causes are assigned for the phenomenon in question. The first is purely hypothetical. We have no reason whatever to believe in the possibility, much less in the actual existence, of "a slow reflex action of the uterus," continuing after somatic death. For the two others, we have obvious evidence derived from analogy; and we are glad to find that Dr. T. Smith so distinctly recognises the resemblance of the action of the uterus to that of the heart and alimentary canal, which, as is well known, may persist when these organs are altogether separated from their connexions with the cerebro-spinal system. We have ourselves been accustomed to refer the occurrence of post-mortem parturition to the peristaltic action of the uterus; but we have little doubt that Dr. T. Smith is perfectly correct as to the third cause, which was for the first time assigned by himself,—the post-mortem rigidity being far more remarkable in the muscular walls of the visceral apparatus, which are least subordinated to nervous influence, than it is in the muscular system of animal life.

We do not find any unwillingness, then, on Dr. T. Smith's part, to admit the fact that the uterus may contract powerfully on its contents, so as even to expel them, in virtue of its own inherent endowments, when the requisite stimulus has been given; and he even states that this form of motor action is the basis of the other uterine actions. But he seems to consider that this peristaltic movement must be excited by a stimulus *ab extra*, in order that parturition may be properly induced; and for this stimulus he looks to the influence of the spinal cord, called into reflex action by some remoter excitant. The amount of this influence, and its *modus operandi*, next require our attention; and we shall in the first place cite Dr. T. Smith's account of *reflex* uterine action:

> "The reflex actions of the uterus are very numerous; and it is upon these, and the numerous extra-uterine reflex actions excited during the process, that the natural performance of parturition essentially depends. Contraction of the uterus from irritation of the mammæ, as in the act of suckling the child; contraction of this organ from the cold-water douche, applied to the vulva or the abdominal surface; contraction excited by irritating the rectum, as by stimulating enemata; or of the stomach, by drinking a gulp of cold water; of the ovaria, by the presence of the menstrual nisus; of the vagina, by manual irritation, as in "taking a pain;" of the os uteri by irritation, as in the introduction of the hand into the uterus,—are all to be considered as so many instances of reflex spinal action. Thus, in parturition, the uterus may be excited, in a reflex form, by irritation of the mammary incident excitor nerves; the pubic and abdominal branches of the intercostals; the rectal; the gastric division of the pneumogastric; the ovarian nerves; and also by the nerves of the vagina, and the os and cervix uteri." (p. 35.)

All this is perfectly true: the facts are familiar to obstetricians; and the rationale of them will at once occur to every one acquainted with the physiology of reflex action. If they are not so explained in recent obstetrical works, it has been because too many of the so-called practical men think it quite unnecessary to keep pace with the progress of science. That the facts in question *had* been recognised and explained in the very same manner, before Dr. T. Smith first directed public attention to them, will be obvious to the reader of the following extract from a review of Dr. M. Hall's 'New Memoir on the true Spinal Marrow,' contained in the 'Brit. and For. Med. Review,' for Jan. 1844 (p. 179). After quoting Dr. Hall's anticipations of improvement in the obstetric art, from the application of reflex physiology to that department of medicine, the reviewer continues:

> "We rather wonder that Dr. Hall should not have adverted to the operation of several of the means employed by the accoucheur to bring about the contraction of the uterus in cases of hemorrhage after parturition, as examples of this kind of action. Every one knows the efficacy of friction upon the abdominal surface; where this fails, the sudden contact of the hand, previously cooled by immersion in cold water, will often produce the effect; if this should not succeed, the uterine contraction may often be brought about by the sudden immersion of the patient's hands in cold water; and the continuance of the hemorrhage requires the still more violent remedy of a dash of cold water over the surface of the trunk. Or the application of the child to the nipple has been said to produce uterine contraction, when other mild remedies have been unsuccessfully tried. It can scarcely be necessary to point out, that reflex action is evidently the channel of all these operations."

Illustrations of reflex uterine action may further be drawn, as Dr. T. Smith very justly remarks, from the phenomena of abortion, which frequently exhibit the influence of remote irritation, acting through the spinal system of nerves, in a way that can scarcely be mistaken. A due appreciation of these facts is of the greatest importance in guiding our endeavours for the prevention of abortion.

Fully coinciding with Dr. Smith in the foregoing particulars, we yet feel ourselves unable to discern that they have any direct bearing on the *normal* physiology of parturition. That the uterus is susceptible of being excited to contraction by reflected stimulation, no more proves that its ordinary actions are dependent on the spinal cord, than the influence of remote excitants upon the actions of the heart and alimentary canal proves that these actions are essentially reflex in their nature. In fact, it does not appear to us at all difficult to show that the influence of the nervous system on the uterus is precisely of the same character with the influence of any stimulus directly applied to that organ, and is very different from the ordinary forms of reflex action as seen in the muscular system of animal life. In the latter case, irritation of a motor nerve supplying a muscle excites immediate and vigorous contractions in every part of it at once; whilst direct irritation of any part of the muscle will produce a contraction limited to that part. In the former, it is very difficult to obtain any evidence by experiment as to the influence of the nerves in producing muscular contraction, so slight, in comparison, is the power they exert; and observation of the contractions which do follow the reflex irritations already alluded to, shows that, instead of being consentaneous and powerful through the whole organ at once, they are equally *peristaltic* with those which originate in direct irritation. Thus it appears that, when uterine contraction is excited through the nervous system, it is not by a new and powerful agency, taking the place of the feebler powers of the organ itself, but by the application of a new stimulus, which causes *remote* irritation to have the same effect as one of a direct or immediate kind; so that the peristaltic contraction of the uterus is really its *only* mode of expelling its contents.

It is by no means impossible that, although he does not say as much, Dr. T. Smith may be ready to admit the correctness of our view; his main object being, as we have already seen, to establish the dependence of the *commencement* of the parturient action upon a reflex stimulation derived from the ovaria, rather than to show that the uterine contractions, when once established, derive their force from nervous power. To this question we shall return when we have considered the other muscular movements concerned in parturition. The following is Dr. T. Smith's general account of them:

> "But besides the uterine reflex actions, there is, in the expiratory actions supervening in the course of labour, from the irritation of the presenting part of the fœtus, another remarkable series of reflex parturient actions, extra-uterine in their seat, but which combine and harmonize in a remarkable degree with the reflex actions proper to the uterus. It is found that tumours in the vagina, or the introduction of the hand of the accoucheur, may produce the same result as the pressure of the presenting part of the child in labour. It has been noticed of tumours, which produce little or no motor disturbance while they remain in the cavity of the uterus, that no sooner do they pass from the uterus into the vagina, than 'bearing pains,' as they are termed, are produced. In other words, the action of the expiratory muscles is excited in a reflex form by irritation of the vaginal mucous surface." (p. 36.)

In the subsequent more detailed account of the phenomena of parturition (pp. 153 et seq.), we find Dr. T. Smith maintaining still more decidedly, that the extra-uterine reflex actions are not called into play until after the stage of dilatation is completed, and the fœtal head is beginning to press upon the vaginal walls. We cannot but regard this as an error, being very sure that the expiratory effort is often involuntarily connected with the uterine "pains," when as yet the os uteri is only partially dilated, and nothing but the bag of membranes is protruded through it. In his general doctrine, however, that the remote actions are excited by the pressure of the ovum against the several parts of the genital canal, in the course of its expulsion, we heartily coincide; and it seems to us, that the germ of this view is distinctly contained in the following passage, cited from the first edition of Dr. Carpenter's 'Human Physiology' by Dr. T. Smith, in order to show the entire ignorance which prevailed as to the phenomena of parturition before his own researches began to throw light upon it:

> "It seems by no means unlikely that the uterus, though not itself dependent upon the spinal cord for its power of contraction, may contain numerous afferent or excitor fibres, and that these, being compressed by the efforts of its own muscular structure, may propagate to the spinal cord the stimulus necessary for the consentaneous action of the assistant muscles. Those who may watch a labour with an attentive consideration of its phenomena, will find that the 'pains' usually commence in the uterus itself, and that it is only when they become decided, that the power of other muscles is called into operation." (p. 25.)

We really do not see that Dr. T. Smith has advanced our knowledge of the matter in any great degree beyond this, save in the more distinct statement, that the excitor stimulus to the remoter actions is probably given by the pressure of the presenting part against the portions of the genital canal which it successively encounters: and in limiting their first excitation to the entrance of the head into the vagina, we believe him to be decidedly in error.

We now, however, approach the topic which involves what Dr. T. Smith regards as his great discovery; and this topic we shall introduce with the question,—*What is the primary stimulus to the uterine actions in parturition?* In order to present Dr. T. Smith's views on this subject in their full force, we must follow him in his survey of the phenomena of menstruation, gestation, and abortion.

Menstruation. Dr. T. Smith adopts to its full extent "the modern theory of menstruation, which makes it dependent on the periodical excitement of the ovaria, and the escape of mature ovules from the Graafian vesicles; the secretion of the catamenial fluid being secondary to the ovarian excitement;" and regards as fully proved "the fundamental analogy between menstruation in the human female, the œstruation of mammalia, and the oviposition of oviparous animals." (p. 77.) In the greater part of these expressions we entirely concur; but, as we have recently observed in another connexion (Vol. III, p. 141), we cannot admit their complete accuracy. No one, we believe, has made a larger series of observations on this subject than Dr. Ritchie, of Glasgow; and he distinctly states, as the results of his inquiries,—1. That even during the period of childhood, long before the appearance of the catamenia, there is a continual rupture of ovisacs and discharge of ova at the surface of the ovarium. 2. That although, after the establishment of the catamenial flow, the discharge of ova takes place *most frequently* at the menstrual period, the two occurrences are not necessarily coexistent; for menstruation may take place without any such rupture; whilst on the other hand, the maturation and discharge of mature ova may take place in the intervals of menstruation. These *facts* are alluded to by Dr. T. Smith, with a strange confusion of terms, as "aberrations of the ovular *theory* of menstruation (p. 82); and he thus expresses himself in regard to them :

> "The conclusion I should draw from Dr. Ritchie's papers is, that in certain cases the ovarian action of menstruation is incomplete; that though it excites the catamenia, it is insufficient to cast off an ovule. The ovules shed in the intervals between the catamenial periods, without any uterine discharge, are probably either immature, like those of childhood, or they may escape from the ovaria without a sufficient degree of ovarian excitement to set up the catamenia, being in this respect the antithesis of those other cases in which there is uterine secretion without the extrusion of ova from the ovarium. In perfect menstruation, both ovarian excitement, and the escape of an ovule concur; but the ovarian excitement is absolutely indispensable." (p. 82.)

From the statements quoted by Dr. T. Smith, with laudable impartiality, in his appendix, we gather that he finds himself obliged to allow a somewhat greater force to these exceptional cases. He quotes from a letter addressed to him by his *magnus Apollo*, Dr. Power, which is confirmatory of the doubt which we formerly expressed. "I am not satisfied," says Dr. Power, "that the bursting of a Graafian vesicle and escape of the ovule of Baer is the general occurrence or law, and am inclined to believe that the ovule, when impregnation does not take place, having lost its vitality, is not unfrequently absorbed within the ovarium." And he cites the cases recorded by Dr. Ashwell, which go to prove that the menstrual flow may take place with regularity for some years, and yet no signs of the rupture of Graafian vesicles or of the escape of ovules may be discernible on the surface of the ovarium. At the same time, he cites Dr. Girdwood's explanation of the case of the Jewish women referred to by us (loc. cit.); which is to the effect that conception in them generally takes place immediately *before* the catamenial period,—which we think by no means unlikely.* Our own inquiries on this subject lead us unhesitatingly to subscribe to the doctrine that conception is far more apt to take place immediately before or after the catamenial period, than during the interval; but, on the other hand, we have obtained distinct evidence that impregnation *may* occur at a period midway between two periods. We cannot, therefore, admit the validity of Dr. T. Smith's conclusion, that, in the exceptional cases in which ova are discharged independently of the menstrual flux, these ova are always imperfectly developed. Still less can we admit that these exceptional cases are to be passed by, in drawing from the phenomena in question any general conclusions which are to serve as a foundation for other doctrines. It is Dr. T. Smith's object to show that the uterine excitement is dependent upon the ovarian; the catamenial secretion from the uterus being the result of a sympathetic action propagated from the ovaries. To us, on the other hand, it appears that the two phenomena are in some degree independent, though closely linked together, as are other phenomena having a common purpose. The ovarian excitement tends to the maturation and discharge of ova; the uterine, to the formation of a decidua. The former may never be perfected, and yet the latter may completely take place. On the other hand, the former may take place effectually, and yet the latter may not occur in any degree consentaneously. Again, the phenomena of suspended and of vicarious menstruation appear to us to indicate that the periodicity is not simply ovarian but constitutional; the periodical loss of a certain part of the contents of the sanguiferous system being required, for the maintenance of the healthy condition of the system at large. Fully admitting that in a large number of cases amenorrhœa is rather the result than the cause of the general disorder, we yet think it impossible to contemplate the results of the suspension of normal menstruation by some local cause, without coming to the conclusion that, whether the menstrual flux consist of pure blood, or of certain of its constituents more or less altered, its removal from the sanguineous current is required to keep the latter in its due condition. This we can understand, even if these constituents should be no others than those adapted to become organized into a decidual membrane; since, as Mr. Paget (following Treviranus) has pointed out, the constituents of any organized tissue stand to the remainder of the body in the condition of materials destined for excretion.

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* We are surprised to find among those who adopt the modern doctrine of œstruation and menstruation, so little recognition of the possibility of conception occurring from a coitus anterior to the catamenial period. Dr. Meigs does not once allude to it. Among many of the lower animals, it is well known that a single coitus will fertilize a long succession of ova, of which the latest could not have been near maturity when the spermatic fluid was received by the female; and there is no reason why that fluid should not retain its vitality for a few days, as well in the passages of the female, as in those of the male genital apparatus. We are satisfied that the occurrence is by no means unfrequent in the human female; the catamenia being sometimes interrupted altogether, sometimes diminished in amount and duration, but sometimes recurring nearly as usual.

We are disposed, then, to regard the uterine excitement as by no means so entirely dependent upon the ovarian as our author would have us believe. Although usually consentaneous, either *may* occur independently of the other; and there is no constant correspondence between the degree in which the two organs respectively are affected. Here, as elsewhere, we see the necessity of attending to the *exceptional* cases. To put them aside as mere "aberrations of a theory," is obviously unphilosophical. No theory can be complete which does not include the unusual as well as the ordinary phenomena; and it frequently happens that the greatest amount of information is obtained respecting the conditions of the latter, from the careful observation of the former.—In his zeal for the prerogative functions of the ovaria, Dr. T. Smith goes so far as to assert his conviction that the greater part of the pain in dysmenorrhœa consists in ovarialgia; "the deep lumbar pain being decidedly ovarian and not uterine." Dr. T. Smith's experience of this disorder must, we apprehend, be rather limited, or his observations must be very much swayed by his preconceived hypothesis; for many a woman can assure him that her monthly pains bear a close resemblance to those of the first stage of labour; and he will not, we presume, assert that *these* are entirely ovarian. "Uterine disturbance must be considered as a secondary condition—an aggravated symptom of ovarian excitement in painful menstruation." He is "decidedly opposed to the view that dysmenorrhœa is *caused*, in the majority of cases, by chronic inflammation of the os and cervix uteri. The relation between dysmenorrhœa and inflammation of the os uteri is generally one of coincidence, not of causation, nay, it is often a symptom induced by ovarian irritation, a symptom requiring palliation, but the relief of which by no means constitutes the whole of our treatment." We would not go so far as to assert that the cause of dysmenorrhœa is to be found in the inflamed or strictured condition of the os uteri "in a majority of cases;" but that in a large number of instances, the violent pains and spasms attending the return of the catamenial period are directly consequent upon the morbid condition or malformation of the uterus, is proved by the immediate relief which the judicious treatment of these conditions affords. If ovarian disorder in such cases participate at all in the general distress, it must therefore be as a secondary consequence of the undue uterine excitement.

Here, as in numerous other cases, Dr. T. Smith mistakes his own convictions for facts, and reasons upon them accordingly. For example, at p. 52 he tells us that the Fallopian tubes have a peristaltic movement, and that, during œstruation in the lower animals, and menstruation in women, they apply themselves firmly to the ovaria. Not content with this undeniable fact, he goes on to speculate (p. 53) that during coition there "may be an action of the Fallopian tubes similar in kind to that affecting the ejaculator muscles, and the bulb of the urethra. At all events," he continues, "it is extremely probably that during coitus contractions of the tubes upon the ovaria take place." Now in what this strong probability consists, we must own ourselves to be at a loss to discover. The close application of the trumpet-shaped mouths of the Fallopian tubes to the surface of the ovary, and the peristaltic movements of the tube itself, have evident reference to the reception of the ovum at the time of its escape from the Graafian vesicle, and to its transmission to the uterus. The occurrence of such phenomena at any other period than that at which ova are being matured preparatory to their escape, would be altogether meaningless; and the supposed analogy of the contractions of the ejaculator muscles and the bulb of the urethra is no analogy at all, because these contractions form the very essence of the coitus. Notwithstanding the entirely gratuitous character of the hypothesis, Dr. T. Smith goes on to argue that this movement of the tubes must be reflex in its character, dependent upon ovarian excitement; whereas, if he had ever looked at the parts in a rabbit in heat, he must be aware that they are all so turgid with blood, as to be in a state legitimately comparable to erection, and that the movements are of that peristaltic kind which, as we have before urged, is independent of nervous stimulation. Nevertheless, this supposed action of the tubes in coitus is repeatedly referred to by our author, as if it were all but an established fact, and as affording an additional example of reflex action excited by ovarian stimulation.

Parturition. We pass over, for the present, Dr. T. Smith's hypothesis of the cause of the abdominal movements felt by pregnant women; as the discussion of it would lead us too far away from our inquiry into the validity of his doctrine regarding the origin of the parturient actions. The following extract concisely states the views which he has been led to entertain upon this subject, and which are believed by him to constitute an important advance in the physiology of parturition:

> "I believe that at the time of parturition in mammalia, the uterus and the uterine nervous system are excited by the ovaria; that it is the ovarian excitement which induces both the permanent contraction of the uterus immediately before the coming on of labour, and the tendency to those reflex, emotional, and peristaltic actions, by which parturition is completed. In menstruation, a small synergic and reflex arc is described between the ovaria and the Fallopian tubes: in parturition, a larger arc is in operation, extending from the ovaria to the uterus. According to my researches, the excitement of the uterine nervous system at parturition, upon

> the presence of which the due performance of this function depends, is caused by ovarian excitement. At the time of ordinary menstruation, the ovarian irritation which excites the contraction and rigidity of the Fallopian tubes is manifest. Throughout utero-gestation, the ovarian excitement returns in a slight degree at each periodic date; but at the eleventh period after conception (reckoning the last catamenial period inclusively) the ovarian excitement returns in full force, and, as a consequence, the uterine excitability and the uterine actions of labour begin." (p. 111.)

According to Dr. T. Smith, therefore, the muscular excitability of the uterus at the full term is dependent upon the ovarian excitement; and the occurrence of its contractions on the 280th day (or thereabouts) after conception is a reflex phenomenon, for which the exciting cause is to be found in the eleventh periodical access of ovarian excitement. We quote the following passage from the Appendix, in order to show how much of this doctrine is regarded by Dr. T. Smith as his own, and how far he considers himself to have been anticipated in it by those obstetricians, who have held similar views with himself as to the coincidence between the ordinary duration of pregnancy and ten menstrual periods:

> "The following extract from Dr. Fleetwood Churchill's work on the 'Theory and Practice of Midwifery,' shows how perfectly the idea of the regular duration of pregnancy, and the relation of parturition to the catamenial periodicity, may exist, without any idea of the cause of labour. Dr. Churchill is perfectly conversant with 'the fact,' that the average duration of labour is 280 days, or a time equalling ten catamenial periods; and he suspects that labour comes on at the tenth period after impregnation; but the 'exciting cause' is quite undiscovered. It is the knowledge of an excitor spinal nerve in the ovarium which is the one great point wanting, and without which the mere fact of periodicity is confused and of little value. I insist upon this point strongly, because attempts have been already made by those who confound *cause* with mere *coincidence* and *time,* to set aside my claim to whatever merit may lie in the detection of the cause of labour. The passage I quote proves that it is quite possible to have a tolerably accurate notion of the 'utero-ovarian periodicity' of pregnancy, and yet to consider that 'all search has hitherto failed in discovering the exciting cause of labour.' Obstetricians might have gone on, as they had done before, from generation to generation, repeating to themselves the fact, that the duration of gestation is a multiple of the catamenial period, without any conception of the ovario-uterine nervous arc, which excites the uterus to parturient action." (p. 392.)

If we recall to the minds of our readers the expression we have previously cited from the preface, that "no one had perceived that the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration," we think that our readers will have a distinct notion of the claim which Dr. T. Smith puts forward as an original discoverer. Granting for a moment that his doctrines are correct, and that the evidence which he has adduced in their support is adequate to establish them, we are as ready as he can desire to accord to him the credit to which he considers himself entitled; since we fully recognise the distinction which he draws in the preceding extract. But, unfortunately, we can neither admit that he *has* produced sufficient evidence in support of his doctrines to entitle them to be received as physiological verities, even if no opposing facts could be adduced to render them otherwise than probable; nor can we think that Dr. T. Smith has taken sufficiently into account the facts which run counter to his views. We are far from asserting that he has *wilfully* shut his eyes to these; but we trace here, as elsewhere, the indication of a want of that comprehensiveness of survey, which is an essential part of the truly philosophic character.

We shall now go into this question in some detail; endeavouring to give a fair summary of the evidence actually adduced by Dr. T. Smith in support of his views, whilst we point out the deficiencies in his proof, and the objections to his doctrine. Let him supply the deficiencies, and remove our objections, and we will most readily concede to him all the credit he demands.

In the first place, it might be supposed, from the language used by Dr. T. Smith, that he had *experimentally demonstrated* "the ovario-uterine nervous arc, which excites the uterus to parturient action;"—that he had furnished some kind of visible evidence that "the relation of the ovarian nerves to parturition is the same as the relation of the pneumogastric nerves to respiration." Physiologists have a right to expect such evidence. They do not believe that the pneumogastric nerve is an excitor of the respiratory actions, on the strength of Dr. Any-body's "conceptions" or "convictions," or on the ground of inferences founded on its anatomical distribution and connexions; but because it has been shown, over and over again, that irritation of the pulmonic branches of the vagus excites an inspiratory movement through the nervous centres, and that section of the trunks of that nerve is constantly followed by a great diminution in the rate of the respiratory actions. So, again, in the case of the act of deglutition, we rely upon experimental demonstration for the conviction that the glosso-pharyngeal nerve is the chief excitor, and the pharyngeal branches of the par vagum the chief motor nerve, concerned in the process. Now Dr. Tyler Smith has not added one particle, either by new anatomical researches, or by experimental demonstration, to our knowledge of the "ovario-uterine nervous arc." He has not proved that any more direct communication exists between the ovaries, the uterus, and the spinal cord, than that which is established through the spinal fibres intermingled with the sympathetic; whose endowments differ so much from those of ordinary spinal nerves, that any arguments founded upon the analogy of the latter are unsafe in their application to the former. He has not shown that irritation of the uterine nerves will at any time excite parturient actions in the uterus. He has not shown that section of these nerves prevents the access of the parturient actions at the usual period. Still less has he shown that irritation of the *ovarian* nerves will excite uterine contractions by reflex agency; or that section of the ovarian nerves, by interrupting the continuity of the "ovario-uterine nervous arc," will prevent the access of the parturient efforts of the uterus at the regular period. All this he must demonstrate experimentally, before he can be admitted to have established his position, that the ovarian nerves stand in the same relation to the act of parturition as do the pneumogastric to that of respiration. Not one particle of evidence on this subject is adduced by him, except that afforded by the experiments of MM. Brachet and Segalas, of which we have already disposed. Before leaving this part of the inquiry, we must remark that Dr. T. Smith seeks to establish a relation even more intimate than that to which he refers for illustration; for every physiologist knows that there are many excitors to reflex action in addition to the pneumogastrics; so that section of the latter diminishes the frequency of the respiratory movements, but does not suspend them altogether;

whereas Dr. T. Smith refers so exclusively to the ovarian nerves as the excitors of parturition, that this action would seem, in his view, to be entirely dependent upon the stimulus which they supply.

The evidence upon which Dr. T. Smith relies, is to our minds of far too vague and unsatisfactory a nature to be admitted as *establishing* a principle of such importance, even if there were no opposing considerations of any weight. It consists of little else than a reiteration of the facts and arguments he has already adduced respecting the coincidence between uterine action and ovarian excitement; together with other facts, the relevancy of which we cannot admit. "Very early in the present inquiry," he says, "I saw that the cause of labour in the human female must also be the cause of all the parturient phenomena of the animal kingdom; and this set me to observe and deduce from the parturient actions of the lower animals, in order to explain those of the human subject." Now very nearly all these illustrations are derived from the phenomena of *ovi-position,* between which and *parturition* there is no real analogy. We cannot but feel surprised that so acute an observer as Dr. T. Smith should not have perceived that what he terms the "parturition" of oviparous animals is analogous to the transit of the ovum through the Fallopian tubes into the uterus of mammalia; and that the whole of the subsequent phenomena peculiar to the latter class must be considered as belonging to a distinct category. Dr. T. Smith speaks of the well-known phenomena attending the reproduction of frogs as *proving* "that the activity of the ovaria and testes produces a long-continued and involuntary contraction of certain muscles; and further, that they increase the excitability of the nervi-motor apparatus devoted to the function of reproduction." (p. 110.) How easy it is to find proofs, when such *coincidences* as these are regarded as sufficient! At a certain time of the year, the testes of the male frog and the ovaria of the female undergo a great increase of development, and attain a condition of functional activity. At the same period, certain parts of the nervous system exhibit a peculiar excitability, closely resembling that of tetanus; and this excitability, called into action by the mechanical contact of any object with the general surface, but particularly with the tubercles then developed upon the thumbs of the male, is evidently destined to maintain the prolonged sexual congress of these animals. The two phenomena are *coincident;* but what proof have we of the dependence of either of them upon the other? A physiologist who regards the nutritive actions as resulting from the agency of the nervous system, might quote this as an illustration of the activity of nutrition in a certain organ, consequent upon the excitement of the nervous centres connected with it, with just as much propriety as Dr. T. Smith quotes it in support of his doctrine of the dependence of the nervous excitability upon the development of the sexual apparatus. *Both* phenomena appear to us as likely to be the conjoint results of that which must be admitted, on either hypothesis, to be the cause of *one* of them,—namely, the seasonal change. Dr. T. Smith says that the motor phenomena "would not appear at all if the ovaria and testes were wanting;" what proof has he of this? Granting, however, that he is right upon all he affirms on this point, we again ask, what has all this to do with the phenomena of *parturition* in the human female, save in the way of remote analogy? This excitability of the nervous system has relation to the sexual congress, not to the oviposition; the latter seems anything but a reflex act, judging by the gradual way in which the ova are voided during several weeks; and even if it were, the uterine contractions of mammalia, by which the fœtus is detached from its placental connexion with the parent, and expelled alive, has nothing in common with the deposition of an ovum which has previously escaped from the ovarium, and which is lodged as a foreign body in the oviduct.

The whole of the evidence adduced by Dr. T. Smith from the comparative physiology of the mammalia, which alone, we maintain, can be admitted as bearing upon the question, is contained in the following paragraph:

> "In many of the lower mammalia, we may witness the processes of oviposition or œstruation, parturition, congress, and conception, all going on as nearly as possible at the same time. In the guinea-pig, for instance, immediately that the young are dropped, the female admits the male, conception takes place, and a new utero-gestation commences, dating from the very hour of parturition. There is, in these animals, œstruation and ovulation going on in the ovaria, while parturition is taking place in the uterus; and during or immediately after the expulsion of the young, the sexual heat developes itself. The same phenomena are present in all the mammalia, in a greater or less degree. In those animals of which we know the order of the œstrual periods, as the rabbit, the horse, and the cow, the duration of pregnancy is a multiple of the œstrual period. Doubtless this law is as extensive as periodical œstruation itself. Not only is gestation a multiple of the œstrual period, but the time of parturition is positively an œstrual period. The maturation of ova, which has ceased during utero-gestation, is resumed, and the sexual instinct is predominant, just as though the uterus did not contain the product of a former ovulation and conception. The presence of heat or œstruation, in parturient animals, did not escape the observation of Harvey, though the fact has never hitherto been utilised in physiology." (p. 115.)

Granting, for the moment, the accuracy of Dr. T. Smith's facts, we must again point out that they amount to nothing else than *coincidences*. There is not the shadow of a proof that the œstrum, in the cases cited, is the *cause* of the parturient movements. There is nothing to disprove the assertion, if any one chose to make it, that the activity of the ovaria is dependent upon the accomplishment of parturition, as a consequence of the determination of blood to those organs,—the explanation usually given of the excitement of mammary action after parturition. And if it be affirmed that the relation of the periods of œstruation and parturition is altogether non-essential, we do not see what evidence to the contrary is afforded by the cases cited by Dr. T. Smith. Further, it is to be observed that all these facts are drawn from domesticated or semi-domesticated mammalia; in which, as is well known, the excitement of the sexual apparatus loses much of its natural periodicity. How Dr. T. Smith can take upon him to assert, with as much coolness as if nobody even thought of questioning the fact, that "the same phenomena are present in all the mammalia, in a greater or less degree," we are utterly at a loss to understand. He does not refer to a single authority in support of his assertion; and whilst we are so ignorant of the history of the reproductive function in the greater part of the *feræ naturæ*, as not even to know with certainty whether the period of gestation in the wolf is or is not the same with that of the dog, it must be obvious that unless he has become possessed, by original observation, of a vast accession to our knowledge on this subject, he is altogether unjustified in the assertion we have cited.

Again, with regard to the "œstrual periods of the rabbit, the horse, and cow," of which the duration of pregnancy in these animals is said by him to be a multiple, we must ask whether the existence of these œstrual periods has been satisfactorily established, and by whom? Whatever may be the case with these animals in the state of domesticity,—in which, as in the common fowl, an ample supply of food, with warmth and shelter, tend to produce an increased frequency in the returns of ovarian excitement, as in the functional activity of the male apparatus,—we have no satisfactory evidence, so far as we are aware, that the same holds good of these or similar animals in a state of nature. In fact there is every probability to the contrary. In the cow, for example, the average duration of gestation is about that of the human female; but the period of heat or œstruation, both in the male and female, is only annual. What coincidence is there here? If it be said that there is a monthly œstrum in the female of wild cattle, we have only to reply that we shall not believe this until it be proved; and that all probability is against it, there being no corresponding œstrum in the male. But even were such a coincidence shown to exist, it would no more be sufficient to establish a relation of cause and effect between œstruation and parturition, than the *coincidence* between the periods of heat in the male and female of the same species entitles us to assert that the œstrum of either is *dependent* upon that of the other.

We have next to examine into the evidence adduced by our author from the phenomena of human parturition. Here, again, we shall cite his own words, to avoid the possibility of misrepresenting him:

> "The duration of human pregnancy is well known to be a multiple of a catamenial period. It has also been observed that, in rare cases, where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case, and will prove of very great importance in cases of supposed criminality in wedlock, or in establishing the legitimacy of posthumous children. On the other hand, it is well known that when utero-gestation is brought to a premature termination, it is at what would have been a menstrual period that abortion usually takes place. There is, in fact, in all women, a tendency to abortion at the times represented by the catamenial periods. In placenta prævia, the hemorrhage occurring in the later months of pregnancy generally happens at the periodic dates. Hemorrhage sometimes occurs for two or three periods in succession before parturition in these cases, with perfect immunity in the intervals. The occurrenee of uterine pains at the termination of the natural period of gestation, in cases of extra-uterine fœtation, points very decidedly to the operation of some extra-uterine source of excitement. After abortion,—and very speedily, too, in some cases,—it is known that there is a remarkable aptitude to conceive; and during the catamenial climacteric, when the ovaria are irregularly excited, there is frequently an increased tendency both to conception and abortion. These accumulated facts are sufficient to establish the influence of the ovaria and of the ovarian function on the uterine phenomena of gestation and parturition. But to complete the evidence, we must inquire what proofs there are that the sexual excitement which attends exaltation of the ovarian irritability in the lower animals, is present in the human subject. An attentive observer will not fail to perceive the existence of this excitement, though in a rudimentary degree. I have insisted in a former lecture, that there is an actual increase of the sexual emotion during, or immediately after, the catamenial periods. There are also distinct traces of sexual excitement in some cases of parturition. That they are not always present does not tell against my

excited to contraction by reflected stimulation, no more proves that its ordinary actions are dependent on the spinal cord, than the influence of remote excitants upon the actions of the heart and alimentary canal proves that these actions are essentially reflex in their nature. In fact, it does not appear to us at all difficult to show that the influence of the nervous system on the uterus is precisely of the same character with the influence of any stimulus directly applied to that organ, and is very different from the ordinary forms of reflex action as seen in the muscular system of animal life. In the latter case, irritation of a motor nerve supplying a muscle excites immediate and vigorous contractions in every part of it at once; whilst direct irritation of any part of the muscle will produce a contraction limited to that part. In the former, it is very difficult to obtain any evidence by experiment as to the influence of the nerves in producing muscular contraction, so slight, in comparison, is the power they exert; and observation of the contractions which do follow the reflex irritations already alluded to, shows that, instead of being consentaneous and powerful through the whole organ at once, they are equally *peristaltic* with those which originate in direct irritation. Thus it appears that, when uterine contraction is excited through the nervous system, it is not by a new and powerful agency, taking the place of the feebler powers of the organ itself, but by the application of a new stimulus, which causes *remote* irritation to have the same effect as one of a direct or immediate kind; so that the peristaltic contraction of the uterus is really its *only* mode of expelling its contents.

It is by no means impossible that, although he does not say as much, Dr. T. Smith may be ready to admit the correctness of our view; his main object being, as we have already seen, to establish the dependence of the *commencement* of the parturient action upon a reflex stimulation derived from the ovaria, rather than to show that the uterine contractions, when once established, derive their force from nervous power. To this question we shall return when we have considered the other muscular movements concerned in parturition. The following is Dr. T. Smith's general account of them:

> "But besides the uterine reflex actions, there is, in the expiratory actions supervening in the course of labour, from the irritation of the presenting part of the fœtus, another remarkable series of reflex parturient actions, extra-uterine in their seat, but which combine and harmonize in a remarkable degree with the reflex actions proper to the uterus. It is found that tumours in the vagina, or the introduction of the hand of the accoucheur, may produce the same result as the pressure of the presenting part of the child in labour. It has been noticed of tumours, which produce little or no motor disturbance while they remain in the cavity of the uterus, that no sooner do they pass from the uterus into the vagina, than 'bearing pains,' as they are termed, are produced. In other words, the action of the expiratory muscles is excited in a reflex form by irritation of the vaginal mucous surface." (p. 36.)

In the subsequent more detailed account of the phenomena of parturition (pp. 153 et seq.), we find Dr. T. Smith maintaining still more decidedly, that the extra-uterine reflex actions are not called into play until after the stage of dilatation is completed, and the fœtal head is beginning to press upon the vaginal walls. We cannot but regard this as an error, being very sure that the expiratory effort is often involuntarily connected with the uterine "pains," when as yet the os uteri is only partially dilated, and nothing but the bag of membranes is protruded through it. In his general doctrine, however, that the remote actions are excited by the pressure of the ovum against the several parts of the genital canal, in the course of its expulsion, we heartily coincide; and it seems to us, that the germ of this view is distinctly contained in the following passage, cited from the first edition of Dr. Carpenter's 'Human Physiology' by Dr. T. Smith, in order to show the entire ignorance which prevailed as to the phenomena of parturition before his own researches began to throw light upon it:

> "It seems by no means unlikely that the uterus, though not itself dependent upon the spinal cord for its power of contraction, may contain numerous afferent or excitor fibres, and that these, being compressed by the efforts of its own muscular structure, may propagate to the spinal cord the stimulus necessary for the consentaneous action of the assistant muscles. Those who may watch a labour with an attentive consideration of its phenomena, will find that the 'pains' usually commence in the uterus itself, and that it is only when they become decided, that the power of other muscles is called into operation." (p. 25.)

We really do not see that Dr. T. Smith has advanced our knowledge of the matter in any great degree beyond this, save in the more distinct statement, that the excitor stimulus to the remoter actions is probably given by the pressure of the presenting part against the portions of the genital canal which it successively encounters: and in limiting their first excitation to the entrance of the head into the vagina, we believe him to be decidedly in error.

We now, however, approach the topic which involves what Dr. T. Smith regards as his great discovery; and this topic we shall introduce with the question,—*What is the primary stimulus to the uterine actions in parturition?* In order to present Dr. T. Smith's views on this subject in their full force, we must follow him in his survey of the phenomena of menstruation, gestation, and abortion.

Menstruation. Dr. T. Smith adopts to its full extent "the modern theory of menstruation, which makes it dependent on the periodical excitement of the ovaria, and the escape of mature ovules from the Graafian vesicles; the secretion of the catamenial fluid being secondary to the ovarian excitement;" and regards as fully proved "the fundamental analogy between menstruation in the human female, the œstruation of mammalia, and the oviposition of oviparous animals." (p. 77.) In the greater part of these expressions we entirely concur; but, as we have recently observed in another connexion (Vol. III, p. 141), we cannot admit their complete accuracy. No one, we believe, has made a larger series of observations on this subject than Dr. Ritchie, of Glasgow; and he distinctly states, as the results of his inquiries,—1. That even during the period of childhood, long before the appearance of the catamenia, there is a continual rupture of ovisacs and discharge of ova at the surface of the ovarium. 2. That although, after the establishment of the catamenial flow, the discharge of ova takes place *most frequently* at the menstrual period, the two occurrences are not necessarily coexistent; for menstruation may take place without any such rupture; whilst on the other hand, the maturation and discharge of mature ova may take place in the intervals of menstruation. These *facts* are alluded to by Dr. T. Smith, with a inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case." We could scarcely cite a stronger instance than this of Dr. T. Smith's rashness of statement, and disregard of all facts but such as tend to confirm his own hypotheses. It must be remembered that in the Gardner peerage case, the object of those who sought to establish the legitimacy of the offspring was to show that pregnancy might be protracted to 311 days, or 31 days beyond the usual term; and in the medical testimony that was adduced to prove the possibility of such an extension of the usual term, any minor degrees of prolongation would of course be less attended to. Yet it will be obvious, we are sure, to those who will go through the evidence given on that case, that there is nothing even there to justify the assertion, that pregnancy, if protracted at all beyond the tenth catamenial period, extends to the eleventh. In the table published by Dr. Merriman, who was one of the witnesses upon that occasion, it is expressly stated that whilst 33 women were delivered in the 40th week, 22 were delivered in the 41st, 15 in the 42d, 10 in the 43d, and only 4 in the 44th. Yet, according to Dr. T. Smith's assertion, all the women who passed the 40th week ought to have retained their ova until the 44th. Nature, however, refuses to tie herself down to such rules. Dr. Murphy, in his 'Report of the Obstetric Practice of University College for 1844,' gives a similar table, in which it is stated that whilst 24 women were delivered in the 39th week, 25 in the 40th, and 32 in the 41st, 25 were delivered in the 42d week, 19 in the 43d, 9 in the 44th, and 11 in the 45th. We believe that all these cases are open to the objection that the date of conception was not known, and that impregnation was presumed to have occurred immediately *after* the last recurrence of the catamenia; whereas it might have taken place just *before* the next œstruation, which appears to be the case (see Dr. T. Smith's Appendix, p. 386) with those Jewish females who observe the Rabbinical law of abstinence from sexual intercourse for five days beyond the period laid down by Moses. This would cut off about three weeks from the presumed period of gestation, and would probably tend to reduce a large proportion of the cases of supposed protraction to the regular term. But it will not do so in by any means all cases; and moreover we have an amount of trustworthy evidence, derived from cases in which the day of conception could be fixed, and in which there was no motive for falsification, sufficient to establish the possibility of protraction beyond the usual term, for *any number* of days or weeks *less* than the additional month; how much *more* may be allowed, can scarcely be considered as yet decided.

The evidence of comparative gestation is all to the same effect; and we cannot refrain from asking why Dr. T. Smith has kept it back, when he is evidently acquainted with at least that part of it supplied by Tessier, and himself contends (p. 125) that "a comparison between human and comparative parturition, and all the other forms of parturient action, is essentially and absolutely necessary to a comprehension of this branch of human physiology." If it be admitted that we have a right to reason from the one case to the other, it is obvious that the class of phenomena in question affords most satisfactory evidence in regard to variability in the term of gestation; since the date of conception can be more positively determined than is usually possible in the case of the human female. In addition to the observations of Tessier, we have now a long series carried on upon the cow, under the direction of the late Earl Spencer; from which it appears that, the average term of gestation in that animal being 284 or 285 days, an excess of a few days is not at all uncommon; whilst a prolongation to the extent of a fortnight or three weeks is much more common than an extension to a month. We shall presently speak of one very curious part of these observations; from which it appears that the *male* parent exerts an influence over the term of gestation, a phenomenon altogether inconsistent with Dr. T. Smith's hypothesis.

Having now tested the value of Dr. T. Smith's hypothesis by applying it to three different classes of facts which are altogether unnoticed by him, and having found it deficient even in the attribute of probability, still more in any claim to be received as a demonstrated truth, we might leave the question where we found it, and say, as many have said before us, that no account whatever could be given of the ordinary limitation of the term of gestation to 280 days. Even if no valid explanation can be given, however, it seems to us that it will be useful to know in what direction we are to seek for one; at any rate, it cannot but be advantageous to point out certain facts which any theory on the subject ought to embrace.

In the first place, then, it must be admitted on all hands that there is a certain disposition or tendency, on the part of the uterus, to disburthen itself of its contents at the termination of the fortieth week of gestation, rather than at any other time. Even if we grant, with Dr. T. Smith, that at each period of œstruation during pregnancy there is a tendency to abortion, we have still a right to ask why, in such a vast majority of cases, the fœtus is retained until the tenth of these periods has elapsed, and is then ejected? Why should not labour take place just as readily at the *tenth* as at the *eleventh* œstrum; and why should it be so very seldom deferred until the *twelfth?* We cannot see that Dr. T. Smith can give any other answer to these questions, than that the uterus and its contents are then in such a condition, as to render the ovarian stimulus far more effectual at that time than at any other. In assigning the ovarian stimulus, then, as *the cause* of parturition, Dr. T. Smith has altogether left out of view the fact that this cannot operate alone; and that unless there be, coincidently with this, a certain peculiar readiness on the part of the uterus to part with its contents, labour would no more come on at the end of the tenth monthly period than at the close of the ninth. As he has not shown whence this arises, we must consider him as having advanced but little in the explanation of the causes of parturition, even if we concede to him all that he demands. The act of parturition was likened by Buffon to the dropping of ripe fruit; and we believe, that in seeking for its cause partly (to say the least) in that condition of the uterus and its contents which may be designated as *maturation*, we are justified by all the facts at present known to us. The phenomena of the separation of the offspring from the parent, as a consequence of the completion of certain stages of growth on the part of the former, the loosening of the connexion between them being a part of this sequence of changes, are seen very extensively through the organized world; and they are obviously analogous to the phenomena of separation and detachment, which occur in the parts of an integral structure during its evolution. No one has any difficulty in understanding that the fall of the leaf is due to the death of the tissue which connects it with the stem; or that the opening of a flower is the result of the turgescence of its tissue, consequent upon its full development. Every one knows that there are flowers whose first opening takes place with great regularity at a particular hour; we have ourselves repeatedly witnessed this in a West Indian species of Amaryllis, the long floral leaves of which became detached from each other, and sprung apart, within a few minutes before or after six o'clock in the evening. Although many vegetable and animal periodicities may be shown to be under the influence of external agencies to a certain extent, yet there is ample proof that they are essentially independent of such agencies, and that they form parts of that cycle of phenomena, of which the whole life of the being, from its origin to its final decay, is made up. We do not see anything more wonderful in the recurrence of certain changes at particular times, than in the progressive evolution of the entire organism. The exuviation of the milk-teeth, for example, in a certain regular succession, and at such uniform times, as to afford one of the best tests of the age of the individual, is a phenomenon which strongly marks the disposition in the human system to the spontaneous severance of a vital connexion which has served its purpose, and to the detachment of the part in which it has ceased to exist.

We have adequate reason to believe that the embryo of warm-blooded animals is extremely regular as to its term of development. Thus, in the common fowl, we may calculate almost to an hour (provided that the eggs have not been detained in the oviduct of the hen) the time when the chick will emerge from the shell. And it is well known that the stages of progressive development in the human fœtus afford indications from which its age can be determined, with a pretty close approximation to truth. That the development of the uterine structure goes on *pari passu* with that of the contained fœtus, appears equally evident from a consideration of its phenomena; especially those attending the formation of the placenta, in which organ there is such a marvellous commingling of parts, formed from two distinct and independent centres of development. Now we see pretty clear indications that the placenta of the fully-developed fœtus is somewhat in the condition of the footstalk of a ripening fruit; that is, that having attained its full evolution as an organ of temporary function, its connexion has a tendency to become dissevered, in virtue of causes inherent in itself alone, and quite independent of external agency. Something of this kind seems to have been recognised by Dr. T. Smith himself, who expresses himself, in his chapter on abortion, to the following effect:

> "The placenta has a tendency to become unfit for fœtal respiration towards the end of utero-gestation, when Nature is preparing for the change from branchial to pulmonary breathing. There is frequently observed on the surface of the mature placenta crystals of carbonate of lime, which must tend to interfere with its functions as a respiratory apparatus, and generally, I believe, to facilitate its separation from the uterus. This caducous preparation of the placenta, by the deposition of the salt of lime, is probably connected with the demand for ossific matter in the fœtus; but it must also remind you of the deposit of lime upon the egg of the bird, or of silica in the stems of ripe fruit, to facilitate its separation from the parent tree; or we may compare it to the deposit of earthy salts in the lungs of old age, as preparatory to the death of the individual. It is pretty certain that, in some cases of abortion in the latter months, caused by the death of the fœtus, the death has depended on the low respiring power of the placenta, the placental

development having progressed so rapidly as to render the organ prematurely deciduous." (p. 145.)

Another indication of the necessity for this maturation is to be found in the difficulty with which uterine contractions are excited by the ergot of rye before the normal time for parturition has arrived. Perhaps the strongest proof, however, that the parturient contraction of the uterus is in some way dependent upon the condition of the placenta, is derived from the phenomena which have been described under the designation of *superfœtation*. In whatever way we explain these phenomena,—whether we regard the two fœtuses as the products of two distinct conceptions,—or whether we regard them as twins, of which one is more advanced in its development than the other,—the facts remain the same; namely, that one child may be expelled by the parturient efforts of the uterus, whilst the other is detained; that the uterine contractions do not detach the placenta of the latter; but that pregnancy may continue for many weeks or even months longer, until the second child has attained its full term of development. In some of these cases, it is positively affirmed that the first child presented every appearance of maturity; in others, it appears that an abnormal condition of the placenta was the cause of its detachment. In all, however, it is obvious that the retention of the second fœtus was consequent upon *its* immaturity; and we do not see how this could influence the uterine contractions, except through the condition of the placenta.

There is another most important indication, that the immediate cause of the supervention of labour is to be looked for in the condition of the attachment of the fœtus to its parent, rather than in ovarian excitement; namely, that preparatory changes are obviously taking place during the last fortnight, or thereabouts, of gestation. The uterus, as Dr. T. Smith himself tells us, begins to contract more closely upon its contents; and the disposition to relaxation in the soft parts surrounding the outlet of the pelvis manifests itself in the descent of the whole mass. We have no doubt that, during this period, a progressive change is taking place in the placental attachment; for such a change may be easily verified in the placenta of many of the lower animals (such as the cat), in which the fœtal and maternal portions remain more distinct than they do in the human female, these being separable from each other, as the period of parturition draws near, far more easily than at any previous time. And it is not difficult to understand that the completion of this change, involving (so to speak) the death of the entire placenta, shall be the immediate cause of the supervention of labour; just as we know that premature labour may result from the death or abnormal condition of the fœtus and of the fœtal portion of the placenta. This view affords us a rational explanation of the occurrence of uterine action in cases of extra-uterine fœtation, which is considered by Dr. T. Smith as clearly indicating that the exciting cause of labour is to be found elsewhere than in the uterus itself. For if the condition of the placental attachment furnish that cause, instead of the state of the ovary, it will do so equally, whether the placenta be attached to the lining of the uterus, or to that of the Fallopian tube, or to any other organ.—As an additional indication that the exciting cause of labour has its place in the fœtal, rather than in the ovarian, connexions of the uterus, we may notice the fact, ascertained by Earl Spencer, that of 75 cows in calf by a particular bull, the average period was 288½ days; none of these having gone less than 281 days, and two fifths of them having exceeded 289 days. That the male parent could produce any modifying influence on the ovarian periodicities of the female, cannot be thought probable for a moment; but that he should influence the term of fœtal maturation, must be admitted to be not unlikely.

We have thus fully discussed the chief theoretical questions raised by Dr. T. Smith, in regard to the cause of parturition; because we consider that his claims are put forward in a manner to attract much attention amongst those who are disposed to accept specious novelties without sufficient investigation into their merits; and because we deem it our duty to subject all such claims to a rigorous and searching examination. We would again disclaim any wish to disparage Dr. T. Smith's merits. He has displayed great ingenuity and acuteness in the manner in which he has built up his hypothesis, and strengthened it with so many props and buttresses, that its inherent weakness may pass unnoticed. And when we come to discuss the practical portion of the work, we shall find that this same ingenuity has led him to originate many useful suggestions, for which he will deserve the gratitude of obstetricians. But in the character of a philosophic physiologist, of which he is obviously ambitious, we are obliged to regard him as having altogether failed, through a want of that comprehensiveness of survey of phenomena, and of logical appreciation of their entire relations, without which no discovery can be established, however ingenious may be the conceptions put forth, and however probable the doctrines advanced.—In order to avoid the possibility of misconception, we shall briefly recapitulate our positions, in such a form as to indicate what we believe to be the true view of the subject, and to point out the chief errors into which we consider that Dr. T. Smith has fallen.

1. The muscular action of the uterus we regard as essentially peristaltic and independent of nervous stimulation. We fully recognise, however, the fact that the induction of this action is *often* brought about by nervous agency; but we consider this agency as operating, not directly upon the entire muscular structure of the organ (as when a voluntary muscle is thrown into contraction), but indirectly, by exciting its peristaltic movements, as in the case of other organs composed like it of non-striated fibre, and receiving, like it, their nervous supply from the sympathetic system. We altogether dissent from Dr. T. Smith's doctrine, that the parturient contractions of the uterus, when once established, and even its tonic contraction anterior to labour, are excito-motor; because no adequate proof has been given that they are dependent upon nervous agency at all; and because all analogy, as well as the evidence of experiment and pathological observation (so far as they extend), go to prove that they are not. Nevertheless, we would not deny that the parturient actions *may be* excited in the first instance by nervous agency; it being very obvious, from the phenomena of abortion, that causes originating at a distance, and not capable of acting in any other way than through the nervous system, may bring about these actions. That the uterus may be thus affected through

the nervous system, has long been taught by physiologists and obstetricians; and Dr. T. Smith's chief movement, in supposed advance, is in referring the regular parturient actions of the uterus to this source.

2. Dr. T. Smith maintains that the exciting cause of parturition is to be found in the recurrence of the periodical excitement of the ovary; which continues during the entire period of gestation, giving a special tendency to abortion at each return; and which, at the eleventh recurrence, acts with such potency as to induce parturition. He assigns no other cause, however, why this eleventh recurrence should be so much more effectual than the rest, than that by this time there is a much greater aptitude to contraction in the uterus itself, and a readiness to be thrown off on the part of the placenta. That this ovarian excitement, acting by reflexion on the uterus through the spinal system of nerves,—the *ovarian* nerves being the *excitors*, and the *uterine* the *motors*,—is the exciting cause of normal parturition, is, we believe, an entirely original doctrine of Dr. T. Smith's; no others having advanced beyond the mention of the usual *coincidence* of the period of parturition with the eleventh ovarian nisus.—From this doctrine, however, we feel called upon to withhold our assent; because, in the first place, no adequate proof has been afforded by Dr. T. Smith, that the coincidence of the two phenomena is in any way essential; and because, secondly, a number of phenomena, altogether left out of view by him, afford a clear indication to our minds that the coincidence is *not* essential, the normal parturient actions of the uterus being entirely independent of the ovarian nisus. We do not deny that abortion may sometimes result from ovarian excitement; but we conceive that this has no special action, and that it is only one out of many causes that may produce the result where a predisposition to it exists.

3. In searching for the real exciting causes of parturition, we conceive that there is ample evidence, both from the analogy of similar phenomena in the lower mammalia, and from observation on the human subject, to justify the expectation that it will be found in the condition of the placental attachment; and we can see no reason why the changes in which maturation consists, and on which, according to Dr. T. Smith, the peculiar efficiency of the eleventh ovarian nisus depends, should not, when complete, be themselves as potent in exciting the first uterine contractions, as he supposes the ovarian stimulus to be.

4. The parturient efforts of the uterus having gradually advanced the presenting part of the child into the vagina, the other expulsor muscles are called into action through the spinal system of nerves; the requisite stimulus being supplied, either by afferent nerves of the uterus itself, as first suggested by Dr. Carpenter, or by those of the walls of the parturient canal, against the several divisions of which the presenting part of the child successively presses, as maintained by Dr. T. Smith. We are inclined to regard the latter as the correct view of the source of the action; but if any merit be due to the idea of the local origin of the stimulus which excites the reflex action of the expulsors, it appears to us that it may be fairly claimed by the first-named author.

Having thus discussed the main subject of the first portion of Dr. T. Smith's treatise, we may now revert to one or two of the minor topics, of which we thought it better to postpone our notice until this should have been disposed of.—In regard to the *fœtal movements* supposed to be felt during pregnancy, our author maintains that, for the most part at least, they are in reality local spasmodic contractions of the *uterus*. In fact, all his arguments go to prove that there is no such thing as fœtal movement at all; and he does not even attempt to suggest any diagnostic marks by which the true fœtal movements may be distinguished from the uterine. That *many* of the supposed movements of the fœtus are really uterine, we quite agree with Dr. T. Smith in believing; but that the fœtus is capable of executing reflex movements within the uterus, and that many of its supposed movements are really due to itself, we are equally satisfied. There is no mistaking the form of the infant's foot, which often causes a projection of the uterus in the later months of pregnancy, and which may be distinctly felt when the abdominal parietes are thin; there is no possibility of error when we can feel this withdrawn and pushed out elsewhere, so prominently that we can even count the toes. Nor can there be any mistake when fœtal movements are excited during labour by irritation of the presenting part. On the other hand, the spasmodic twitchings of the uterus itself communicate a different sensation to the hand laid over the part; and we can scarcely imagine that any practitioner endowed with the *tactus eruditus*, and possessed of the knowledge of both sources of movement, can have much difficulty in the diagnosis when either occurs separately. We are disposed to think, however, that the two are frequently concurrent; and that either may excite the other. Nothing would be so likely to excite local contractions of the uterus, as the pressure or impulse of the limbs of the fœtus against its walls; and nothing would be so efficient in calling the reflex mobility of the fœtus into play, as the spasmodic twitchings of the portion of the uterus that might happen to press on any prominent part. We would commend to the attention of Dr. T. Smith, and of our readers, the very interesting papers on the reflex movements of the fœtus, as the cause of its position in the uterus, contributed by Professor Simpson to the recent numbers of the 'Edinburgh Monthly Journal.'

Active dilatation of the os uteri.—Great stress is laid by Dr. T. Smith upon what he terms a positive dilatation of the os uteri, over and above the mechanical dilatation of this structure by the pressure of the amniotic bag, and the tension exerted on the opening mouth of the uterus by the contractions of the rest of the organ; and upon the reflex nature of this dilatation. Other authors have spoken of a "disposition to dilatation," by which they implied (we suppose) a *passive* condition of the sphincteric fibres, which caused them to yield before the propulsive force exerted by the active contractions of those of the remainder of the uterus. But Dr. T. Smith considers that the dilatation of the os uteri is itself an *active* process, the result of an operation of that same reflex function which he has called in to the assistance of the uterus in explaining the other parturient actions. We are unable to appreciate the force of the evidence which has led him to this assumption; a certain hypothesis that other sphincters have a similar power of "active dilatation" being the chief groundwork of his argument. Now we cannot, for our own part, see the slightest proof that the cardiac sphincter, or the sphincter ani, are ever in any condition different from those common to other muscles,—namely, a state of contraction more or less powerful, and a state of relaxation more or less complete. In the former condition they tend to keep the orifice closed, even when a powerful force is in action to distend it; in the latter they are disposed to yield to the expulsor power of other muscles, still retaining contractile force enough to keep it closed when not thus distended; or if the relaxation be rendered complete and permanent by paralysis or section of the nerves, they no longer maintain the closure of the orifice. Now we do not admit that the dilatation of the os uteri is in any degree more dependent upon nervous agency than is the contraction of the fundus; and we cannot see that there is any more difficulty in accounting for its "disposition to relax" at this particular period, than there is for the "disposition to contract" on the part of the fundus; both changes being due, as it seems to us, to the very same cause, and that cause residing in the maturation of the uterine structures connected with the function of gestation. We are acquainted with only one sphincter in the body which has even the appearance of an active dilating power; this is the iris, upon the nature of whose actions (as our readers must be well aware) physiologists are not agreed. At any rate, if the dilatation of the pupil, as well as its contraction, be truly muscular, it is produced by the antagonistic action of a set of fibres passing radially across the circular fibres of the sphincter. Some have attributed the dilatation of the os uteri to the contraction of a set of fibres supposed to be implanted in the lip, and to pass in a straight line towards the fundus. Dr. Meigs favours this idea; but it is altogether repudiated by Dr. T. Smith, who does not need the assistance of any such fibres, finding all that he seeks in the "active dilatation" of the sphincter itself, excited through the nervous system.

The Genesial cycle.—We have perused with great attention the lecture in which the theory is propounded that bears the above designation; and we must own ourselves at some loss to discover what are the special *ideas* which the author desires to develope. We shall do our best to give a concise expression of them; being only deterred from doing so in the author's own words, by the length of the quotations that would be required to convey them. Dr. T. Smith in the first place brings systematically together the various "sympathies" and "synergies," physiological and pathological, which indicate a close relation between the ovaries, uterus, and mammæ. This relation was previously, we think, sufficiently appreciated both by physiologists and practitioners; the former having been in the habit of looking at these three organs as parts of the same apparatus, and as performing three distinct but mutually adapted offices in one function; the latter having the connexion forced upon their attention by phenomena of daily occurrence.

Dr. T. Smith divides the reproductive life of the human female into a series of "great genesial cycles," each of them composed of a period of ovarian, of uterine, and of mammary activity;—the ovarian commencing with puberty, continuing until conception takes place, and then giving place to the uterine; the uterine lasting for the term of gestation, and then being succeeded by the mammary; and on the termination of lactation, a fresh cycle commencing with the return of ovarian activity. Now in this it is obvious that there is but a formalization of facts which everyone knew before; and this formalization (very different from the inductive process by which a true *law* is evolved) is valuable only in so far as it leads to further results. To refer the metastasis of activity, as

Dr. T. Smith does, to the operation of the reflex function, is about as gratuitous a hypothesis as we have anywhere met with, although, as we have shown, such hypotheses are by no means unfrequent with our author: we might as well, it appears to us, refer the withering of the stamens and floral envelopes of a blossom, and the increase of the fruit, to a nervous system. Nothing is more common than this alternation of action among the different parts of the same apparatus; and where this involves the organic functions only, we do not see any difficulty in accounting for them in accordance with the general doctrines of nutrition. The succession of phenomena designated as the "great genesial cycle," is repeated, however, on a small scale, according to our author, during every catamenial period; and he thinks that he thus satisfactorily accounts for the regularity of the monthly return of ovarian excitement.

> "We know that it is the state of the ovaria at the ovarian period which produces the uterine secretion. This action continues for two, three, four, or five days or more, and then ceases. Now I believe that during the rest of the days of the catamenial cycle of twenty-eight days, the breasts and uterus are exerting an influence on the ovaria of a slower and more moderate character, which gradually prepares them for the next ovarian excitement. Thus in a case in which the catamenial secretion and mammary disturbance lasts four days, there is, so to speak, a neural discharge from the ovaria to the uterus and mammæ during four days. At the end of this time, the activity of the ovaria declines, and there is now a neural discharge set up in the contrary direction, so that for twenty-four days the mammæ and uterus are modifying and directing the actions of the ovaria. During the intervals between the catamenial flow, the uterus and mammæ are acting upon the ovaria, causing them to prepare and mature the ovule or ovules of each ovulation, just as, during the days of the catamenial flow, the ovaria are acting on the uterus, causing it to pour out the catamenial secretion—the reason of the distinct ovarian periodicity being, that this action of the uterus and mammæ upon the ovaria is prolonged for a greater number of days than the reaction of the ovaria upon these organs. According to this view, the twenty-four days is as much a periodicity as the four days of the catamenial flow. The twenty-four days are the term of the uterine and mammary periodicity, the four days are the term of the ovarian periodic excitement, and the entire catamenial period of twenty-eight days is the record of the time taken in one revolution of the reciprocal actions of the mammæ and uterus upon the ovaria, and of the alternative action of the ovaria upon the uterus and mammæ. If the catamenial flow had lasted fourteen days, and the apparent quiescence of the uterus had continued just an equal time, the cause of the periodic times of the sex would have been evident long ago; but owing to the disproportion between the uterine and ovarian times, the uterine and mammary phenomena of the intervals between the catamenial flow have never been recognised. All the facts of the case—and they might be multiplied—seem to me to point to a power of neural induction, excitation, or polarity, existing between the three principal organs of reproduction, and regulating and producing their functional revolutions or periods, just as simply and decisively as gravitation causes the motions of the heavenly bodies." (p. 181.)

At the conclusion of the lecture which contains the foregoing passage, we find Dr. T. Smith giving utterance to the following speculation, which forms an appropriate pendant to it.

> "By an extension of the same principle as that which I have been tracing between the organs of reproduction, I think we may acquire a better knowledge of intermittent fevers, perhaps of Asiatic cholera itself." (p. 188.)

We cannot think it requisite to inflict on our readers any laboured demonstration of the utter baselessness of this hypothesis, which, though at first brought forward merely as the author's belief, is afterwards spoken of as if it were the expression of observed facts. We can imagine the unsophisticated student imbibing it with all the gravity with which it is propounded; and learning with surprise, as he grows wiser, that the "neural discharge" is a pure invention of the author's, and that there is about as much ground for the belief in this "reciprocal action of the mammæ and uterus upon the ovaria," as there is in the doctrine of "lunar influence," which, we recollect, was brought forward afresh in a new and specious form in our earlier days, and was very attractive to our young imagination. The consideration does not seem to have occurred to Dr. T. Smith, that such mammary sympathies as present themselves in connexion with this monthly periodicity are *coincident with* the ovarian excitement, not reciprocal to it; many women having pains in the breasts, and some even forming a lactescent secretion, during every catamenial flow. But a theorist of his wonderful ingenuity, can have no difficulty in accounting for such apparent obstacles; in fact we shall not be in the least surprised if Dr. T. Smith should convert every one of the objections which we have advanced against his hypotheses, into triumphant demonstrations of their truth. Nothing is more easy to a sanguine speculator, mounted upon his Pegasus, than to dash through the petty obstacles which sober and cautious reasoners would build up in his course; and there is a certain fascination in thus giving oneself up to the exhilarating influence of rapid motion, without being troubled about the payment of turnpikes over which we leap, or the value of the crops we trample down, that is for a time extremely enticing. The misfortune is, that instead of making real progress, we are too often led back in our chase to the point from which we started, or are carried in the direction precisely opposite to that of our goal. And, as in the case of the hare and the tortoise, the cautious reasoner, who is content with a less rapid advance, but whose movement, however slow, is steadily progressive, generally wins the race in the end.

It has been with much regret that we have felt ourselves compelled to say so much in disparagement of Dr. T. Smith's hypotheses, and of the method of philosophising which he has thought fit to adopt. With nothing but a friendly feeling towards himself, we yet consider the interests of truth paramount; and the questions which he has raised, and the manner in which he has raised them, call for a full and free discussion. We trust that neither he nor our readers will consider that we have in any degree transgressed the bounds of candour and good feeling, in criticising his production as we have done. We again repeat that we consider that great merit is due to him for his labours, although we cannot coincide in his own estimate of the value of his results; and we may add that the reader of his treatise cannot but be favorably impressed by the author's range of acquirement, his ingenuity and fertility of thought, and his very agreeable style, even when most widely dissenting from his views.

We must postpone until our next Number the discussion of the practical portions of the two works before us.

Art. II.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXI. Second Series, Vol. XIII. —*London*, 1848. 8vo, pp. 522. With Seven Plates.

We continue, from our last Number, our analysis of this volume; the interesting nature of whose contents gives occasion to this unusually lengthened notice of them.

XVI. *Contributions to the statistics of valvular disease of the heart;* by A. Whyte Barclay, M.D., Physician to the Chelsea, Belgrave, and Brompton Dispensary, and Medical Registrar of St. George's Hospital.

The present paper neither contains, nor professes to contain, any suggestions for the improvement of diagnosis, except in as far as *a priori* probabilities may be available, nor does it hint at any novel principle for the guidance of practice. It consists of a tabulated statement of the necroscopic appearances of 79 bodies in which more or less valvular disease was discovered, and of the sex, age, and cause of death of the sufferers, preceded by some remarks which may be considered as a running commentary upon the whole. Much patient industry is required for the collection and arrangement of the materials of such a record of facts, which is calculated to throw great light upon the history and relations of the particular disease under discussion. We cannot pretend to give an adequate sketch or analysis of a memoir of this kind, but shall content ourselves with extracting some of the more striking facts and inferences that are deducible from the numerous observations here detailed.

Out of 419 cases occurring during two years at St. George's Hospital, in which the condition of the organs of circulation was ascertained, 79 detailed in the table were found to present various modifications of valvular disease: so that 18·8 per cent. is the proportion in which we may expect to meet with these lesions in persons dying of all diseases. As regards the frequency with which the different valves were affected, it appears that in 36, or 45 per cent. the aortic and mitral were so simultaneously, in 26, or 33 per cent. the aortic alone, and in 17, or 22 per cent. the mitral alone.

The age of the patient seems to have a marked influence upon the production of fibrinous deposit, since in the first 18 cases in which the age varied from 12 years to 34, it occurred 16 times, while it is only mentioned 5 times in the remaining 51, in which the age ranged from 34 to 79 years.

Atheroma, on the contrary, is more abundantly developed as life advances. Of 113 instances out of the 419, in which it was met with on the valves or at the origin of the aorta, the numbers falling in different decades of age give, as compared with the whole numbers of deaths in the corresponding decades—

12 per cent. as the rate of its occurrence between 20 and 30;
21 per cent. between 30 and 40, and also between 40 and 50;
43 per cent. between 50 and 60, and also between 60 and 70;
54 per cent. above 70.

Valvular disease is superadded in about two fifths of the cases of atheroma, but in little more than half of these is the valve found in a condition of atheromatous degeneration; and it does not appear from Dr. Barclay's researches that the mitral valves are more liable to atheromatous disease than the aorta, though a contrary statement has been made by several pathologists.

The frequency with which the aortic and mitral valves are simultaneously affected in connexion with rheumatic fever is very remarkable. Of 15 cases traced to this disease, out of the 79, there were 13 in which both valves thus suffered together; and of another series of 35 cases similarly circumstanced, there were 28 in which both were implicated. In the few remaining cases in which either valve was separately diseased, it appeared that there was no distinction between them as to the frequency of this occurrence. Hypertrophy, dilatation, and atrophy are equally found as consequences of severe lesion of the aortic and of the mitral valves, and of adherent pericardium, and sometimes would seem to exist in a considerable degree of development, with a condition of valves in no way proportionate to the muscular changes.

Deviations from the healthy and normal condition of the kidney are noticed forty-one times in the table. In 28 cases the disease was granular degeneration; in 5 the kidney was large and mottled. It is remarkable that, while of 61 cases of atrophied kidney, 29 are coincident with valvular disease, only 5 cases out of 21 of those in which the kidney was large and mottled present examples of endocardial affections; very nearly 50 per cent. in the one condition, and scarcely 25 per cent. in the other. And in comparing these two forms of disease, we find that, while contraction with thickening of the valve exists in numerous cases of granular degeneration, only one ambiguous instance is recorded as coincident with a large and mottled kidney; a result which agrees with the fact that endocarditis occurs in the former without complications, and is not found in the latter, except under such conditions as would seem to develope it where the kidneys were healthy.

> "Were I," writes the author, some pages farther on, "to allow myself to speculate on blood-poison as a cause of endocarditis, of which acute rheumatism and granular degeneration are supposed to furnish us with examples, I might inquire why it is that in the one the inflammatory action is so liable to extend to both sets of valves, while the other does not exhibit any marked deviation from the ordinary statistics which I have obtained for all forms of cardiac lesion together? whence the liability to a recurrence of endocarditis is derived, when no subsequent affection of the joints has been observed? or why, in the last stages of granular degeneration, when epilepsy or coma marks the transmission of the poison to the brain, recent inflammation is so rarely found, and evidence of previous disease is of such constant occurrence? But for the present it must suffice to point out these as inferences which may be fairly drawn from the statistics now presented." (p. 203.)

It results from the author's investigations that acute rheumatism has some, but no great, influence in developing atheroma, since he found that all the cases of diseased heart gave a proportion of 45 per cent. of atheroma in general, and 25 per cent. of atheroma of the valves; while diseased hearts, connected with rheumatic fever, give, as the corresponding proportions, 53 and 27 per cent.

We here terminate our notice of a paper which must be consulted in the original, in order to form a just estimate of its merits, and which will always be referred to by those who wish for a complete knowledge of the subjects of which it treats.

XVII. *On the anatomy and physiology of the cysticercus tenuicollis;* by C. B. Rose, F.R.C.S., F.G.S., &c.

We candidly admit that we are not prepared to treat this paper critically. We do not profess entire ignorance of the subject, but the field selected by Mr. Rose is a peculiar one, and it requires the devotion of considerable time and careful attention for the successful cultivation of such a science as helminthology. We certainly consider that the author has made a step in advance, in the account which his investigations have enabled him to give us of this interesting hydatid, about which much has been already written. We pass by the historical notice with which the original observations in the paper are introduced, and will briefly point out the chief novelties it contains, without professing to have verified them by personal examination.

After describing the structure of the vesicular, transparent body of this entozoon, which he regards as consisting of an external fibrous, and an internal granular lamina, the author proceeds to speak of its proboscis, and the corpuscles with which it is beset, and which he has satisfied himself are *not* ova, but what Mr. Gulliver has demonstrated them to be, viz. "oval bodies possessing a granular texture, with a calcareous shell." He considers their function to be protective. The circlet of hooks attached to this organ the author considers to be capable of free movement, and that their function is to act as a mechanical irritant, "wherewith to scratch the interior of the vascular cyst, for the purpose of exciting it to secrete a pabulum for the sustenance of its tenant." He regards the absorbent property of the suctorial cups to be entirely exploded, and that they in reality afford no channel to the interior of the hydatid; that, in short, they are purely suctorial. This opinion he holds in common with other naturalists of the present day. We now come to a point of, we believe, entire novelty. On opening the vesicular body of the hydatid by a crucial incision, and turning back the anterior flaps, a transparent filament is seen hanging from the interior of the vesicle where the proboscis commences, and which may be traced high up within the proboscis. The length of this organ increases with the growth and age of the hydatid, and it is very strong and elastic. The author was fortunate enough once to discover "a cluster of young ones sprouting from the upper part of the pendulous body" in question. From this he infers that it is a special reproductive organ, and denominates it accordingly the "*Gemmarium.*" Mr. Rose believes, however, that there is still much to learn with respect to the reproduction or propagation of this in common with other cystic entozoa, and certainly we are quite in the dark as to how they can be disseminated; so that, as the author remarks, the difficulty does not consist in the question of whether they are generated by ova or gemmæ, but in the mode of escape from their incarcerated position. He is inclined, however, to think that they may propagate by ova as well as gemmæ. As regards the nutrition of this parasite, Mr. Rose regards the fluid contained in the cyst as the source whence it derives its nourishment;

and considers that the thickened cyst seen surrounding this entozoon when found dead, results from the excess of irritation by its armed proboscis in its demand for food.

The lithographed illustrations which accompany this paper are well worthy of it. They are extremely well executed, and must, we apprehend, faithfully delineate the objects described by the author.

XVIII. *Description of the dissection of a brain, in which the corpus callosum, fornix, and septum lucidum were imperfectly developed;* by Mitchell Henry, House Surgeon to St. Bartholomew's Hospital. Communicated by James Paget, Esq.

The readers of the 'Medico-Chirurgical Transactions' will recollect that a brain, presenting an analogous imperfection of development to that noticed in the subject of the present paper, is described by Mr. Paget in the 29th volume. Both the physiologist and psychologist are interested in the multiplication of such instances, where a faithful and trustworthy record is given of the post-mortem appearances, associated with any mental, moral, or other functional peculiarity. That we are still far, very far, from possessing any satisfactory data for generalizing on the metaphysical functions of the great nervous centre must be readily conceded; and but little hope can we hold out to the student of the abstruse science of the connexion between mind and matter, that the present case will afford him, by parallel, or rather by contrast, with Mr. Paget's, the means of making another step in advance. In Mr. Henry's case, the ventricles were exposed above through the anterior half of their extent, the corpus callosum measuring not more than an inch and a half in length, and bifurcating anteriorly, so as to bound a fissure of a nearly quadrilateral form, in which were seen the contents of the lateral ventricles, and the interval between the optic thalami, toward the third ventricle; the fornix was, therefore, likewise wanting at the same part, and the septum lucidum entirely absent. Our space will not allow us to give a more detailed description of the course and arrangement of the fibres composing the imperfectly developed structures; but we find room first to state that, as the boy from whom the specimen was taken was a foundling, an opportunity was afforded of learning that he was very slow in acquiring knowledge, that he was prone to fall asleep when occupied in reading or writing, and generally bewildered when suddenly questioned. He was, however, shrewd in driving a bargain, and in looking after *number one.* In Mr. Paget's case, the mental phenomena presented a marked contrast to the above, the girl being vivacious, quick in action, &c.; whilst, as remarked, the boy was cautious and stupid. A well-executed lithograph accompanies this paper.

XIX. *Report of a case in which gastrotomy was performed for the relief of obstruction of the bowels;* by Robert Druitt, F.R.C.S.E. Communicated by Thomas Peregrine, Esq.

This case would have served well to illustrate Mr. Phillips's paper, which stands at the head of this review; and it adds another to the instances of failure which he records—not, to all appearance, because the operation was inapplicable in this instance; on the contrary, it seemed to be the only resource; the failure being chiefly attributable to delay in its performance,—that most rife cause of the fatal issue of so many operations performed for the relief of intestinal obstructions, whether in the form of ordinary hernia or otherwise.

The delicate lad who was the subject of this attack and operation was affected with inflammation of the bowels in February 1848, which yielded to the usual remedies, including injections and purgatives. In April the author was again requested to see him for an apparently similar attack; there were present abdominal pain, vomiting, rapid pulse, scanty urine, &c., and the last full evacuation of the bowels was on March 30th. The belly became gradually tense, but less tender; and the chief pain and tenderness were referred to a spot below and to the right of the navel. The patient's condition varied a little, sometimes better, at others worse; but the most prominent and important features of the case remained unchanged, and the constipation was complete. At the close of a fortnight, the consent of the boy's friends to an operation was obtained, and the abdomen was opened by an incision, four inches in extent, between the umbilicus and pubes; and the cause of obstruction was ascertained to be a dense, glistening band, passing from one fold of intestine to another, and binding down a further portion beneath it. A finger was passed under it, and it was divided. The self-gratulation of the author on the success of this step was unfortunately cut short by the outpouring of fæcal matter from the bottom of the peritoneal cavity: a large ulcerated opening was found in the ileum, near to the lower end of the divided band. The patient expired two hours and a half after the operation. An examination of the body discovered traces of bygone peritonitis, which had left behind the evidence of adhesions at various points. The whole of the large intestine was pale and contracted, as was likewise the last twenty-five inches of the ileum: the remainder of this portion of the small intestine, together with the jejunum and duodenum, were enormously distended and reddened; the stomach was also distended. The two ends of the divided band were found attached to different points of the ileum; and there were two or three other ulcerated patches near to that which had given way.

The case seems to have been treated judiciously, the author confessing his regret that the operation was not performed earlier; and that the inflammatory symptoms, masking the mechanical obstructing cause of constipation, misled him in the first instance; but the true nature of the patient's condition became apparent when these symptoms were mitigated.

XX. *Cases illustrative of some consequences of local injury;* by Thomas Hodgkin, M.D.

The author has arranged the cases here published into two groups, viz.: one in which the effects had more or less the character of common inflammation, though probably not unconnected with the operation of a morbid poison; the other distinguished by the production of an adventitious structure, having the character of malignant disease. The first of these groups contains three, the second five examples.

The subject of the first case was a gentleman forty-five years of age, an active and energetic man of business, of spare habit, who, though not robust, had seldom suffered from bad health. It was discovered, but not till after his death, that rather more than two months previously, he had fallen down upon the edge of the kerb-stone in London, but was not sensible of having received much injury, nor was he detained from his business. About six or seven weeks after this, when heated and fatigued, he was exposed, ill-protected, to wet and cold, on the outside of a coach. In the evening he looked chilly and distressed, felt cold, and had a severe pain at a spot in the right side, where the effects of the injury were afterwards found. Nevertheless, he went next day to town, and was again exposed to some wet and cold. The following day he was confined to the house, but after this he went through arduous mental exertions. A blister over the seat of pain, aperients, and effervescing salines afforded him considerable relief, and he made no more complaint of the pain in the side, but continued in a febrile state, marked with great debility, yet he took animal food. Two half glasses of claret were followed by great excitement, and some wandering of reason, from which he recovered after sleep, but slighter paroxysms occurred two or three times, though in the intervals he was quite rational. We cannot trace the subsequent anomalous symptoms farther than to mention that there was throughout slight tendency to somnolence, while there was nothing very remarkable in the condition of the thoracic or abdominal viscera, but that the pulse, though regular, was inordinately frequent, rarely so low as 110, and sometimes upwards of 130. Small pustules or maturating vesicles on the body and limbs were only noticed a short time before death.

The pleura costalis corresponding to the fifth or sixth rib on the right side, from its anterior extremity to its angle, as well as that corresponding to a greater part of the intercostal space immediately above and below, was elevated by purulent deposits of various sizes. Few, if any, had perforated the pleura, but the opposed pleura pulmonalis was at this part slightly attached by adhesion, and presented several purulent deposits; while the immediately adjacent portion of the lung was in a state of recent hepatization. The muscular structure corresponding to the affected part was found to be thickly sprinkled with similar but smaller purulent deposits. Pus also was met with in a small subcutaneous tumour, near the head of the right fibula, and was probably contained in a similar but much larger tumour at the upper part of the left calf, as well as in scattered pustules on the surface of the body. Nothing worthy of much note was found within the head, but the stomach presented small, scattered, depressions near its small curvature, supposed to be enlarged orifices of follicles.

In the second case of the first group, there was severe pain occupying a small spot a little below the right knee, originating probably in some violence or strain in riding, about six weeks before. Swelling of the knee and limb shortly followed, and there was great constitutional disturbance, and death took place within five days of his complaining of the pain. Puriform matter was found within the knee-joint; and a larger collection existed around it, but without communicating with that which was within the capsule. The lungs, when cut into, were found to have a portion of a dark olive-brown colour, infiltrated with fluid of the same colour, highly lacerable, and emitting an offensive and peculiar odour, not precisely that of gangrene. The substance of the spleen was readily broken down into a lilac-coloured grumous mass.

The third case occurred in a lady, who, whilst in an early stage of pregnancy, met with a severe sprain, followed by great pain in the side for some hours, which left a foreboding of death, even while she was apparently in good health, that was not realized for three months. There were some very peculiar symptoms towards the close of this melancholy case, as emphysema of the skin about the iliac region, and an odour of the breath resembling first that of ground malt, and latterly that of caramel; otherwise there appears to us little remarkable in the origin or progress of the disorder. On examination, much mischief was found about the lower part of the abdomen, involving the appendages of the uterus and neighbouring parts.

The five cases illustrating the second group, in which malignant growths were developed, as consequences of some local injuries, may be more briefly described.

In the first case a piece of tobacco-pipe was forced into a man's head, so that two inches of it lay for four months along the side of the cavernous sinus, its presence there being unsuspected. At this part the dura mater was raised by a small mass of cerebriform matter, as large as a wild cherry. No similar matter was found elsewhere in the body.

In the second, a man injured his back while carrying a load, and paraplegia followed in the course of six months, with increased projection of the spinous processes of the lumbar vertebræ. After death one of the middle lumbar vertebræ was found crushed, and rounded masses of fungoid or cerebriform matter grew from both sides of their bodies at this part.

Case three is an example of malignant disease of the right testicle, subsequently extending into the abdomen, which followed a blow received upon the former part.

In case four, a severe blow over the loins, inflicted with a heavy poker, produced at the time severe pain, but no inconvenience, after this had subsided, for nearly three years. Sharp pains in the loins then came on; a nodulous tumour was felt through the abdomen at a later period, and the patient died exhausted, three or four months after seeking medical assistance. A nodulous, fungoid, or medullary tumour projected forward from the lumbar vertebræ below the kidneys.

Case five is that of a gentleman between 40 and 50 years of age, who, intending to sit down on a chair which he had seen behind him shortly before, but which in the interval had been removed, came with violence to the floor. Some months after, among other symptoms, there was angular projection of the spine about the middle of the dorsal vertebræ; and a resistant tumour was detected in the epigastric region. The bodies of two of the vertebræ were found nearly destroyed. The spinal cord was much encroached upon, and considerable masses of fungoid growth protruded from the spinal column, both before and behind.

It is the object of the succeeding remarks to offer some explanation of the differences occurring in these two groups of cases. And first it is observed, as a circumstance common to them both, that they originated in local injury inflicted a more or less considerable time before death, yet so obscurely emanating from it, that in some instances the connexion was only discovered on post-mortem examination.

Regarding the first group, the author writes:

"It seems to me that the immediate effect of the local injury is so to impair a portion of the animal tissue, as to render it incapable of the proper maintenance of those molecular changes by which interstitial absorption and deposition are carried on; in fact, that the part thus damaged is very much in the state in which a portion of bone exists when it is about to become necrosed from injury. During the existence of this state of things, another exciting cause may come into operation, such as the exposure to cold in the first two instances, and the exposure to cold and abortion in the third. In the febrile state, produced by these causes, the surrounding structures are no longer able to resist the morbid influence of the previously injured part, in which chemical changes have progressively been going on. So modified, it becomes a source of irritation to the parts in contact with it, and the result is common inflammation, or a derangement of a more specific kind, modified by the poisonous character of the offending cause, and by the further production of a like cause in the system." (p. 275.)

Of the remarkable odour of malt, Dr. Hodgkin observes that he is led to believe it a certain omen of death. Its occurrence three times in pregnant women at first induced him to suspect that it had some connexion with the puerperal state; but this impression was at once removed by his meeting with it, in a remarkable degree, in a lad who died of mesenteric disease. We ourselves once observed the strong and characteristic odour of caramel in the matter of an abscess bursting into the œsophagus, which we had reason to believe proceeded from the abdomen in a youth, in whose case we were unable to make an examination after death, which followed in a few days.

"The phenomena of the *second group* of cases," says the author, "appear to me to offer a striking contrast with those to which I have adverted in connexion with the first group. In the first group the vitality of the injured part appeared so reduced, that neither resolution nor process of repair took place. In the second group, on the contrary, the distinguishing phenomenon is the production of a new growth, to which vital organization is essential, although the organization proves itself to be abnormal and injurious. I believe that all parts, whether old or new, can only be nourished by, or produced from, living material, and that every living cell is produced from a previous cell. If in the first group of cases the local injury was sufficient to destroy life, to a small extent, in the second group, without going quite so far, it materially modified the nutrition of the parts by exerting an influence on very minute molecules. It is comparatively indifferent, as respects the present speculation, whether the newly-modified cells take their origin in such as may be proper to the injured tissue itself, or in those of the blood brought to it for its proper nutrition; in either case the original cells are modified by the circumstances in which they are placed.... It is by no means necessary," he afterwards adds, "that such transformations should be immediate. A succession of transformations may intervene between the normal corpuscle and the complete production of a heterologue adventitious structure." (p. 280.)

We must here terminate our sketch of a paper which does not readily admit of a brief and just analysis. The high reputation of the author as a pathologist, and the interest which attaches to the cases themselves, and to his views regarding them, must be our excuse for having extended our notice beyond the ordinary limits.

XXI. *Account of a dislocation, in consequence of disease, of the first and second cervical vertebræ;* by James Paget, Assistant-Surgeon to St. Bartholomew's Hospital, &c., and an Appendix by Alexander Shaw, Surgeon to the Middlesex Hospital.

This paper and its appendix contain two interesting cases, with illustrative woodcuts, of remarkable diminution in the calibre of the upper part of the spinal canal, consequent in each, apparently, on disease, producing a slow and gradual encroachment on the tender and important viscus contained within. The specimen described by Mr. Paget came into his possession by chance, and unaccompanied by any history of the individual to whom the bones once belonged. In both axis and dentata there was evidence of pre-existent disease, in the form of caries, which had given rise to osseous union between the two bones. There must also have been considerable disease of the ligaments, inasmuch as the axis occupies a position very much behind its normal one, appearing "as if quite loosed from its natural fastenings, and moved backwards, with a very slight deviation to the left, till its odontoid process is close to the posterior portion of the atlas." The result of this remarkable displacement is, that the ring of the atlas is divided by the odontoid process of the axis into two unequal parts, of which the anterior is by far the larger, as regards its antero-posterior diameter; the posterior division measuring only two lines from the odontoid process to the posterior arch of the atlas, and rather more on each side of the median line; its transverse diameter being nine and a half lines. Into this small space the cord and its coverings must have been compressed during life. "The bones," the author adds, "are not diminished in size, nor materially altered in form, but slender."

The preparation exhibited by Mr. Shaw when Mr. Paget's paper was read before the Society, was taken from a man who was brought into the Middlesex Hospital dead, by strangers who could give no account of the accident which caused his death. On examination, the base of the skull was found fractured, and the brain lacerated. Circumstances have thus deprived us, in each of these cases, of the opportunity of ascertaining how far the functions of the nervous system were properly performed; but there is some collateral evidence in favour of its non-impairment in Mr. Shaw's case, such as the strong marking of the processes giving attachment to the muscles (we presume the muscles themselves were well developed, but this is not stated), and the general solidity of the bones, which denotes muscular power. The occipital bone and atlas were firmly anchylosed together; there being only a slight variation in the relative position of the two bones, causing the axis of the foramen magnum to be a little to the left of that of the atlas. The alteration in the relation of the atlas to the axis is very striking. As in Mr. Paget's case, the former bone has been carried forwards on the latter, so as to diminish the canal to a transversely oval aperture at this spot: the advancement, however, is greater on the left than on the right side. The relation of the axis to the third vertebra is normal. The author remarks that, according to the most correct measurement he can make, it appears that "the central point of that part of the atlas which enters into the spinal canal is situated in a line half an inch before the axis of the spinal foramen of the dentata; and the effect of that change is seen by the posterior arch of the atlas crossing the spinal foramen of the dentata and intersecting it about equally." These bones are, of course, firmly united by anchylosis.

How much it is to be regretted that no history is attached to these cases. Certain, however, we may be that the original possessors of these curious specimens survived the various stages of disease and reparation, until the latter was complete. That the unyielding character of the connexion between the atlas and occipital bone aided in the production of the fatal fracture in the latter case, seems highly probable. Mr. Shaw adds a postscript, in which he states that he has a patient under his care, now (August 1848) in the Middlesex Hospital, in whom there is every reason to suppose that the same deformity exists as in the case he has described, the head being projected forwards and leaning obliquely to the left side. In her case there were no symptoms of paralysis.

XXII.—*On the minute anatomy of the emphysematous lung*; by George Rainey, M.R.C.S., Demonstrator of Anatomy at St. Thomas's Hospital.

The form of emphysema which furnishes the subject of the following details is that which is called vesicular emphysema, and the specimens of the disease selected for minute examination are of the ordinary kind.

The author first describes the minute anatomy of the lung in its healthy state, as far as the structure of the air-cells is concerned. These are said to differ much in size and form, but for the most part to be cuboidal. The walls of these cavities appear to be formed of a dense plexus of capillaries, consisting only, however, of one layer, so placed as to form the common boundary of the two contiguous air-cells between which it is situated, one side being turned towards the cavity of the one, the other towards that of the adjacent cell. The air-cells communicate by large circular openings, through which the air can pass from one to another. The pulmonary membrane, which is very thin and almost transparent, lines the air-cells, and in passing from one into another, incloses the plexuses of capillary vessels between the two; whence between each two contiguous air-cells, there is one layer of vessels, and two layers of membrane. This membrane appears to be the seat of disease in emphysema; for it is then seen to be perforated or cribriform. These perforations cannot be confounded with the natural openings by which the air-cells open into one another or into the intercellular passages, the latter being surrounded by a circle of anastomosing vessels.

The pulmonary membrane in the vicinity of these perforations, as well as in many parts not yet cribriform, is studded with brightish spots generally of a circular form, which vary in number and size, and might be mistaken for the nuclei observable in the coats of the capillary vessels, did they not occur in situations in which there are no such vessels, as in the areolæ of the plexuses. These spots exist either singly or in clusters, and are supposed to precede the formation of the perforations, the latter having the same form and size, and occupying the same parts of the membrane as the former. This appearance is seen to be due to the presence of oil in the tissue of the membrane, or upon its surface; as is farther proved by the fact of oil being left upon the glass when a small portion, previously dried and gently heated, is pressed between two pieces; and also by the disappearance of the spots after digestion of a dried portion in sulphuric ether. Hence it is inferred, that the materials of which the pulmonary membrane is composed are converted into oil, and that its texture is in some places weakened, in others wholly destroyed.

If those parts of an injected emphysematous lung in which the air-cells are merely dilated be examined by reflected light, the capillaries in their walls are seen to be smaller than natural, whilst the spaces circumscribed by the meshes are much increased in size, these changes being proportioned to the amount of dilatation; and seeming to indicate that the membrane, previously to its giving way, had been in a state of great extension. This extension may be due either to increased distending force, or to weakening of the membrane, or to both together. In parts where a visible lesion of the membrane between the air-cells has taken place, and several cells have become united so as to form one cavity, fragments of cell-wall are seen within it, containing extremely elongated capillaries and portions of plexuses, in which the vessels are very much contracted, and the areolæ enlarged, but mostly in length. These views of the author, as far as they relate to the presence of oil in the air-cells, and its influence in the production of emphysema, are altogether new; and should they meet with the confirmation necessary to establish their truth, he will have all the merit of originality, as well as that which we readily accord to his zeal and industry.

XXIII. *On the utility of trisnitrate of bismuth in the diarrhœa accompanying phthisis;* by Theophilus Thompson, M.D., F.R.S., Physician to the Hospital for Consumption and Diseases of the Chest.

Out of twenty-one cases in which this remedy was given for the cure of diarrhœa, it is stated by the author to have removed the symptoms entirely in fifteen, to have afforded temporary relief in four, and to have failed only in two. The medicine was administered to adults in doses of five grains, with three of gum arabic, and two of magnesia, at intervals of four or six hours, and is said to have produced marked benefit usually after the second dose.

Dr. Thompson mentions the prior use of bismuth in cholera by Dr. Leo of Warsaw, in 1831, and its successful employment by Guersent in two cases of non-febrile diarrhœa in children, published in 1833. These latter examples seem to have attracted the attention of Dr. Lombard of Geneva, whose favorable opinion recommended it to the present writer; and he made experiments which led him to the result, that in the diarrhœa of phthisis, it surpasses, both in efficacy and safety, our most approved remedies for that complaint.

We have ourselves made trial of the virtues of the medicine in a few cases of diarrhœa of various kinds, and as far as such limited experience warrants a conclusion, consider it entitled to some confidence, and tender our thanks to Dr. Thompson for having diffused the knowledge of a novel application of this substance.

XXIV. *Case of resection of the scapula;* by William Fergusson, Professor of Surgery in King's College, London.

Rare operations are always worthy of record, not as an inducement to surgeons to repeat them needlessly, but as precedents for similar practice in cases positively requiring such operative interference. We trust, however, that, now we know pretty well the extent to which the human body may be mutilated and still survive, such cautions are scarcely requisite, and that few of our brethren would desire to build a reputation on so illegitimate a basis as a bold use of the knife affords. To be prompt and dexterous in time and circumstances of need, is no unimportant characteristic of the accomplished surgeon; to seek for operations for the sake of display, is unworthy of the scientific cultivator of surgery; but to display skill, be it ever so refined, unless positively needed, is an offence of a much more grave character than the idle parade of mechanical skill.

The present is an interesting instance of extensive disease, threatening life, and curable only by a bold and rare operation. The qualities for judging of the necessity of the course pursued, and decision in executing the serious task, were evidently combined in the able surgeon who treated the case, as the issue proved. The patient had suffered from disease of the right shoulder-joint for seven years, when he had the limb removed at the articulation, in Fort Pitt Hospital. The wound never entirely healed; and when he came under the author's notice, there were many openings about the shoulder, some of which communicated with bare bone. The discharge and constant suffering were making inroads into the constitution; and a careful consideration of the case induced Mr. Fergusson to undertake the extirpation of the entire scapula, about two inches of the clavicle being removed with it. The subclavian artery was compressed over the first rib, and but little blood was lost. The patient recovered without any untoward symptom of importance; and a year after the operation, was reported to be in very good health, though occasionally troubled with small abscesses in the breast. On examination of the parts, the soft tissues connected with the scapula were found thickened and diseased, and the bone itself was hypertrophied, being at almost every part thickened and hardened. The glenoid cavity was carious, and new ossific matter had sprung up around it. A characteristic woodcut accompanies this paper.

XXV. *Case of a patient in whom a foreign body had been introduced into the pelvis through the vagina, and a description of a successful operation for its removal;* by J. C. W. Lever, M.D., Physician-accoucheur to Guy's Hospital, and John Hilton, F.R.S., Assistant-surgeon to Guy's Hospital.

The foreign body referred to in this joint production of Dr. Lever and Mr. Hilton is a bone netting mesh, which it appears that the patient, 34 years of age, was employing to apply some ointment to the vagina, when she was suddenly disturbed and sat down, by which means the mesh was forced through the vagina out of sight, and at length, by her fruitless attempts to remove it, likewise out of reach. It remained undisturbed for six months, when her continued suffering induced her to seek admission into Guy's Hospital. Her health was impaired, and pain extended along the course of the sciatic nerve, over which, as it emerges from the pelvis, there was considerable tenderness. On examination, Mr. Hilton expressed his conviction that the foreign body was "lying obliquely in the pelvis, with its lower extremity firmly fixed near the tuberosity of the right ischium, and close to the foramen ovale." Its upper extremity appeared to extend towards the sacro-iliac synchondrosis. It could be distinctly felt through the vagina or rectum. Mr. Hilton cut down on the foreign body, but found it necessary to divide it with a pair of bone forceps before he could remove it. The mesh, when entire, measured between five and six inches in length, and was a quarter of an inch broad. The patient had a smart attack of inflammation, probably involving the peritoneum as well as other tissues in the pelvis, for which she was judiciously treated; and at the end of a month was quite well. A full-length portrait of the mesh on wood embellishes the margin of the text.

XXVI. *Case of obturator hernia, simulating intestinal obstruction within the abdomen, to relieve which gastrotomy was performed;* by John Hilton, F.R.S., Assistant-surgeon to Guy's Hospital.

The present case constitutes another interesting commentary on the inferences deduced by the author of the first paper in the volume under review; for, though the case strictly pertains to the hernial class of intestinal obstructions, the nature of the symptoms, and the marked character of the actual seat and cause of stricture, lead naturally to its being placed with those forms of obstruction which are only amenable to such surgical interference as was resorted to. We consider, in short, that, from Mr. Hilton's showing, he was justified in the course he pursued, though the issue of the case was unfavorable.

It appears that the subject of the hernia, a lady, æt. 36, had an attack of vomiting, with constipation of the bowels, in September 1847, which was relieved by aperients, injections, emetics, &c.; but she had since been frequently subject to spasmodic pain in the bowels. On January 20, 1848, she was attacked with vomiting, attended again by constipation and spasm. No hernia could be discovered; but the symptoms continuing unrelieved, Mr. Hilton was requested to see her on the 31st. The urine secreted was scanty, the abdomen was not distended, and the colon was capable of receiving and retaining nearly three pints of fluid without distress. The chief pain, on pressure, was felt deeply a little below, and to the left of, the umbilicus, and the constitutional disturbance was such as to call for some decided step. The signs above enumerated, together with others given more in detail in the paper, determined Mr. Hilton to recommend gastrotomy, it being presumed that the obstruction would be found in the small intestine, and below the umbilicus. The operation was performed in a warmed room, chloroform having been previously administered to the patient. An incision was carried from the umbilicus to the pubes, and the abdomen opened. On raising the colon on the left side, some coils of pale, contracted small intestine were exposed; and on tracing them downwards, it was found that one of them had entered the obturator foramen of the left side. This was gently withdrawn, and found to be dark coloured, but not gangrenous nor ruptured. The edges of the wound were brought together by a continued suture; and the patient being in a state of collapse, was wrapped in a blanket, and had brandy administered. Two or three small vessels required ligatures, but there seems to have been but little blood lost. The patient is stated to have been under the influence of chloroform for about an hour; and when sensibility returned, the pulse was 150. She expired the same evening, there being, apparently, no attempt at reaction after the operation.

The post-mortem examination discovered "marked evidence of general inflammation of the peritoneum in the lower half of the abdominal cavity." The position and relations of the hernial sac are illustrated by a woodcut which accompanies the paper. It had passed through the upper part of the obturator muscle, the superior fibres of which constricted the fundus of the sac, so as to cause strangulation of its contents; which leads the author to remark justly, that, in such a case, the reduction of an obturator hernia would require relaxation of the muscle in question. The exhibition of metallic mercury was suggested, but not resorted to. We think with the author that this was one of those rare cases, in which it might have been effectual in dragging the intestine out of its abnormal position.

One word in conclusion. We acquiesce with Mr. Hilton in the course he pursued as regards the operation itself; but was this a case in which the employment of chloroform was advisable, nay, justifiable? We think decidedly not; and we cannot peruse the history before us without feeling a strong impression that this is one of the many cases, in which the indiscriminate use of this powerful anæsthetic has acted most prejudicially in complicating the shock of an operation, and lessening the patient's chance of recovery.

XXVII. *The history of a case of dislocation of the head of the femur backwards, with some observations on that form of dislocation,* by Richard Quain, Professor of Clinical Surgery to University College, London; Surgeon to University College Hospital.

The rare opportunities which present themselves, as Professor Quain remarks, of making dissections of recent dislocations, renders any contribution like the present valuable; and this is especially the case when, as in the present instance, the display of the actual relation of parts leads a careful observer to modify in a measure the received opinions on the subject, and to point out other practical deductions with which it is important to be acquainted.

The subject of the injury which is described by Mr. Quain in this communication, was a stout man of 60, who was killed by a fall from a ladder. Deformity in the right lower extremity led to a careful examination of the cause. There was considerable apparent, though but slight actual, shortening of the limb, which was inverted, and separated from, as well as further back than, the sound one. The head of the femur was found lying below the pyriform muscle, and immediately behind the acetabulum, over the base of the ischiatic spine: the ilium was fractured. The external obturator and quadratus muscles were torn through; as was likewise the internal part of the capsular ligament at its attachment to the cervix femoris: the round ligament was torn out of the depression on the head of the femur; and a small portion was broken off the edge of the acetabulum.

These are the leading facts elicited by this dissection; and it will be found that they present certain points of contrast with the commonly received account of the accident, which is shown by the author in the references he makes to the notices of similar dissections recorded by Sir A. Cooper, Dr. Scott and by M. Billard; and also one in which a modification of the displacement of the femur backwards is described by Mr. Wormald. It is entirely beyond the scope of a review such as the present to follow the learned Professor through the details of this division of his paper; which, however, we must do him the credit to say, exhibits both acuteness and research. We must content ourselves with the inferences deduced by him from his previous observations; and here we will permit him to speak for himself, which he does to the following effect:

"In the ordinary form of the dislocation backwards, the femur does not reach the sciatic notch.

"The head of that bone is lodged immediately behind the acetabulum, over the base of the ischiatic spine and opposite to a small part of each of the two sacro-sciatic foramina.

"The advanced position of the displaced limb at the knee, and the situation of the foot, 'the toe resting against the great toe of the other foot,' are not necessarily present in this dislocation.

"During the extension made to restore the bone to its place, the thigh is most advantageously directed across the pelvis, so that it shall form a right angle, or nearly a right angle, with the abdomen.

"At the same time the limb is to be in a state of abduction. The femur is thus drawn away from the pelvis forwards and outwards.

"And the knee is to be bent, the extending force being fixed above this joint." (p. 351.)

A clear and bold lithograph illustrates the appearances presented by the recent dissection of the parts in the immediate neighbourhood of the seat of injury.

XXVIII. *On the movements of respiration in disease, and on the use of a chest-measurer;* by Francis Sibson, Resident Surgeon to the General Hospital, near Nottingham.

We shall, in the first instance, endeavour to describe the instrument employed, though, to convey a correct idea of it without the aid of a diagram, is no easy undertaking. Let us assume that the person to be examined is lying flat upon his back, and that the chest-measurer is applied to him in this position, which is that recommended and generally adopted by the author for this purpose. The chest-measurer, then, consists of a flat brass plate or base, covered with silk, on which the patient lies, from one end of which a cylindrical rod or stem rises vertically by his side. This stem is divided into inches and tenths, and from it another rod projects horizontally; this horizontal rod or branch being inserted into a slide, which admits of free motion upon the vertical stem. The branch can be drawn out like a telescope, being formed of an outer cylinder sliding on an inner. Hence it can at pleasure be lengthened or shortened within certain limits; the outer portion can also be rotated on the inner. At the extremity of this horizontal branch which is farthest from the stem, a dial is fixed, having an index in front; while a small rod, lying close to its hinder surface, projects downwards beyond its edge, and communicates motion to the index when the free extremity of the rod is moved in the direction of its own axis. One revolution of the index corresponds to a movement of an inch taking place in the extremity of the rod, and each of the divisions on its graduated circumference, to one of an hundredth of an inch. It is the free extremity of this last-mentioned rod which is to be so applied to the part, the motion of which is to be ascertained, that the direction of the rod may correspond with that of the motion. If the vertical elevation of the sternum, in one lying horizontally, is to be measured, the rod must have its extremity brought into contact with that part, and must itself be placed vertically. This is easily effected, in consequence of the freedom of motion secured by the construction of the instrument.

Mr. Sibson first examines the *Movements of Respiration in Health*, and commences by dividing the ribs into three sets, terming the first to the fifth inclusive, thoracic,—the ninth to the twelfth, diaphragmatic,—and the remaining three, viz. the sixth, seventh, and eighth, intermediate.

Each of the thoracic set, he tells us, ascends during inspiration more than that next above it; consequently they move nearer to each other; while the diaphragmatic and intermediate ribs move further apart.

The following is his *Summary of the Respiratory Movements in Health*, which we extract at length:

"In the healthy robust male, the movement of the sternum and of the thoracic and intermediate ribs, from the first to the seventh, is from ·02 to ·07 inches, during an ordinary inspiration, and from ·5 or ·7 to 2 inches (the amount varying with the extreme breathing capacity) during a deep inspiration. The ordinary abdominal movement (diaphragmatic) is from ·25 to ·3 inches, the extreme ·6 to 1·6 inches. The ordinary lateral expansion of the diaphragmatic or lower ribs is greater, and the extreme expansion is usually less, than the respective ordinary and extreme expansion of the thoracic or upper ribs. The expansion of the second ribs is usually alike on both sides; below, all the inspiratory movements, especially those over the heart, are usually somewhat less on the left side than on the right, both during ordinary and extreme inspiration.

"In the healthy boy, owing to the greater flexibility of the costal cartilages, the extreme movement of the thoracic ribs is greater in proportion to the breathing-capacity than it is in the adult; the upper part of the sternum advances more than the lower end during a deep inspiration; but there is little decided difference during tranquil respiration.

"In the old man, owing to the consolidation of the cartilages, the motion of the sternum during respiration is usually greater than that of the ribs (in the youth it is less), and the lower end of the sternum usually advances more than the upper.

"In females, the thoracic expansion is exaggerated, and that of the diaphragm and of the lower ribs is restrained, owing in great part to the use of tight stays. The difference is much greater when the stays are on than when they are off.

"When the stays are on, the thoracic movement at the second rib is from ·06 to ·2 inches; the abdominal from ·06 to ·11 inches. When they are off, the thoracic movement is from ·03 to ·1 inches, and the abdominal from ·08 to ·2 inches.

"The restrained movement of the lower ribs, during a deep inspiration, is much greater when the stays are on, than when they are off.

"In infants, the thoracic expansion is considerable, being from ·02 to ·12 inches; while the abdominal is from ·06 to ·15 inches. The lower end of the sternum and the adjoining ribs, usually recede during inspiration, especially if the abdomen be large and the inspiration quick or sobbing." (pp. 376-7.)

Regarding the *Rhythm of Respiration*, it is remarked, that, in the perfectly tranquil breathing of adults, the inspiration is equal to the expiration; while, under excitement and change of age, the latter becomes prolonged, as compared with the former.

Having treated of the modifications of respiration during health, Mr. Sibson proceeds next to explain the *Causes that disturb the Respiratory Movements, the Lungs themselves being healthy.*

The motions of the ribs on both sides are restrained in posterior curvatures of the spine; those on one side in lateral curvature of the spine, injury or disease of the ribs, of the intercostal muscles, including pleurodynia, of the mamma; or of the axilla, shoulder, or arm; and probably in hemiplegia.

The motion of the diaphragm is restrained throughout by peritonitis, abdominal tumours, especially those connected with the diaphragm, and aortic aneurism. It is restrained on the right side only by greatly enlarged liver, from abscesses or hydatid cysts, and adherent liver. Shortened expiration may be regarded as one of the effects and signs of peritonitis.

Cases of general abdominal distension from flatus, ascites, or extensive, enlarged, and adherent ovarian cysts, are those in which the action of the diaphragm is slightly lessened during ordinary inspiration, considerably restrained during a deep inspiration.

We are told that ovarian cysts of moderate size, free from adhesions, and the impregnated state, even to the last months, do not affect the respiratory movements either during tranquil or deep inspiration.

None of the above causes of altered respiratory movements, except peritonitis, affect the rhythm of respiration; and this negative circumstance is one of the means by which they may be distinguished from diseases of the organs of respiration.

The third head of inquiry is, the *Effect of Diseases of the Respiratory Organs on the Movements of Respiration.*

The first section of this part of the subject treats of the effects of obstruction in the outer breathing-passage on the movements of respiration; which effects are principally visible in the sinking-in of parts of the thoracic parietes, which, in ordinary inspiration, are either raised or remain fixed in their position. Thus, when the obstruction to the entrance of air is slight, as in an instance of enlarged tonsils, only the lower end of the sternum falls in. If the obstruction be a little greater, the sixth costal cartilages in addition fall in. If it be still greater, as in a case of laceration of the larynx and trachea, the upper end of the sternum in addition falls in. If still greater, as sometimes from bronchocele, the fourth costal cartilages in addition fall in. If still greater, the second costal cartilages in addition fall in.

While, in extreme cases, all the thoracic and the intermediate ribs may collapse during inspiration, in every case, the lower or diaphragmatic ribs move outwards to the normal or to an exaggerated extent. In similar cases, the expiratory movements are just the reverse of inspiratory; that is to say, when a rib falls back in inspiration, it advances in expiration. In all cases of obstruction to respiration from laryngitis, expiration is longer than inspiration; it is so generally in obstructions at the commencement of the air-passages.

In emphysema and bronchitis we witness the effects of obstruction in the smaller bronchial tubes. In emphysema, and to a less extent in bronchitis, the form of the chest and abdomen and the position of the viscera are the same that they are during the deepest possible healthy inspiration. The chest is full and prominent, the shoulders raised, the spine curved, the sternum forward, the costal cartilages at each side of it full, but not so prominent as usual. The diameter of the chest is everywhere increased; the abdomen, just below the xyphoid cartilage, is unusually hollow, the lower boundaries of the lungs and heart are a full inch lower than in the normal state.

In inspiration, the diaphragm descends, the abdomen protrudes, and the first, second, third, and fourth ribs ascend and advance with energy at the same time; the lower end of the sternum and the sixth cartilages fall backwards in the greater number of cases from childhood to the age of fifty. The intercostal spaces fall in during inspiration.

In old age, and in adults with stiff and ossified cartilages, the lower end of the sternum advances, and the ribs move outwards.

As both the superior thoracic and the diaphragmatic muscular actions are always, and the movements usually, exaggerated, the head is lowest at each inspiration, indicating excess of costal (query, spinal?) motion; while the larynx descends considerably, indicating excess of diaphragmatic action.

The rhythm of respiration is materially and characteristically affected in Emphysema and Bronchitis. The inspiration is short; the expiration is prolonged. During expiration the air rushes out easily and quickly at first, but with increasing slowness and difficulty towards the end; and this increasing slowness of expiration distinguishes obstruction of the smaller bronchi from obstruction in the larynx, in which latter case it is also prolonged, but is equally slow throughout.

Pleuritis usually restrains the respiratory movements, sometimes because of pain, but sometimes although there be no pain. In some cases the movements are not at all lessened, and, in simple or dry pleuritis, they are seldom, if ever, entirely destroyed. The respiratory movements of the opposite lung and of the unaffected portions of the same lung, are from compensation exaggerated.

We pass on to make some extracts from the summary of the effects of phthisis on the movements of respiration:

> "There is almost invariably some movement of inspiration over a cavity. But although the part in question always advances during inspiration, especially a deep inspiration, yet at the beginning of inspiration it sometimes recedes slightly, and frequently stands still just before its inspiratory advance.
>
> "The firm walls surrounding a cavity have no inspiratory expansion; the respiratory movements over the region of dullness surrounding a cavity are much smaller than those over the cavity itself; they are often immobile; their motion is often reversed at the beginning or through the whole course of inspiration and expiration.
>
> "The descent of the diaphragm is somewhat restrained on the affected side in about one half the cases. If the diaphragm act freely, the movements of the sixth costal cartilage may be reversed. The respiratory movement of the opposite lung is, in the great majority of cases, exaggerated." (pp. 455-6).

In the fourth part, the *Effects of Diseases of the Heart and Pericardium on the Movements of Respiration* are considered.

In severe cases of pericarditis with effusion, in which the central border of the diaphragm is inflamed, the motion of the abdomen at the centre may be diminished, absent, or reversed during inspiration, and the movements of the left fourth, fifth, and sixth cartilages may be reversed (either wholly, or only at first), abolished, or diminished.

When the heart is materially enlarged, the expansion of the lower end of the sternum and of the cartilages and ribs in front, and to the side of the cardiac region, is restrained.

If there be pericardial adhesions, with valvular disease and enlargement of the heart, the costal expansion in front of the heart is restrained. Moreover, while the movements of the centre of the chest and abdomen are restrained, the lateral superior movements of the former, and the lateral movements of the latter, are not restrained; the extent of the impulse, too, is not lessened during inspiration.

The author has given, near the end of his paper, a list of the several modifications which respiration may undergo, arranging, under each variety of deviation from the healthy movements, the diseases which give rise to that particular abnormal condition. He admits that we cannot form a diagnosis of disease, by observing the arrest, restraint, or exaggeration of any particular respiratory movement; but contends that we may thereby make a good step towards it, inasmuch as we narrow our inquiry to a certain small class, and make out the seat of the disease by our inquiry.

Extensive tables are appended, in which are recorded the results of two hundred different cases observed in health, or under various diseases, on which are founded the conclusions set forth in the paper.

It will be manifest from the copious abridgment we have made, that many of the facts detailed are not new to medical men; still we conceive that, even with regard to these, Mr. Sibson's researches have given increased certainty and precision to our knowledge; while he has pointed out some cases of disease, which his method of measurement is calculated to elucidate, and to which it had not previously been generally applied.

The care and industry with which his memoir has been prepared, reflect great credit upon its author; and we must ever rank among the benefactors of the art of medicine one who has done so much to facilitate the practical diagnosis of disease.

ART. III.

1. KOSMOS. *Entwurf einer physischen Weltbeschreibung.* Von ALEXANDER VON HUMBOLDT.—*Stuttgard u. Tubingen.* 8vo. Erster Band, 1845, pp. 510; Zweiter Band, 1847, pp. 544.
2. COSMOS. *A Sketch of a Physical Description of the Universe.* By ALEXANDER VON HUMBOLDT. Translated from the German by E. C. OTTÉ.—*London,* 1848-9. In Two Volumes, post 8vo, pp. 826.
3. *Briefe über Alexander von Humboldt's Kosmos. Ein Commentar zu diesem Werke für gebildete Laien.* Erster Theil. Bearbeitet von BERNARD COTTA, Professor.—*Leipzig,* 1848. 8vo, pp. 366.

Letters on the Cosmos. Being a Popular Commentary on that Work. By Professor COTTA. First Part.

4. *Kosmos für Schulen und Laien nach Alexander von Humboldt.* Von Dr. K. G. REUSCHLE, Professor der Mathematik und Geographie zu Stuttgart.—*Stuttgart,* 1848. 8vo. Erster Theil, pp. 212; Zweiter Theil, pp. 304.

Cosmos for Schools and General Readers. By Professor REUSCHLE.

No one can rise from the perusal of the COSMOS without the conviction that he has read a most extraordinary work—a work equally remarkable for the profundity of its knowledge in the most varied departments of science, its rich stores of literary research, and the captivating style in which it is written.

The COSMOS, when completed, will form three volumes, of which two only are yet published; and we fear that the advanced age and recent indisposition of its illustrious author render it far from certain when the third volume will appear, or whether it will appear at all. The first volume comprises a sketch of all that is at present known regarding the physical phenomena of the universe; the second is divisible into two distinct and separate parts, the first of which treats of the incitements to the study of Nature afforded by descriptive poetry, landscape painting, and the cultivation of exotic plants; while the second and larger part enters into the consideration of the different epochs in the progress of discovery and of the corresponding stages of advance in human civilization. The third and last portion of the work "will, for the better elucidation of the general picture of nature, set forth those results of observation on which the present condition of scientific opinions is principally based," and will thus, probably, fill up several apparent blanks in the volumes already published.

Alexander von Humboldt was born on the 14th of September, 1769; and is therefore now in his 80th year. None of our readers can be altogether ignorant of his extensive researches in the physical geography of the New World, carried on between the years 1799 and 1804, or of the results of his scientific journey to Siberia in the year 1828, in company with his distinguished friends, Ehrenberg and Gustav Rose, and under the special protection of the Russian government. We merely allude to these points because he refers to them in his preface, in which he beautifully and feelingly explains the reasons that have induced him to undertake the present work, and sets forth, with truth and simplicity, his peculiar qualifications for such a task. "In the late evening of an active life, I offer to the public a work, whose undefined image has floated before my mind for almost half a century. I have frequently looked upon its completion as impracticable, but as often as I have been disposed to relinquish the undertaking, I have again—although, perhaps, imprudently—resumed the task." The idea of a physical description of the universe, such as he had conceived and resolved to work out, does indeed present startling difficulties; but if any man be qualified to meet and successfully grapple with those difficulties, Alexander von Humboldt is that man.

> "Although the outward relations of life, and an irresistible impulse towards knowledge of various kinds, have led me to occupy myself for many years—and apparently exclusively—with separate branches of science, as, for instance, with descriptive botany, geognosy, chemistry, astronomical determinations of position, and terrestrial magnetism, in order that I might the better prepare myself for the extensive travels in which I was desirous of engaging, the actual object of my studies has nevertheless been of a higher character. The principal impulse by which I was directed, was the earnest endeavour to comprehend the phenomena of physical objects in their general connexion, and to represent nature as one great whole, moved and animated by internal forces. My intercourse with highly-gifted men early led me to discover that, without an earnest striving to attain to a knowledge of special branches of study, all attempts to give a grand and general view of the universe would be nothing more than a vain illusion. These special departments in the great domain of natural science are, moreover, capable of being reciprocally fructified by means of the appropriative forces by which they are endowed. Descriptive botany, no longer confined to the narrow circle of the determination of genera and species, leads the observer who traverses distant lands and lofty mountains to the study of the geographical distribution of plants over the earth's surface, according to distance from the equator and vertical elevation above the sea. It is further necessary to investigate the laws which regulate the differences of temperature and climate, and the meteorological processes of the atmosphere, before we can hope to explain the involved causes of vegetable distribution; and it is thus that the observer who earnestly pursues the path of

knowledge is led from one class of phenomena to another, by means of the mutual dependence and connexion existing between them.

"I have enjoyed an advantage which few scientific travellers have shared to an equal extent, viz. that of having seen not only littoral districts, such as are alone visited by the majority of those who take part in voyages of circumnavigation, but also those portions of the interior of two vast continents which present the most striking contrasts, manifested in the Alpine tropical landscapes of South America, and the dreary wastes of the steppes in Northern Asia. Travels, undertaken in districts such as these, could not fail to encourage the natural tendency of my mind towards a generalization of views, and to encourage me to attempt, in a special work, to treat of the knowledge which we at present possess, regarding the sidereal and terrestrial phenomena of the Cosmos in their empirical relations. The hitherto undefined idea of a physical geography has thus, by an extended and perhaps too boldly imagined a plan, been comprehended under the idea of a physical description of the universe, embracing all created things in the regions of space and in the earth.

"The very abundance of the materials which are presented to the mind for arrangement and definition, necessarily imparts no inconsiderable difficulties in the choice of the form under which such a work must be presented, if it would aspire to the honour of being regarded as a literary composition. Descriptions of nature ought not to be deficient in a tone of life-like truthfulness, whilst the mere enumeration of a series of general results is productive of a no less wearying impression than the elaborate accumulation of the individual data of observation. I scarcely venture to hope that I have succeeded in satisfying these various requirements of composition, or that I have myself avoided the shoals and breakers which I have known how to indicate to others." (pp. ix—xi.)

The first volume commences with an Introduction, extending over sixty-one pages of the English translation. This Introduction consists of two distinct initiatory essays: the first is entitled "Reflections on the different degrees of enjoyment presented to us by the aspect of Nature, and the study of her laws," while the second treats of the "Limits and method of exposition of the physical description of the universe."

Of these essays we have not very much to say. There is an indefiniteness and want of methodical arrangement in them, which we do not find in any other part of the work. Nevertheless, they are beautiful—in many parts, exquisitely beautiful. Who, for instance, can read the following passage without a deep feeling of its intense truthfulness, and of the singular accuracy with which the author can depict human emotions?

"In reflecting upon the different degrees of enjoyment presented to us in the contemplation of nature, we find that the first place must be assigned to a sensation, which is wholly independent of an intimate acquaintance with the physical phenomena presented to our view, or of the peculiar character of the region surrounding us. In the uniform plain bounded only by a distant horizon, where the lowly heather, the cistus, or waving grasses, deck the soil; on the ocean shore, where the waves, softly rippling over the beach, leave a track, green with the weeds of the sea; everywhere, the mind is penetrated by the same sense of the grandeur and vast expanse of nature, revealing to the soul, by a mysterious inspiration, the existence of laws that regulate the forces of the universe. Mere communion with nature, mere contact with a free air, exercise a soothing yet strengthening influence on the wearied spirit, calm the storm of passion, and soften the heart when shaken by sorrow to its inmost depths. Everywhere, in every region of the globe, in every stage of intellectual culture, the same sources of enjoyment are alike vouchsafed to man. The earnest and solemn thoughts awakened by a communion with nature intuitively arise from a presentiment of the order and harmony pervading the whole universe, and from the contrast we draw between the narrow limits of our own existence and the image of infinity revealed on every side, whether we look upwards to the starry vault of heaven, scan the far-stretching plain before us, or seek to trace the dim horizon across the vast expanse of ocean.

"The contemplation of the individual characteristics of the landscape, and of the conformation of the land in any definite region of the earth, gives rise to a different source of enjoyment, awakening impressions that are more vivid, better defined, and more congenial to certain phases of the mind, than those of which we have already spoken. At one time the heart is stirred by a sense of the grandeur of the face of nature, by the strife of the elements, or, as in Northern Asia, by the aspect of the dreary barrenness of the far-stretching steppes; at another time, softer emotions are excited by the contemplation of rich harvests wrested by the hand of man from the wild fertility of nature, or by the sight of human habitations raised beside some wild and foaming torrent. Here I regard less the degree of intensity, than the difference existing in the various sensations that derive their charm and permanence from the peculiar character of the scene.

"If I might be allowed to abandon myself to the recollections of my own distant travels, I would instance, among the most striking scenes of nature, the calm sublimity of a tropical night, when the stars, not sparkling, as in our northern skies, shed their soft and planetary light over the gently-heaving ocean;—or I would recall the deep valleys of the Cordilleras, where the tall and slender palms pierce the leafy veil around them, and waving on high their feathery and arrow-like branches, form, as it were, "a forest above a forest;"* or I would describe the summit of the Peak of Teneriffe, when a horizontal layer of clouds, dazzling in whiteness, has separated the cone of cinders from the plain below, and suddenly the ascending current pierces the cloudy veil, so that the eye of the traveller may range from the brink of the crater, along the vine-clad slopes of Orotava, to the orange-gardens and banana-groves that skirt the shore. In scenes like these, it is not the peaceful charm uniformly spread over the face of nature that moves the heart, but rather the peculiar physiognomy and conformation of the land, the features of the landscape, the ever-varying outline of the clouds, and their blending with the horizon of the sea, whether it lies spread before us like a smooth and shining mirror, or is dimly seen through the morning mist. All that the senses can but imperfectly comprehend, all that is most awful in such romantic scenes of nature, may become a source of enjoyment to man, by opening a wide field to the creative powers of his imagination. Impressions change with the varying movements of the mind, and we are led by a happy illusion to believe that we receive from the external world that with which we have ourselves invested it.

"When far from our native country, after a long voyage, we tread for the first time the soil of a tropical land, we experience a certain feeling of surprise and gratification in recognising, in the rocks that surround us, the same inclined schistose strata, and the same columnar basalt covered with cellular amygdaloids, that we had left in Europe; and whose identity of character, in latitudes so widely different, reminds us, that the solidification of the earth's crust is altogether independent of climatic influences. But these rocky masses of schist and of basalt are covered with vegetation of a character with which we are unacquainted, and of a physiognomy wholly unknown to us; and it is then, amid the colossal and majestic forms of an exotic flora, that we feel how wonderfully the flexibility of our nature fits us to receive new impressions, linked together by a certain secret analogy. We so readily perceive the affinity existing amongst all the forms of organic life, that although the sight of a vegetation similar to that of our native country might at first be most welcome to the eye, as the sweet familiar sounds of our mother tongue are to the ear, we nevertheless, by degrees, and almost imperceptibly, become familiarised with a new home and a new climate. As a true citizen of the world, man everywhere habituates himself to that which surrounds him; yet fearful, as it were, of breaking the links of association that bind him to the home

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* "This expression is taken from a beautiful description of tropical forest scenery in 'Paul and Virginia,' by Bernardin de Saint Pierre."

> of his childhood, the colonist applies to some few plants in a far distant clime the names he had been familiar with in his native land; and by the mysterious relations existing amongst all types of organization, the forms of exotic vegetation present themselves to his mind as nobler and more perfect developments of those he had loved in earlier days. Thus do the spontaneous impressions of the untutored mind lead, like the laborious deductions of cultivated intellect, to the same intimate persuasion, that one sole and indissoluble chain binds together all nature." (Vol. i, pp. 3-5.)

How beautiful, too, is the following brief description of the "picture of nature," presented by the equatorial regions of America!

> "This portion of the surface of the globe affords in the smallest space the greatest possible variety of impressions from the contemplation of nature. Among the colossal mountains of Cundinamarca, of Quito, and of Peru, furrowed by deep ravines, man is enabled to contemplate alike all the families of plants, and all the stars of the firmament. There, at a single glance, the eye surveys majestic palms, humid forests of bambusa, and the varied species of musaceæ, while above these forms of tropical vegetation appear oaks, medlars, the sweetbrier, and umbelliferous plants, as in our European homes. There, as the traveller turns his eyes to the vault of heaven, a single glance embraces the constellation of the Southern Cross, the Magellanic clouds, and the guiding stars of the constellation of the Bear, as they circle round the arctic pole. There the depths of the earth and the vaults of heaven display all the richness of their forms and the variety of their phenomena. There the different climates are ranged the one above the other, stage by stage, like the vegetable zones, whose succession they limit; and there the observer may readily trace the laws that regulate the diminution of heat, as they stand indelibly inscribed on the rocky walls and abrupt declivities of the Cordilleras." (Vol. i, pp. 11-12.)

Did our limits permit, there are several other passages which we should be glad to transfer to our pages; as it is, we must content ourselves with a very brief notice of the leading points discussed in these essays.

Starting, then, from the spot from which we extracted the last passage, we find our author proceeding in general terms to point out how it is, that, to the inhabitants of a small section of the temperate zone, the rest of mankind owe the first revelation of a correct acquaintance with the forces governing the physical world, and that "from the same zone (which is apparently more favorable to the progress of reason, the softening of manners, and the security of public liberty) the germs of civilization have been carried to the regions of the tropics, as much by the migratory movement of races as by the establishment of colonies, differing widely in their institution from those of the Phœnicians or Greeks:" and to indicate the difficulties that have opposed the progress of physical investigations, and checked the attempt to trace phenomena to the causes from which they spring. Foremost amongst these difficulties he places inaccurate and imperfect observations, handed down from one generation to another—a physical philosophy composed of popular prejudices, which "is not only injurious, because it perpetuates error with the obstinacy engendered by the evidence of ill-observed facts, but because it hinders the mind from attaining to higher views of nature."

In referring to the apprehension entertained by a class of narrow-minded persons, "that Nature may by degrees lose a portion of the charm and magic of her power, as we learn more and more how to unveil her secrets, comprehend the mechanism of the heavenly bodies, and estimate numerically the intensity of natural forces," he forcibly points out the manner in which general views impart an exalted and solemn character to natural science, and shows, by well-selected examples, drawn from astronomy, recent optical discoveries, physical geognosy, and the geography of plants, that it is always possible to separate generalities from specialities; and he concludes with the following remarks on the object of this introductory essay, which is not "solely to draw attention to the importance and greatness of the physical history of the universe, for, in the present day, these are too well understood to be contested; but likewise to prove how, without detriment to the stability of special studies, we may be enabled to generalize our ideas by concentrating them in one common focus, and thus arrive at a point of view, from which all the organisms and forces of nature may be seen as one living, active whole, animated by one sole impulse."

The second essay, on the "Limits and Method of Exposition of the Physical Description of the Universe," commences with an attempt to establish *the science of the Cosmos* as a distinct and independent department of knowledge, and not a mere encyclopædic compilation of the physical sciences. How he regards it may be learned from the following passages:

> "Physical cosmography is founded on the contemplation of all created things—all that exists in space, whether as substances or forces—that is, all the material beings that constitute the universe. The science which I would attempt to define, presents itself, therefore, to man, as the inhabitant of the earth, under a twofold form—as treating of the earth itself, and of the regions of space. . . . . . In this work, I use the word COSMOS in conformity with the Hellenic usage of the term, subsequently to the time of Pythagoras, and in accordance with the precise definition given of it in the treatise entitled *De Mundo*, which was long erroneously attributed to Aristotle. It is the assemblage of all things in heaven and earth, the universality of created things constituting the perceptible world. If scientific terms had not been long diverted from their true verbal signification, the present work ought rather to have borne the title of *Cosmography*, divided into *Uranography* and *Geography*." (Vol. i, pp. 38 and 53.)

As some of our readers may possibly not have a very distinct idea of the "usage of the term" at the period referred to, we may observe that Κόσμος originally signified *ornament*, as applied to the dress of a human being or the trappings of a horse, and that it was also used to imply *the order or adornment* of a discourse. Pythagoras was the first who used the word to designate *the order in the universe, and the universe itself*. The word, with this novel signification, soon became generally received. Thus Plato, while he designated the heavenly bodies by the name of *Uranos*, applies the term *Cosmos* to the order pervading the regions of space; and in the works of Aristotle, it signifies "the universe and the order pervading it."

The importance of distinguishing between the physical *history* and the physical *description* of the world is clearly pointed out. The former, if the materials for writing it existed, should trace every varied phase of the world's existence, "from nebulæ dissolving or condensing, to the first stratum of cryptogamic vegetation on the still imperfectly cooled surface of the earth, or on a reef of coral uplifted from the depths of ocean," whilst the latter presents a picture of all that exists in space—of the simultaneous action of natural forces, together with the phenomena which they produce.

For a correct comprehension of nature, it is, however, impossible strictly to separate the present from the past. All forms of matter—inorganic as well as organic compounds—are continually undergoing decomposition, and entering into new combinations.

> "We cannot survey the crust of our planet without recognising the traces of the prior existence and destruction of an organic world. The sedimentary rocks present a succession of organic forms, associated in groups, which have successively displaced and succeeded each other. The different superimposed strata thus display to us the faunas and floras of different epochs. In this sense the description of nature is intimately connected with its history; and the geologist, who is guided by the connexion existing amongst the facts observed, cannot form a conception of the present without pursuing, through countless ages, the history of the past. In tracing the physical delineation of the globe, we behold the present and the past reciprocally incorporated, as it were, with one another; for the domain of nature is like that of languages, in which etymological research reveals a successive development, by showing us the primary condition of an idiom reflected in the forms of speech in use at the present day. The study of the material world renders this reflection of the past peculiarly manifest, by displaying in the process of formation rocks of eruption and sedimentary strata, similar to those of former ages. If I may be allowed to borrow a striking illustration from the geological relations by which the physiognomy of a country is determined, I would say that domes of trachyte, cones of basalt, lava-streams (*coulées*) of amygdaloid with elongated and parallel pores, and white deposits of pumice, intermixed with black scoriæ, animate the scenery by the associations of the past which they awaken—acting upon the imagination of the enlightened observer like traditional records of an earlier world. Their form is their history." (Vol. i, pp. 54-5.)

The question is then briefly considered, as to the possibility of embracing the multiplicity of the phenomena of the COSMOS in the unity of thought, and under the form of a purely rational combination. Although, from the intrinsic nature of the experimental sciences, we cannot expect perfect completeness, and although "no generation of men will ever have cause to boast of having comprehended the total aggregation of phenomena," yet, by the grouping of parallel phenomena, we have been enabled to discover the empire of certain natural laws, grand and simple as nature itself.

> "The extent of this empire will no doubt increase in proportion as the physical sciences are more perfectly developed. Striking proofs of this advancement have been made manifest in our own day in the phenomena of electro-magnetism, and in the propagation of luminous rays and radiating heat. In the same manner, the fruitful doctrine of evolution shows us how, in organic development, all that is formed is sketched out beforehand, and how the tissues of vegetable and animal matter uniformly arise from the multiplication and transformation of cells." (p. 56.)

But we have already dwelt long enough on these introductory essays, and proceed to analyse the body of the first volume.

Our author thus describes the plan of this portion of his work:

> "Beginning with the depths of space and the regions of the remotest nebulæ, we will gradually descend through the starry zone to which our solar system belongs, to our own terrestrial spheroid, circled by air and ocean, there to direct our attention to its form, temperature, and magnetic tension, and to consider the fulness of organic life which is unfolded upon its surface beneath the vivifying influence of light. In this manner a picture of the world may, with a few strokes, be made to

include the realms of infinity no less than the minute microscopic animal and vegetable organisms which exist in stagnant waters and on the weather-beaten surface of our rocks. All that can be perceived by the senses, and all that has been accumulated up to the present day by an attentive and variously-directed study of nature, constitute the materials from which this representation is to be drawn, whose character is an evidence of its fidelity and truth." (Vol. i, pp. 62-3.)

Fully agreeing with our author in the opinion, that, as "the view of nature ought to be grand and free, uninfluenced by motives of proximity, social sympathy, or relative utility," the *terrestrial* must give precedence to the *celestial*, we take our stand on the regions of remotest space, and shall follow him in the stupendous course he has depicted in the preceding quotation.

Nebulæ and nebulous stars are the subjects with which he commences. As the "nebular hypothesis" was noticed at some length in a very able review of the 'Vestiges of the Natural History of Creation,' in the pages of one of our predecessors (Brit. and For. Med. Review, Vol. XIX, p. 155, &c.), it is sufficient for us to observe that, somewhat to our surprise, Humboldt regards it as an ascertained physical fact. Many of our readers are doubtless aware, that, since the date of the publication of the first volume of the COSMOS, astronomers have waited with anxious interest for the revelations to be afforded by Lord Rosse's monster telescope. In a late number of the 'Proceedings of the Royal Irish Academy,' there is an account of its present condition, by the Rev. Dr. Robison, of Armagh, from which we learn, that, with a power of 830, it completely resolved the great nebula of Orion, as well as all other nebulæ to which it was directed. We understand that more recent observations, while they resolve many, indeed we may say most, of the best known nebulæ into distinct stars, tend to confirm rather than to overthrow the nebular hypothesis generally. The question must still be regarded as an open one.

Considering, as he does, that the nebulæ are systems of stars in various stages of development, he naturally proceeds from them to treat of the *sidereal system.*

Amongst the points established with any degree of certainty regarding the firmament of stars visible to us, or, as we may term it, *our own starry stratum,* we may mention its form, extent, and distance. Our space will not allow us to enter into these points, and we will content ourselves with a single remark on *its distance.* The measured parallax of the three well-known stars, α Centauri, 61 Cygni, and α Lyræ, places them at distances from us of 226,000, 592,009, and 789,600 radii of the earth's orbit. Light travels at the rate of 166,072 geographical miles in a second (according to Struve's most recent investigations), and when we mention that a ray of light requires respectively 3, 9½, and 12 years to reach us from these three bodies, we may possibly convey to the minds of our readers a faint conception of the stupendous magnitude of the system to which we belong. Nor must it be forgotten, that these three stars are amongst those which are nearest to our system. The elder Herschel was of opinion that the rays of light from the remotest nebulæ observable by his 40-foot reflector, must have been almost two millions of years in passing to the earth; and that, consequently, these bodies must have already had an existence in the sidereal heavens so many years ago, in order to send out those rays by which we now perceive them. Much, therefore, has probably vanished long before it is rendered visible to us; in the poetic language of Humboldt, the luminous rays from these remote objects "reach us like the voices of the past." These considerations render it more than probable, that the light of the most distant heavenly bodies presents us with the most ancient perceptible evidence of the existence of matter.

The last few years have been singularly rich in certain departments of astronomical discovery. It is only four years ago since our author described the solar system as consisting of—

> "*Eleven primary planets*, eighteen satellites, or secondary planets, and myriads of comets, three of which, known as the 'planetary comets,' do not pass beyond the narrow limits of the orbits described by the principal planets. We may, with no inconsiderable degree of probability, include within the domain of our sun, in the immediate sphere of its central force, a rotating ring of vaporous matter, lying probably between the orbits of Venus and Mars, but certainly beyond that of the earth, which appears to us in a pyramidal form, and is known as the *zodiacal light;* and a host of very small asteroids, whose orbits either intersect, or very nearly approach that of our earth, and which present us with the phenomena of aërolites and falling or shooting stars." (p. 74.)

Instead of the *eleven primary planets*, we are now acquainted with *seventeen*, viz. with (in addition to those referred to in the text) Astrea, discovered by Henke at Driessen, on the 8th of December, 1845; Neptune, discovered by Galle, at Berlin, on the 23d of September, 1846; Hebe, discovered by Henke, on the 1st of July, 1847; Iris, discovered by Hind, at Mr. Bishop's observatory, in Regent's Park, on the 13th of August, 1847; Flora, discovered by the same observer, on the 18th of October of the same year; and lastly, Metis, discovered by Graham, at Mr. Cooper's observatory, at Markree, in Ireland, on the 25th of April, 1848. Thus, six planets have been discovered in the space of two years and a half. The discovery of these telescopic planets leads us to suspect the existence of many others yet undiscovered, and, in all probability, in a few years, there will be a large addition to the known members of the solar system. M. Valz has laid before the French Academy of Sciences, a system, by which, he believes, all these unknown planets might be discovered in four years, by examining carefully all the small stars near the ecliptic. M. Faye, the French translator of the COSMOS, has constructed a celestial chart, containing all the stars, to the twelfth magnitude, in or near a section of the ecliptic, illustrating the views of M. Valz.

The discovery of the planet Neptune requires more than the mere passing record of the fact. It may be regarded as one of the most glorious intellectual triumphs of this or, indeed, of any age. We extract the following paragraph from an able note on the subject, by the translator (or rather, we believe we should say, the translatress) of these volumes.

> "The discovery of this planet is not only highly interesting from the importance attached to it as a question of science, but also from the evidence it affords of the care and unremitting labour evinced by modern astronomers in the investigation and comparison of the older calculations, and the ingenious application of the results thus obtained to the observation of new facts. The merit of having paved the way for the discovery of the planet Neptune is due to M. Bouvard, who, in his persevering and assiduous efforts to deduce the entire orbit of Uranus from observations made during the forty years that succeeded the discovery of that planet in 1781, found the results yielded by theory to be at variance with fact, in a degree

that had no parallel in the history of astronomy. This startling discrepancy, which seemed only to gain additional weight from every attempt made by M. Bouvard to correct his calculations, led Leverrier, after a careful modification of the tables of Bouvard, to establish the proposition that there was "a formal incompatibility between the observed motions of Uranus and the hypothesis that he was acted on *only* by the Sun and known planets, according to the law of universal gravitation." Pursuing this idea, Leverrier arrived at the conclusion that the disturbing cause must be a *planet*, and finally, after an amount of labour that seems perfectly overwhelming, he, on the 31st of August, 1846, laid before the French Institute a paper, in which he indicated the exact spot in the heavens where this new planetary body would be found. Essential difficulties still intervened, however, and as the remoteness of the planet rendered it improbable that its disc would be discernible by any telescopic instrument, no other means remained for detecting the suspected body but its planetary motion, which could only be ascertained by mapping, after every observation, the quarter of the heavens scanned, and by a comparison of the various maps. Fortunately for the verification of Leverrier's predictions, Dr. Bremiker had just completed a map of the precise region in which it was expected the new planet would appear, this being one of a series of maps made for the Academy of Berlin, of the small stars along the entire zodiac. By means of this valuable assistance, Dr. Galle, of the Berlin Observatory, was led, on the 25th of September, 1846, by the discovery of a star of the eighth magnitude, not recorded in Dr. Bremiker's map, to make the first observation of the planet predicted by Leverrier." (Vol. i, pp. 274-5, *note.*)

It is unnecessary for us to follow the author of the note into the question of the disputed claim of Adams and Leverrier to this discovery. No one who has carefully read the documents that have been published on the subject can entertain a doubt that the English and French astronomers almost simultaneously made the discovery, pursuing different courses of investigation, each in entire ignorance of the proceedings of the other, and yet both agreeing on almost the identical spot on which the planet, never seen by mortal eye, would first appear. We cannot conclude this subject without an expression of regret at the attempt, first made by Professor Peirce, an American astronomer, and subsequently by M. Babinet, to deprive Leverrier and Adams of their well-earned laurels, by asserting that the planet Neptune is not the planet that their calculations had pointed out. That there should be certain differences in the elements of this planet, as indicated by pure mathematical reasoning, and as deduced from observation, was only to be expected; and they are certainly not greater than might have been fairly anticipated.

We now come to an excellent sketch of those wonderful and mysterious bodies, which in earlier times have often struck terror into the hearts of nations—the comets.

It is worthy of observation, that the earliest important observations of comets are due to the Chinese, and extend back to more than five hundred years before the Christian era; and in later times, whilst the comet of 837, which for twenty-four hours was within 2,000,000 miles from the earth, terrified Louis I of France to such a degree that he busied himself in building churches and founding monastic institutions, in the hope of averting the evils threatened by its appearance, the Chinese astronomers were coolly observing its path and measuring the length of its tail.

It would be endless to recount the diversities of form presented by these bodies.

> "The tails sometimes appear single, sometimes, although more rarely, double; and in the comets of 1807 and 1843 the branches were of different lengths; in one instance (1744) the tail had six branches, the whole forming an angle of 60°. The tails have been sometimes straight, sometimes curved, either towards both sides, or towards the side appearing to us as the exterior (as in 1811), or convex towards the direction in which the comet is moving (as in that of 1618); and sometimes the tail has even appeared like a flame in motion." (p. 86.)

Some comets, again, have no visible tail. Moreover, besides differing from one another in form, the same comet may often be seen to undergo successive and rapidly-occurring changes in configuration. The most singular change ever remarked has been noticed since the original publication of the COSMOS; Biela's comet being observed, in 1846, to separate into two parts, which continued their course, side by side, without apparently disturbing one another.

We cannot close our remarks on comets without a reference to the splendid experiments instituted regarding the origin of their luminosity:

> "The most important and decisive observations that we possess on the nature and the light of comets, are due to Arago's polarization experiments. His polariscope instructs us regarding the physical constitution of the Sun and comets, indicating whether a ray that reaches us from a distance of many millions of miles, transmits light directly, or by reflection; and if the former, whether the source of light is a solid, a liquid, or a gaseous body. His apparatus was used at the Paris Observatory, in examining the light of Capella, and that of the great comet of 1819. The latter showed polarized, and therefore reflected, light, whilst the fixed star, as was to be expected, appeared to be a self-luminous sun. The existence of polarized cometary light announced itself not only by the inequality of the images, but was proved with greater certainty on the reappearance of Halley's comet, in the year 1835, by the more striking contrast of the complementary colours, deduced from the laws of chromatic polarization discovered by Arago in 1811. These beautiful experiments still leave it undecided, whether, in addition to this reflected solar light, comets may not have light of their own. Even in the case of the planets, as, for instance, in Venus, an evolution of independent light seems very probable." (Vol. i, pp. 90-1.)

The author concludes the subject with one of those beautiful episodes, with which all his descriptive works abound—contrasting the vague fears entertained in early ages, of the general conflagration of the world by *flaming swords* and stars with *fiery streaming hair*, with the more cheerful, although singular, turn which the public mind has now taken, more especially in his own country, with regard to comets; for, in the German vineyards in the picturesque valleys of the Rhine and the Moselle, it is now generally believed that these once ill-omened bodies exert a beneficial influence on the ripening of the vine. They are regarded as wandering stars, capable of radiating heat.

From comets we pass to the consideration of the phenomena and origin of meteors, including meteoric stones, shooting-stars, and fire-balls. These are regarded as small bodies, moving with planetary velocity and revolving, in obedience to the general law of gravitation, around the sun as a focus. If they approach the earth in their course, they are attracted by its mass, and, entering within the limits of our atmosphere in a luminous condition, frequently let fall more or less strongly-heated fragments, covered with a shining black crust. The connexion existing between meteoric stones and fire-balls, the former being forcibly projected from the latter, is thoroughly established by numerous instances, many of which are quoted in the text.

> "The formative power, and the nature of the physical and chemical processes involved in these phenomena are questions all equally shrouded in mystery, and we are as yet ignorant, whether the particles composing the dense mass of meteoric stones are originally, as in comets, separated from one another in the form of vapour, and only condensed within the fiery ball when they become luminous to our sight, or whether in the case of smaller shooting-stars any compact substance actually falls, or, finally, whether a meteor is composed only of a smoke-like dust, containing iron and nickel; whilst we are wholly ignorant of what takes place within the dark cloud from which a noise like thunder is often heard for many minutes before the stones fall." (Vol. i, p. 101.)

The largest meteoric masses at present known are, according to our author, those of Otumpa, in Chaco, and of Bahia, in Brazil, which are upwards of seven feet in length. Humboldt ventures to express a hope, that the discovery of the enormous meteoric stone of Ægos Potamos, celebrated in the annals of antiquity, may even yet, after the lapse of more than twenty-three centuries, reward the labours of some Thracian traveller. A huge aerolite, which, in the beginning of the tenth century, fell into the river at Narni, has actually been found during the last few years. The following are our author's remarks on the physical and chemical characters of these solid masses:

> "They undeniably exhibit a great degree of general identity with respect to their external form, the character of their crust, and the chemical composition of their principal constituents. These characteristics of identity have been observed at all the different epochs and in the most various parts of the earth in which these meteoric stones have been found. This striking and early-observed analogy of physiognomy in the denser meteoric masses is, however, met by many exceptions regarding individual points. What differences, for instance, do we not find between the malleable masses of iron of Hradschina, in the district of Agram, those from the shores of the Sisim, in the government of Jeniseisk, rendered so celebrated by Pallas, or those which I brought from Mexico, all of which contain 96 per cent. of iron, and the aerolites of Siena, in which the iron scarcely amounts to 2 per cent., or the earthy aerolite of Alais (in the Department du Gard), which broke up in water; or lastly, those of Jonzac and Juvenas, which contained no metallic iron, but presented a mixture of oryctognostically distinct crystalline components! These differences have led mineralogists to separate these cosmical masses into two classes, namely, those containing nickelliferous meteoric iron, and those consisting of fine or coarsely-granular meteoric dust. The crust or rind of aerolites is peculiarly characteristic of these bodies, being only a few tenths of a line in thickness, often glossy and pitch-like, and occasionally veined. There is only one instance on record, as far as I am aware (the aerolite of Chantonnay, in La Vendée), in which the rind was absent, and this meteor, like that of Juvenas, presented likewise the peculiarity of having pores and vesicular cavities. In all other cases the black crust is divided from the inner light-gray mass by as sharply-defined a line of separation as is the black leaden-coloured investment of the white granite blocks which I brought from the cataracts of the Orinoco, and which are also associated with many other cataracts, as, for instance, those of the Nile and of the Congo River. The greatest heat employed in our porcelain ovens would be insufficient to produce anything similar to the crust of meteoric stones, whose interior remains wholly unchanged. Here and there, facts have been observed which would seem to indicate a fusion together of the meteoric fragments; but in general the character of the aggregate mass, the absence of compression by the fall, and the inconsiderable degree of heat possessed by these bodies when they reach the earth,

are all opposed to the hypothesis of the interior being in a state of fusion during their short passage from the boundary of the atmosphere to our earth.

"The chemical elements of which these meteoric masses consist, and on which Berzelius has thrown so much light, are the same as those distributed throughout the earth's crust, and are fifteen in number, namely, iron, nickel, cobalt, manganese, chromium, copper, arsenic, zinc, potash, soda, sulphur, phosphorus, and carbon; constituting altogether nearly one third of all the known simple bodies. Notwithstanding this similarity with the primary elements into which inorganic bodies are chemically reducible, the aspect of aerolites, owing to the mode in which their constituent parts are compounded, presents, generally, some features foreign to our telluric rocks and minerals. The pure native iron, which is almost always found incorporated with aerolites, imparts to them a peculiar but not consequently a *selenic* character; for in other regions of space, and in other cosmical bodies besides our moon, water may be wholly absent, and processes of oxidation of rare occurrence." (Vol. i, pp. 117-19.)

The celebrated astronomer Olbers has directed attention to the fact, that, while shells are found in secondary and tertiary formations, no *fossil meteoric stones* have as yet been discovered. "May we," he observes, "conclude from this circumstance, that, previous to the present and last modification of the earth's surface, no meteoric stones fell on it, although, at the present time, it appears probable, from the researches of Schreibers, that 700 fall annually?"

A mass of nickelliferous iron, closely resembling a meteoric stone, has been found in the gold-sand deposit of Petropawlowsk, on the Altai, at a depth of 31½ feet below the surface. Assuming it to be a meteoric stone, it doubtless fell at a period far anterior to that attained by any historical records; but, in a geological sense, the age of this deposit is insignificant.

The most recent case with which we are acquainted, of the fall of a meteoric stone, is that which occurred at Branau, in Bohemia, on the 14th of July, 1847. Its fall was accompanied by a loud explosion, and it was found buried three feet below the surface of the earth. It was too hot to be comfortably touched for six hours after its fall. It weighed 42¼ pounds; its specific gravity was 7·7142 (according to Humboldt, the specific gravity of these bodies varies from 1·9 to 4·3); it was of an iron-gray colour, and of a somewhat quadrangular form, with concavities on its surface. A second portion, weighing upwards of 30 pounds, was found about a mile distant.

Analysed by Duflos and Fischer, it was found to consist of 91·82 parts of iron, 5·517 of nickel, 0·529 of cobalt, and minute quantities, amounting to 0·072 of copper, manganese, arsenic, calcium, magnesium, silicium, carbon, chlorine, and sulphur.

From the distance at which the fragments reached the ground, and from the angle of incidence, it was calculated that the meteor exploded at a height of nearly 30,000 feet.

The alleged periodicity of falls of shooting-stars has recently attracted much popular attention. On this subject Humboldt observes:

"Shooting-stars fall either separately and in inconsiderable numbers, that is, sporadically, or in swarms of many thousands. The latter, which are compared by Arabian authors to swarms of locusts, are periodic in their occurrence, and move in streams, generally in a parallel direction. Amongst periodic falls, the most celebrated are that known as the November phenomenon, occurring from about the

12th to the 14th of November, and that of the festival of St. Lawrence (the 10th of August), whose 'fiery tears' were noticed in former times in a church calendar of England, no less than in old traditionary legends, as a meteorological event of constant recurrence. Notwithstanding the great quantity of shooting-stars and fire-balls of the most various dimensions, which, according to Klöden, were seen to fall at Potsdam, on the night between the 12th and 13th of November, 1822, and on the same night of the year in 1832, throughout the whole of Europe, from Portsmouth to Orenberg on the Ural River, and even in the southern hemisphere, as in the Isle of France, no attention was directed to the *periodicity* of the phenomenon, and no idea seems to have been entertained of the connexion existing between the fall of shooting-stars and the recurrence of certain days, until the prodigious swarm of shooting-stars which occurred in North America between the 12th and 13th of November, 1833, and was observed by Olmsted and Palmer. The stars fell, on this occasion, like flakes of snow, and it was calculated that at least 240,000 had fallen during a period of nine hours. Palmer, of New Haven, Connecticut, was led, in consequence of this splendid phenomenon, to the recollection of the fall of meteoric stones in 1799, first described by Ellicot and myself, and which, by a comparison of the facts I had adduced, showed that the phenomenon had been simultaneously seen in the New Continent from the equator to New Herrnhut in Greenland (64° 14′ lat.), and between 46° and 82° long. The identity of the epochs was recognised with astonishment. The stream, which had been seen from Jamaica to Boston (40° 21′ lat.) to traverse the whole vault of heaven on the 12th and 13th of November, 1833, was again observed in the United States in 1834, on the night between the 13th and 14th of November, although on this latter occasion it showed itself with somewhat less intensity. In Europe the periodicity of the phenomenon has since been manifested with great regularity.

"Another and a like regularly recurring phenomenon is that noticed in the month of August, the meteoric stream of St. Lawrence, appearing between the 9th and 14th of August. Muschenbroek, as early as in the middle of the last century, drew attention to the frequency of meteors in the month of August; but their certain periodic return about the time of St. Lawrence's day was first shown by Quetelet, Olbers, and Benzenberg. We shall, no doubt, in time discover other periodically-appearing streams, probably about the 22d to the 25th of April, between the 6th and 12th of December, and, to judge by the numbers of true falls of aerolites enumerated by Capocci, also between the 27th and 29th of November, or about the 17th of July." (Vol. i, pp. 111-14.)

Other observations of much interest have been instituted regarding the height, velocity, and magnitude of these bodies. For information on these points, we must refer to the COSMOS, and will merely notice the general direction of their apparent motion, a point of great importance in evidence of the *cosmical* origin. It has been shown by Denison Olmsted, of Newhaven, Connecticut (not Massachusetts, as Humboldt has it), that, during the celebrated fall of shooting-stars on the night between the 12th and 13th of November, 1833, the concurrent authority of all eye-witnesses went to prove that the fire-balls and shooting stars all emerged from one and the same quarter of the heavens, namely, in the vicinity of the star γ Leonis, and did not deviate from this point, although the star changed its apparent height and azimuth during the observation. Such an independence of the earth's rotation shows that the luminous bodies came from without; that is to say, that they entered our atmosphere from the external regions of space.

There is every reason to believe that these asteroid-meteors form a closed ring or zone, within which they all pursue one common orbit, which intersects our earth's orbit, much in the same manner that the smaller planets between Mars and Jupiter (with the exception of Pallas) are known to do.

It is worthy of remark, that the Chinese annals have recorded great falls of shooting-stars for seven centuries anterior to the Christian era, and that, from fifty-two observations which Biot has collected from that source, he has drawn conclusions tending to elucidate several important points relative to these bodies.

Gladly would we linger over our author's picturesque description "of the mild radiance with which the zodiacal light, shooting pyramidally upwards, illumines a part of the uniform length of the tropical night," but which, in our gloomy northern zone, is only distinctly visible in the beginning of spring, after the evening twilight, in the western part of the sky, and at the close of autumn, before the dawn of day, above the eastern horizon; and his sketch of the milky way; but our limits warn us that our progress must be hastened, and we follow him "from the regions of celestial forms to the more contracted sphere of terrestrial forces,—from the children of Uranus to those of Gea."

This department commences with a reference to the mysterious chain uniting the two classes of phenomena, and which is especially revealed through certain phenomena of light and radiating heat. The difference in the nature of our knowledge regarding the celestial bodies and our own globe is thus noticed:

> "The physical description of celestial bodies, from the remotely-glimmering nebulæ with their suns, to the central body of our own system, is limited, as we have seen, to general conceptions of the volume and quantity of matter. No manifestation of vital activity is there presented to our senses. It is only from analogies, frequently from purely ideal combinations, that we hazard conjectures on the specific elements of matter, or on their various modifications in the different planetary bodies. But the physical knowledge of the heterogeneous nature of matter, its chemical differences, the regular forms in which its molecules combine together, whether in crystals or granules; its relations to the deflected or decomposed waves of light by which it is penetrated; to radiating, transmitted, or polarized heat; and to the brilliant or invisible, but not on that account less active, phenomena of electro-magnetism—all this inexhaustible treasure, by which the enjoyment of the contemplation of nature is so much heightened, is dependent on the surface of the planet which we inhabit, and more on its solid than on its liquid parts. I have already remarked how greatly the study of natural objects and forces, and the infinite diversity of the sources they open for our consideration, strengthen the mental activity, and call into action every manifestation of intellectual progress. These relations require, however, as little comment as that concatenation of causes by which particular nations are permitted to enjoy a superiority over others, in the exercise of a material power derived from their command of a portion of these elementary forces of nature." (Vol. i, pp. 147-8.)

How comparatively limited are the boundaries of that portion of space from which we derive all our knowledge of the heterogeneous nature of matter! Certain coal-basins in Belgium are ascertained by calculation to be sunk as far below the level of the sea, as Chimborazo is elevated above it. Hence, from the tops of the highest mountains, to the lowest basins containing the vegetation of an earlier world, there is a vertical distance of about 48,000 feet, or not more than the 435th part of the earth's radius; or, in other words, the limits of vertical depth and of the superposition of mineral strata, to which geognostical inquiry could penetrate, even if the general elevation of the upper surface of the earth were equal to the height of the loftiest mountain, would scarcely exceed eight miles.

The size, figure, and density of our earth are next fully considered. The size and figure are determined by three methods; namely, by measurements of degrees, oscillations of the pendulum, and observations on lunar inequalities. On the first two, our author has collected much curious historical information. "When the earth had been measured, it still had to be weighed;" and there are three ways by which its weight or mean density have been determined; namely, by the deflection of the plummet from the vertical line in the vicinity of a mountain; by a comparison of the length of a pendulum on a plain, and on the summit of an elevation; and, finally, by the employment of the torsion balance. The last is the most accurate method; and it is singular that our author has overlooked the accurate experiments of our countryman, Baily, which were known in Germany as early as 1843 (being recorded in Studer's 'Physical Geography'), and which give 5·66 for the earth's mean density.

Passing over the consideration of the probable condition of the interior of the earth, and the evidence adduced to prove the constancy of the mean temperature of the globe (which shows that, since the time of Hipparchus, that is, for upwards of 2000 years, it has not diminished by as much as $\frac{1}{300}$th of a degree of Fahrenheit), we arrive at the subject of terrestrial magnetism.

For some very curious information regarding the history of magnetic discovery, we must refer our readers to vol. i, pp. 173-86, and vol. ii, pp. 628-30 and 654-58 of the English translation. We will merely observe, that there is strong historical evidence to prove that the directive power of the magnetic needle and its relation to terrestrial magnetism were known to the Chinese more than a thousand years before our era. It is impossible for us to do more than indicate the various heads under which our author treats this his favorite science. We must venture to remind our readers, that terrestrial magnetism exhibits three classes of phenomena. A magnetic needle, floating in water, or freely suspended by a fibre, or balanced on a pivot, points either directly to the north, or to some other point in the heavens. The angle between its direction and that of the meridian at any spot is called the *declination* or *variation of the needle;* and lines passing through the different places where this declination is equal, are called *lines of equal declination,* or *isogonic lines.* If the needle be accurately balanced before it be magnetised, it will, when freely suspended, *dip* or assume a fixed position, forming an angle with the horizon. This *dip* varies in different places, and lines drawn through all the spots at which it is equal are called *isoclinic lines.* If a needle be made to oscillate horizontally, the number of oscillations which it performs, before it resumes its position of rest, represent the degree of magnetic force; and lines of equal magnetic force are called *isodynamic lines.* These lines are graphically depicted in various charts, and it is only by a reference to such charts that the general reader can form any definite idea of their curves and undulations. These three elements—the variation, the declination, and the force (or intensity)—are not constant. They vary from age to age, so that magnetic charts require frequent revision. They have also annual and diurnal changes, the elements returning to their original value at the expiration of a year and a day respectively; and lastly, they are sometimes disturbed in a sudden and irregular manner, constituting *magnetic storms*. These magnetic storms are indicated by the simultaneous agitation of the needle over vast regions, and probably sometimes over the whole surface of the globe. One of the most remarkable magnetic storms on record, is that which occurred on the 25th of September, 1841, which was observed at Toronto in Canada, at the Cape of Good Hope, at Macao, at Prague, and in Van Diemen's Land.

Intimately connected with these magnetic storms, and in a degree dependent on them, is the *aurora borealis*.

> "The bold conjecture, hazarded 128 years ago by Halley, that the aurora borealis was a magnetic phenomenon, has acquired empirical certainty from Faraday's brilliant discovery of the evolution of light by magnetic forces. The northern light is preceded by premonitory signs. Thus in the morning before the occurrence of the phenomenon, the irregular horary course of the magnetic needle generally indicates a disturbance of the equilibrium in the distribution of terrestrial magnetism. When this disturbance attains a great degree of intensity, the equilibrium of the distribution is restored by a discharge attended by a development of light." (Vol. i, p. 188.)

In a note, furnished by the translator, we find that the magnificent aurora of October 24th, 1847, which was one of the most brilliant ever known in this country, was preceded by great magnetic disturbance.

> "On the night between the 23d and 24th of October, the changes of position were very large and very frequent, the magnet at times moving across the field so rapidly that a difficulty was experienced in following it. During the day of the 24th of October, there was a constant change of position, but after midnight, when the aurora began perceptibly to decline in brightness, the disturbance entirely ceased." (Vol. i, p. 188, *note*.)

It does not, however, follow as a necessary sequence, that magnetic storms should always terminate in these luminous effusions, any more than that heavily-charged clouds should always present the phenomenon of lightning on their discharge.

It is now generally admitted that there are two distinct varieties of this splendid phenomenon. Of the more magnificent and the more common variety, our author gives the following picturesque and beautiful sketch:

> "Low down in the distant horizon, about the part of the heavens which is intersected by the magnetic meridian, the sky which was previously clear is at once overcast. A dense wall or bank of cloud seems to rise gradually higher and higher, until it attains an elevation of 8 or 10 degrees. The colour of the dark segment passes into brown or violet; and stars are visible through the cloudy stratum, as when a dense smoke darkens the sky. A broad brightly luminous arch, first white, then yellow, encircles the dark segment; but as the brilliant arch appears subsequently to the smoky gray segment, we cannot agree with Argelander in ascribing the latter to the effect of mere contrast with the bright luminous margin. The highest point of the arch of light is, according to accurate observations made on this subject, not generally in the magnetic meridian itself, but from 5° to 18° towards the direction of the magnetic declination of the place. In northern latitudes, in the immediate vicinity of the magnetic pole, the smoke-like conical segment appears less dark, and sometimes is not even seen. Where the horizontal force is the weakest, the middle of the luminous arch deviates the most from the magnetic meridian.
>
> "The luminous arch remains sometimes for hours together flashing and kindling in ever-varying undulations, before rays and streamers emanate from it, and shoot up to the zenith. The more intense the discharges of the northern light, the more

bright is the play of colours, through all the varying gradations from violet and bluish white to green and crimson. Even in ordinary electricity excited by friction, the sparks are only coloured in cases where the explosion is very violent after great tension. The magnetic columns of flame rise either singly from the luminous arch, blended with black rays similar to thick smoke, or simultaneously in many opposite points of the horizon, uniting together to form a flickering sea of flame, whose brilliant beauty admits of no adequate description, as the luminous waves are every moment assuming new and varying forms. The intensity of this light is at times so great, that Lowenörn (on the 29th of June, 1786) recognised the coruscation of the polar light in bright sunshine. Motion renders the phenomenon more visible. Round the point in the vault of heaven which corresponds to the direction of the inclination of the needle, the beams unite together to form the so-called corona, the crown of the northern light, which encircles the summit of the heavenly canopy with a milder radiance and unflickering emanations of light. It is only in rare instances that a perfect crown or circle is formed, but on its completion the phenomenon has invariably reached its maximum, and the radiations become less frequent, shorter, and more colourless. The crown and the luminous arches break up, and the whole vault of heaven becomes covered with irregularly-scattered, broad, faint, almost ashy-gray, luminous immoveable patches, which in their turn disappear, leaving nothing but a trace of the dark smoke-like segment on the horizon. There often remains nothing of the whole spectacle but a white, delicate cloud with feathery edges, or divided at equal distances into small roundish groups, like cirro-cumuli." (Vol. i, pp. 189-91.)

The other form of aurora to which we alluded is unnoticed by our author. It consists of great luminous masses, sometimes almost isolated, but more commonly arranged in more or less perfect arches transverse to the magnetic meridian, and constantly drifting southward with a slow and steady movement. While in the former variety the arrangement is in lines parallel to the dipping-needle, in this we observe either amorphous masses, or more commonly a tendency to an arch transverse to the meridian. It has been suggested by one of our most eminent physicists, that the distinction between the magnetic and diamagnetic forms of matter recently discovered by Faraday* may take a part in these arrangements.

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* The discovery alluded to is one of such paramount importance, that we feel it expedient to direct the attention of our readers to the subject. It consists essentially in this,—that all liquid and solid substances are subject to magnetic influence, and are divisible into two great classes, the *magnetic*, and the *diamagnetic*. The former class includes those bodies which exhibit the well-known phenomena of ordinary magnetic attraction and repulsion, being *attracted* when in their natural state by either pole of a magnet, and, when shaped into bars or rods, and suspended between two opposite magnetic poles, pointing *axially*, that is to say, in a straight line between them. Iron and nickel are the well-known types of this class.

The latter class includes all liquids and solids which do not belong to the magnetic class. The law governing the action of magnets on these bodies is as follows: *A particle of a diamagnetic body placed in the neighbourhood of either pole of a magnet is* REPELLED *by the pole*. Hence a bar of any diamagnetic substance, suspended by its centre midway between two magnetic poles, will point *equatorially*, or at right angles to the line joining the two poles—that being the position in which every part of it is at the greatest possible distance from each of the poles. The position which a bar of any substance assumes when suspended horizontally between two magnetic poles, furnishes the best means of ascertaining whether it belongs to the magnetic or diamagnetic class; if it be magnetic, it will place itself axially; if diamagnetic, equatorially. Bismuth appears to be the most powerfully diamagnetic of all known substances.

"Of the substances which form the crust of the earth (says Dr. Faraday), by far the greater portion belong to the diamagnetic class; and though ferruginous and other magnetic matters, through being more energetic in their action, are more striking in their phenomena, we should be hasty in assuming, that therefore they overrule entirely the effect of the former bodies. As regards the ocean, lakes, rivers, and the atmosphere, they will exert their peculiar effect almost uninfluenced by any magnetic power in them, and, as respects the rocks and mountains, their diamagnetic influence is perhaps greater than might be anticipated." He then describes some experiments, from which he infers, that "it is not at all unlikely that many of the masses which form the crust of this our globe, may have an excess of diamagnetic power, and act accordingly."

The knowledge which we at present possess of the altitude of the aurora is, according to Humboldt, deserving of little confidence, the results varying between several miles and an elevation of 3000 feet. The translator observes in a foot-note, that the height of the bow of light of the aurora, seen at Cambridge on the 19th of March, 1847, was determined by Professor Challis to be 177 miles above the earth's surface.

We proceed to a new and very different subject—to the various manifestations of the internal activity of our planet as seen on its surface.

Our author commences by observing that, as the internal heat of our planet is connected on the one hand with its magnetic phenomena, and consequently with the development of terrestrial light, as shown by the remarkable connexion between the curvature of the magnetic and isothermal lines, first detected by Sir David Brewster, and by the fact of diurnal and annual magnetic periodicity; so, on the other hand, it is the cause of a very different class of effects, giving rise to a group of phenomena, which at first sight appear most heterogeneous, as thermal springs, the effusion of carbonic acid and sulphurous vapour, innocuous salses (mud eruptions), and the fearful devastations of volcanic mountains.

> "In a general view of nature, all these phenomena are fused together in one sole idea of the reaction of the interior of a planet on its external surface. We thus recognise in the depths of the earth, and in the increase of temperature with the increase of depth from the surface, not only the germ of disturbing movements, but also of the gradual elevation of whole continents (as mountain chains on long fissures), of volcanic eruptions, and of the manifold production of mountains and mineral masses. The influence of this reaction of the interior on the exterior is not, however, limited to inorganic nature alone. It is highly probable, that, in an earlier world, more powerful emanations of carbonic acid gas, blended with the atmosphere, must have increased the assimilation of carbon in vegetables, and that an inexhaustible supply of combustible matter (lignites and carboniferous formations) must have been thus buried in the upper strata of the earth by the revolutions attending the destruction of vast tracts of forest. We likewise perceive that the destiny of mankind is in part dependent on the formation of the external surface of the earth, the direction of mountain tracts and high lands, and on the distribution of elevated continents. It is thus granted to the inquiring mind to pass from link to link along the chain of phenomena until it reaches the period when, in the solidifying process of our planet, and in its first transition from the gaseous form to the agglomeration of matter, that portion of the inner heat of the earth was developed, which does not belong to the action of the sun." (Vol. i, pp. 198-9.)

Gladly would we follow our author and his translator, who has enriched this part of the work with many valuable notes, through the subjects of earthquakes, gaseous emanations, hot springs, salses, and volcanoes; but there is too much before us to admit of our delay, and we must therefore restrict ourselves to a few remarks on the chemical and physical characters of volcanic and quasi-volcanic products. Various substances have been ejected during earthquakes, as, for instance, hot water, hot steam, irrespirable gases, causing the death of flocks grazing in the neighbourhood, flames, and columns of smoke.

> "At the destruction of Riobamba, in the year 1797, when the shocks were not attended by any outbreak of the neighbouring volcano, a singular mass, called the *Moya*, was uplifted from the earth in numerous continuous conical elevations, the whole being composed of carbon, crystals of augite, and the silicious shields of infusoria." (Vol. i, p. 209.)

There are places on the earth's surface, even where no volcanic rocks are visible on the surface, in which carbonic acid unmixed with nitrogen, carburetted hydrogen, sulphuretted hydrogen, and sulphurous vapour, and more rarely sulphurous and hydrochloric acids, issue from clefts or fissures. The carburetted hydrogen escaping from the earth in this manner has been used for the purposes of cookery and illumination in the Chinese province of Sse-tschuan for *several thousand years*. In a note by the translator we find an abstract of the recent researches of Dumas, which, in a geological point of view, are of the highest interest, as accounting for the vast deposits of sulphur associated with sulphates which are met with in the western part of Sicily, and for the abundant exhalation of sulphuric acid, which has been noticed in several (but chiefly in extinct) volcanoes. His experiments distinctly prove that when sulphuretted hydrogen (a gas which predominates in dormant volcanoes, while sulphurous acid is chiefly developed from volcanoes possessing a certain degree of activity), at a temperature above 100° F., and still better when near 190°, comes in contact with certain porous bodies, a catalytic action is set up, the products of which are water, sulphuric acid, and sulphur.

Exhalations of carbonic acid (*mofettes*) are, even at the present day, the most frequent and abundant of all gaseous emanations. In various parts of Germany, exhalations of this gas manifest themselves as the last efforts of the volcanic activity of an earlier world. In those remote periods, when a higher terrestrial activity existed, and when a far greater number of fissures remained unclosed, the processes now all but dormant advanced with energetic intensity, and carbonic acid and hot steam were propelled into the atmosphere in enormous volumes, giving rise to the abundant and luxuriant vegetation, of which our coal-beds are the irrefragable evidence. This was the period when the interior of our earth gave off those enormous quantities of carbon which exist in our limestone rocks, and which, it has been calculated, if separated from the oxygen with which it is chemically combined, and reduced to a solid form, would constitute about an eighth part of the bulk of those rocks.

In examining the products of volcanic action, we must not be misled by the inclosed fragments of rocks that are broken through; thus, in the black augitic lava of the volcano of Jorullo, Humboldt has found angular fragments of feldspathic syenite, whose presence can only be explained in this way. Four metals are usually found in the products of existing volcanoes, namely, iron, copper, lead, and arsenic. In the crater of *Volcano* (one of the islands of the Lipari group) "Stromeyer detected the presence of selenium in a mixture of sal ammoniac and sulphur. Another product supposed to be peculiar to this volcano is boracic acid, which lines the sides of the cavities in beautiful white silky crystals." (Translator's note, vol. i, p. 232.)

After a mere reference to the valuable investigations of Rose and Abich on the structural relations of the various kinds of volcanic rocks, Humboldt proceeds to the consideration of the very natural questions—

> "What is it that burns in the volcano? what excites the heat, fuses together earths and metals, and imparts to lava-currents of thick layers a degree of heat that lasts for many years? It is necessarily implied that volcanoes must be connected with the existence of substances capable of maintaining combustion, like the beds of coal in subterranean fires. According to the different phases of chemical science, bitumen, pyrites, the moist admixture of finely pulverized sulphur

and iron, pyrophoric substances, and the metals of the alkalies and earths, have in turn been designated as the cause of intensely active volcanic phenomena. . . . . . So general, deep-seated, and far-propagated an activity as that of volcanoes, cannot assuredly have its source in chemical affinity, or in the mere contact of individual or merely locally distributed substances. Modern geognosy rather seeks the cause of this activity in the increased temperature with the increase of depth at all degrees of latitude, in that powerful internal heat which our planet owes to its first solidification, its formation in the regions of space, and to the spherical contraction of matter revolving elliptically in a gaseous condition. We have thus mere conjecture and supposition side by side with certain knowledge. A philosophical study of nature strives ever to elevate itself above the narrow requirements of mere natural description, and does not consist, as we have already remarked, in the mere accumulation of isolated facts. The inquiring and active spirit of man must be suffered to pass from the present to the past, to conjecture all that cannot yet be known with certainty, and still to dwell with pleasure on the ancient myths of geognosy which are presented to us under so many various forms." (Vol. i, pp. 233-5.)

Volcanic action is no longer considered, as in times past, simply as an isolated phenomenon—as the disturbance of the earth's crust by a subterranean force; it is now regarded in a more general and comprehensive point of view, as *forming new rocks, and transforming those that already exist.* Here, then, to adopt the words of the author of the COSMOS—

"We arrive at the point at which a well-grounded study of the activity of volcanoes, whether igneous or merely such as emit gaseous exhalations, leads us, on the one hand, to the mineralogical branch of geognosy (the science of the texture and the succession of terrestrial strata), and on the other, to the science of geographical forms and outlines—the configuration of continents and insular groups elevated above the level of the sea. This extended insight into the connexion of natural phenomena, is the result of the philosophical direction which has been so generally assumed by the more earnest study of geognosy. Increased cultivation of science, and enlargement of political views, alike tend to unite elements that had long been divided." (Vol. i, p. 247.)

We cannot follow our author at any length through his remarks on rocks, which he arranges according to their mode of origin, into four classes, namely, rocks of eruption, sedimentary rocks, transformed or metamorphic rocks, and conglomerates. The process of metamorphism (a term first introduced into geology by Sir Charles Lyell) is one of extreme interest, but involved in much obscurity. Considerable light has been recently thrown upon it by the labours of Studer, which are referred to in one of the notes with which the translator has enriched the English edition. Various examples and cases of metamorphism are given by our author and the translator. The laboratory researches of Rose and Mitscherlich on the effect of various temperatures and different degrees of pressure on carbonate of lime, sulphur, &c., and those on a larger scale by Sir James Hall, Gregory, Watt, Bischof, &c. are noticed, as throwing a considerable degree of light on the geological process of metamorphism. Various cases actually observed in nature are then referred to; as the conversion of schist, by the vicinity of plutonic erupted rocks, into a bluish-black roofing-slate, the planes of stratification being intersected by another system of cleavage nearly at right angles with the former, and thus indicating a change subsequent to the primary arrangement; the conversion of argillaceous schist into a mass resembling granite, and consisting of a mixture of feldspar and mica, by the contact and plutonic action of granite; the conversion of calcareous sand-stone into the finest statuary marble, and the conversion of limestone into gypsum. He likewise notices, as amongst the most remarkable phenomena of contact, the formation of garnets in argillaceous schist, in juxtaposition with basalt and dolerite (as in Northumberland and the Isle of Anglesea), and the occurrence of a vast number of beautiful specimens of various kinds of crystals, as garnets, vesuvian, augite and ceylanite, on the surfaces of contact between erupted and sedimentary rocks, as at the junction of the syenite of Mouzon with dolomite and compact limestone. We add the following argument in favour of the doctrine of metamorphism, as applied to geological changes.

"All the facts connected with our geological hypotheses on the formation of the earth's crust, and the metamorphism of rocks, have been unexpectedly elucidated by the ingenious idea, which led to a comparison of the slags or scoriæ of our smelting furnaces with natural minerals, and to the attempt of reproducing the latter from their elements. In all these operations, the same affinities manifest themselves which determine chemical combinations, both in our laboratories and in the interior of the earth. The most considerable part of the simple minerals which characterise the more generally diffused plutonic and erupted rocks, as well as those on which they have exercised a metamorphic action, have been produced in a crystalline state, and with perfect identity, in artificial mineral products. We must, however, distinguish here between the scoriæ accidentally formed, and those which have been designedly produced by chemists. To the former belong feldspar, mica, augite, olivine, hornblende, crystallized oxide of iron, magnetic iron in octahedral crystals, and metallic titanium; to the latter, garnets, idocrase, rubies (equal in hardness to those found in the East), olivine, and augite. These minerals constitute the main constituents of granite, gneiss, and mica schist, of basalt, dolerite, and many porphyries. The artificial production of feldspar and mica is of most especial geognostic importance, with reference to the theory of the formation of gneiss by the metamorphic agency of argillaceous schist, which contains all the constituents of granite, potash not excepted. It would not be very surprising, therefore, as is well observed by the distinguished geognosist Von Dechen, if we were to meet with a fragment of gneiss formed on the walls of a smelting furnace, which was built of argillaceous slate and graywacke." (Vol. i, pp. 269-70.)

It is impossible to overrate the importance of the class of investigations referred to in the above quotation. Much has been done in this department since the publication of the COSMOS, especially by Ebelmen, in the 'Annales de Chimie et de Physique,' for the year 1847, and by Percy and Miller, in their valuable 'Report on Crystalline Slags.' These researches are alluded to in a note furnished by Dr. Percy to the translator.

The order of succession and the relative age of different systems of rocks next claim our attention. These are most certainly determined by the presence of organic remains within them, and by the distinguishing characters of those remains, in short, by the science of *Palæontology.*

"The application of botanical and zoological evidence to determine the relative age of rocks—this chronometry of the earth's surface which was already present to the lofty mind of Hooke—indicates one of the most glorious epochs of modern geognosy, which has finally, on the Continent, at least, been emancipated from the sway of Semitic doctrines. Palæontological investigations have imparted a vivifying breath of grace and diversity to the science of the solid structure of the earth.

"The fossiliferous strata contain, entombed within them, the floras and faunas of bygone ages. We ascend the stream of time, as in our study of the relations of super-position we descend deeper and deeper through the different strata, in which lies revealed before us a past world of animal and vegetable life. Far-extending

> disturbances, the elevation of great mountain chains, whose relative ages we are able to define, attest the destruction of ancient, and the manifestation of recent organisms. A few of these older structures have remained in the midst of more recent species. Owing to the limited nature of our knowledge of existence, and from the figurative terms by which we seek to hide our ignorance, we apply the appellation *recent structure* to the historical phenomena of transition manifested in the organisms, as well as in the forms of primitive seas, and of elevated lands. In some cases these organized structures have been preserved perfect in the minutest details of tissues, integument, and articulated parts; whilst, in others, the animal passing over soft argillaceous mud, has left nothing but the traces of its course, or the remains of its undigested food, as in the coprolites. In the lower Jura formations (the lias of Lyme Regis), the ink-bag of the sepia has been so wonderfully preserved, that the material, which myriads of years ago might have served the animal to conceal itself from its enemies, still yields the colour with which its image may be drawn. In other strata, again, nothing remains but the faint impression of a mussel-shell; but even this, if it belong to a main division of mollusca, may serve to show the traveller in some distant land, the nature of the rock in which it is found, and the organic remains with which it is associated. Its discovery gives the history of the country in which it occurs." (Vol. i, pp. 272-4.)

This quotation is followed by a sketch of the order of succession of the various forms of organic life, which have existed on our own planet in its earlier phases, and which have been revealed to us by modern geological and palæontological research. Much do we wish that we could transfer it to our pages; for although, from the very nature of the COSMOS, it is in the highest degree condensed, it is exceedingly luminous and well arranged. The subject is also well and more fully discussed in Cotta's twenty-fourth letter.

From geology we pass to physical geography. The *horizontal configuration* of the continent is one of the first points noticed. The subject is one which in the remote periods of antiquity attracted the attention of Dicæarchus, Eratosthenes, and Strabo, and which has been made a matter for discussion by numerous comparatively recent writers. The following extract contains, to say the least, a singular collection of facts on this subject, and many curious analogies of form existing in remote portions of the globe, from which, however, we are perhaps hardly in a position to attempt to deduce laws.

> "As we have already remarked, one hemisphere of the earth (whether we divide the sphere through the equator or through the meridian of Teneriffe) has a much greater expansion of elevated land than the opposite one: these two vast ocean-girt tracts of land, which we term the eastern and western, or the old and new continents, present, however, conjointly with the most striking contrasts of configuration and position of their axes, some similarities of form, especially with reference to the mutual relations of their opposite coasts. In the eastern continent, the predominating direction—the position of the major axis—inclines from east to west (or more correctly speaking from south-west to north-east), whilst in the western continent it inclines from south to north (or rather from south south-east to north north-west). Both terminate to the north at a parallel coinciding nearly with that of 70°; whilst they extend to the south in pyramidal points, having submarine prolongations of islands and shoals. Such, for instance, are the Archipelago of Tierra del Fuego, the Lagullas Bank south of the Cape of Good Hope, and Van Diemen's Land, separated from New Holland by Bass's Straits. Northern Asia extends to the above parallel at Cape Taimura, which, according to Krusenstern, is 78° 16′, whilst it falls below it from the mouth of the Great Tschukotschja River eastward to Behring's Straits, in the eastern extremity of Asia—Cook's East Cape—which,

according to Beechey, is only 66° 3′. The northern shore of the New Continent follows with tolerable exactness the parallel of 70°, since the lands to the north and south of Barrow's Strait, from Boothia Felix and Victoria Land, are merely detached islands.

"The pyramidal configuration of all the southern extremities of continents belongs to the *similitudines physicæ in configuratione Mundi*, to which Bacon already called attention in his 'Novum Organon,' and with which Rienhold Foster, one of Cook's companions, in his second voyage of circumnavigation, connected some ingenious considerations. On looking eastward from the meridian of Teneriffe, we perceive that the southern extremities of the three continents, viz. Africa as the extreme of the Old World, Australia, and South America, successively approach nearer towards the South Pole. New Zealand, whose length extends fully 12° of latitude, forms an intermediate link between Australia and South America, likewise terminating in an island, New Leinster. It is also a remarkable circumstance, that the greatest extension towards the south falls in the Old Continent, under the same meridian in which the extremest projection towards the North Pole is manifested. This will be perceived on comparing the Cape of Good Hope and the Lagullas Bank with the North Cape of Europe, and the Peninsula of Malacca with Cape Taimura in Siberia. We know not whether the poles of the earth are surrounded by land or by a sea of ice. Towards the North Pole the parallel of 82° 55′ has been reached, but towards the South Pole only that of 78° 10′.

"The pyramidal terminations of the great continents are variously repeated on a smaller scale, not only in the Indian Ocean, and in the Peninsulas of Arabia, Hindostan, and Malacca, but also, as was remarked by Eratosthenes and Polybius, in the Mediterranean, where these writers had ingeniously compared together the forms of the Iberian, Italian, and Hellenic peninsulas. Europe, whose area is five times smaller than that of Asia, may almost be regarded as a multifariously articulated western peninsula of the more compact mass of the continent of Asia, the climatic relations of the former being to those of the latter as the Peninsula of Brittany is to the rest of France. The influence exercised by the articulation and higher development of the form of a continent on the moral and intellectual condition of nations was remarked by Strabo, who extols the varied form of our small continent as a special advantage. Africa and South America, which manifest so great a resemblance in their configuration, are also the two continents that exhibit the simplest littoral outlines. It is only the eastern shores of Asia, which, broken as it were by the force of the currents of the ocean (*fractas ex æquore terras*), exhibit a richly variegated configuration, peninsulas and contiguous islands alternating from the equator to 60° north latitude.

"Our Atlantic Ocean presents all the indications of a valley. It is as if a flow of eddying waters had been directed first towards the north-east, then towards the north-west, and back again to the north-east. The parallelism of the coasts north of 10° south latitude, the projecting and receding angles, the convexity of Brazil opposite to the Gulf of Guinea, that of Africa under the same parallel with the Gulf of the Antilles, all favour this apparently speculative view. In this Atlantic valley, as is almost everywhere the case in the configuration of large continental masses, coasts deeply indented, and rich in islands, are situated opposite to those possessing a different character. I long since drew attention to the geognostic importance of entering into a comparison of the western coasts of Africa and of South America within the tropics. The deeply curved indentation of the African continent at Fernando Po, 4° 30′ north latitude, is repeated on the coast of the Pacific at 18° 15′ south latitude, between the Valley of Arica and the Morro de Juan Diaz, where the Peruvian coast suddenly changes the direction from south to north which it had previously followed, and inclines to the north-west. This change of direction extends in like manner to the chain of the Andes, which is divided into two parallel branches, affecting not only the littoral portions, but even the eastern Cordilleras. In the latter, civilization had its earliest seat in the South American plateaux, where the small alpine lake of Titicaca bathes the feet of the colossal mountains of

Sorata and Illimani. Further to the south, from Valdivia and Chiloë (40° to 42° south latitude), through the Archipelago *de los Chonos* to *Terra del Fuego*, we find repeated that singular configuration of *fiords* (a blending of narrow and deeply indented bays), which, in the Northern Hemisphere, characterises the western shores of Norway and Scotland." (Vol. i, pp. 293-7.)

From the *external* configuration of continents, considered in reference to their horizontal expansions, Humboldt proceeds to notice their *internal* articulation, or the vertical elevation of their soils. "Whatever," he observes, "produces a polymorphic diversity of forms on the surface of our planetary habitation—such as mountains, lakes, grassy savannas, or even deserts encircled by a band of forests—impresses some peculiar character on the social condition of the inhabitants." Then a blending of low discontinuous mountain chains and valleys gives rise to various forms of climate, and to an augmented number of vegetable growths; and thus, in a secondary degree, "gives rise to wants that stimulate the activity of the inhabitants."

The analogies and contrasts between the aqueous and aerial envelopes of the solid surface of our planet are then noticed. While the contrasts are chiefly found in the great difference in their conditions of aggregation and elasticity, there are well-marked analogies in the mobility of their particles and in their currents. The rapid decrease of the temperature of the lower strata of the ocean is noticed as a wise arrangement of nature to enable fishes to go down to get a little cool water, in much the same way as in hot weather we seek an elevated region for its coolness. This view has been recently opposed with much vigour by Dr. Williams, of Swansea, who maintains that the extreme pressure must render these cool strata of water altogether unfit for animal existence. There are many points connected with the ocean, more or less fully noticed in these pages, to which we would direct the attention of our readers; but our limited space warns us that we cannot do more than enumerate a few of the most important. Foremost amongst them we must place the position of the zones of the hottest water, and of that having the greatest saline contents. The waters of the highest temperature appear to form two nearly parallel lines, north and south of the geographical equator. Lenz ascertained that in the Pacific the maxima of density occurred in 22° north, and 17° south latitude, while the minimum was situated a few degrees to the south of the equator. The tides are then considered in a very brief and, we think, unsatisfactory manner; and it is singular that neither in Cotta's Letters, nor in the Translator's notes, is there any reference to the researches of Airy or Whewell. Oceanic currents come in for their due share of attention. Amongst their causes, he observes, we must reckon the period at which tides occur, in their progress round the earth; the duration and intensity of prevailing winds; the modifications of density and specific gravity which the particles of water undergo, in consequence of differences in the temperature, and in the relative quantity of saline contents at different latitudes and depths; and, lastly, the horary variations of the atmospheric pressure, to which we shall presently more specially advert. These currents present the remarkable spectacle of crossing the ocean in various directions, like rivers of uniform breadth, the adjacent strata of water remaining undisturbed, and forming, as it were, the banks of these moving streams. The difference between the moving waters and those at rest is most obvious where long lines of sea-weed are carried onward by the current, and thus enable us to estimate its velocity.

The general movement of the sea from east to west (the so-called equatorial or rotation current) is supposed to be the result of the joint action of the tidal motion and the trade winds. The velocity of this current, as ascertained by the experiments of Humboldt, and the more recent ones of Daussy, is about ten miles in the twenty-four hours. Christopher Columbus, one of the best observers who ever existed, when, during his third voyage, he was entering the tropics, in the meridian of Teneriffe, wrote in his journal as follows: "I regard it as proved that the waters of the sea move from east to west as do the heavens, that is to say, like the apparent motion of the sun, moon, and stars." He adds, shortly afterwards, that "the movement is strongest in the Caribbean sea," a statement fully borne out by Rennell, who terms this region "not a current, but a sea in motion."

The great use of these oceanic rivers is to convey warm water into higher, and cold water into lower latitudes. The great Atlantic gulf stream and its branches are too well known to require any description; our readers may, however, be less familiar with its counterpart, the cold Peruvian stream in the South Pacific, the effect of whose low temperature on the climate of the coast of Chili and Peru was observed by our author in 1802. At certain seasons of the year, the temperature of this cold oceanic current is, within the tropics, only 60°, while the undisturbed adjacent water exhibits a temperature varying from 81° to 84°.

The depth to which these currents of hot and cold water extend has not been ascertained with any degree of precision, but there are reasons for believing that it is very considerable. In connexion with the subject of the temperature of the ocean, we may notice the singular fact first discovered by Franklin, that sand-banks and shoals may be recognised by the coldness of the water over them. Sir Humphrey Davy ascribed this phenomenon to the descent of the particles of water that had been cooled by nocturnal radiation, and which remain nearer to the surface, owing to the hinderance placed in the way of their subsidence; while, according to Humboldt, the depression of temperature depends on the fact that, by the propagation of the motion of the sea, deep waters rise to the margin of the banks, and mix with the upper strata.

Mists are often found over these shoals, owing to the condensation of the vapour of the atmosphere by the cooled waters. These mists often define with sharpness and clearness the form of the shoals below them, appearing as the aerial reflection of the bottom of the sea.

We cannot refrain from transferring to our pages the following exquisitely beautiful passage on the universal diffusion of life throughout the waters of our globe.

"Although the surface of the ocean is less rich in living forms than that of continents, it is not improbable that, on a further investigation of its depths, its interior may be found to possess a greater richness of organic life than any other portion of our planet. Charles Darwin, in the agreeable narrative of his extensive voyages, justly remarks that our forests do not conceal so many animals as the low woody regions of the ocean, where the sea-weed rooted to the bottom of the shoals, and the severed branches of fuci loosened by the force of the waves and currents, and swimming free, unfold their delicate foliage upborne by air-cells. The application of the microscope increases, in the most striking manner, our impression of the rich luxuriance of animal life in the ocean, and reveals to the astonished senses a

> consciousness of the universality of life. In the oceanic depths, far exceeding the height of our loftiest mountain chains, every stratum of water is animated with polygastric sea worms, cyclidiæ, and ophrydinæ. The waters swarm with countless hosts of small luminiferous animalcules, mammaria (of the order of acalephæ), crustacea, peridinea, and circling nereides, which, when attracted to the surface by peculiar meteorological conditions, convert every wave into a foaming band of flashing light.
>
> "The abundance of these marine animalcules, and the animal matter yielded by their rapid decomposition, are so vast, that the sea water itself becomes a nutrient fluid to many of the larger animals. However much this richness in animated forms, and this multitude of the most various and highly-developed microscopic organisms, may agreeably excite the fancy, the imagination is even more seriously, and I might say more solemnly, moved by the impression of boundlessness and immeasurability, which are presented to the mind by every sea voyage. All who possess an ordinary degree of mental activity, and delight to create to themselves an inner world of thought, must be penetrated with the sublime image of the infinite, when gazing around them on the vast and boundless sea, when involuntarily the glance is attracted to the distant horizon, where air and water blend together, and the stars continually rise and set before the eyes of the mariner. This contemplation of the eternal play of the elements is clouded, like every human joy, by a touch of sadness and of longing." (Vol. i, pp. 314-5.)

But we leave the aqueous for the aerial ocean—the atmosphere surrounding our planet. The full consideration of this subject constitutes the science of meteorology—a science of which our author has been one of the most successful cultivators.

The first point noticed is the chemical constitution of the atmosphere. Its chief constituents are oxygen, nitrogen, carbonic acid, and aqueous vapour. While various circumstances modify the proportions of the two latter, the two former seem invariably to exist in the same ratio to each other. The constancy of this ratio was first ascertained by our author and his friend Gay-Lussac in the year 1804. In twenty-nine experiments made on twenty-nine different days, during the months of November and December, in dry as well as in moist or wet weather, and during different winds, the air of Paris above the surface of the river yielded a per centage of oxygen varying from 21·2 to 20·9. This difference is so small, as fairly to rank as an error in the experiments. Since the constant composition of 21 of oxygen to 79 of nitrogen was then assigned to the atmosphere by these experimentalists, the accuracy of their results has been confirmed by most of the observers who have followed them.* As the subject is one of the highest interest, we shall notice it somewhat fully. Thirty years after the publication of the researches of Humboldt and Gay-Lussac, De Saussure made numerous experiments (in an entirely different manner) with air collected over the Lake of Geneva, and at Chambeisy. During various winds and different states of weather, the minimum of oxygen was 20·98, and the maximum 21·15, the mean being 21·05. Similar results have been yielded by air obtained by Gay-Lussac in a balloon excursion, from a height of 21,430 feet, and by Humboldt, from the Antisana, a mountain 16,640 feet in height. Moreover, there is strong evidence to show that impure air, after the removal and subtraction of its impurities and foreign ingredients, contains oxygen and nitrogen in the same constant ratio. Thus Configliachi examined the air collected over rice fields, Seguin, Gay-Lussac, and Humboldt, that from a crowded Parisian theatre, Edmund Davy that from the wards of a hospital, and De Saussure that of bedroom in the morning; and none of these specimens of air showed any variation in the amount of oxygen and nitrogen, the foreign ingredients being previously removed.

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* The numbers 20·8 of oxygen and 79·2 of nitrogen are given by Boussingault, and may be fairly regarded as representing as accurately as possible the true ratio of the two gases.

On the other hand, there are some trustworthy chemists who maintain that the proportions of oxygen and nitrogen are liable to change within certain limits. Thus Levy found in the air of Guadaloupe that the difference between the maximum and minimum of oxygen in seven experiments amounted to 0·46. Before referring to his explanation of the occasional augmentation of the oxygen, we may mention that M. Doyère has recently attempted to prove that there is a constant alteration in the composition of the atmosphere; or, in other words, that while volumes of air taken from the same flask present identical results, the air taken simultaneously at different places yields results which are too discordant to be regarded as depending on mere errors of manipulation. He has found the oxygen to vary from 20·5 to 21·5 in 100 parts of air. His assertion has led to a series of experiments instituted during the past year by MM. Regnault and Reiset, who have failed in detecting such marked differences. They analysed air taken from Paris, from its environs, from the south of France, from the sea side, and from Switzerland; and all their results lay between 20·85 and 21.

Levy attempted to refer the occasional excess of oxygen noticed in his experiments, to its evolution from certain microscopic organisms, and Morren has, to a certain extent, confirmed his views; for he found that air collected on the immediate surface of large ponds of sea-water covering much sea-weed, contained 23 per cent. of its volume of oxygen; and that the minute monads known as the *Enchelis monadina virescens sulesphœnea* of Bory de St. Vincent, mixed with the *Enchelis pulvisculus viridis* of Müller, and the *Monas bicolor* of Ehrenberg, disengage, when exposed to the sun's rays, very large quantities of this gas. He calculated that from a bulk of water of 8000 cubic feet, containing an abundance of the green matter which was shown by the microscope to consist of the above monads, no less than 128 cubic feet of oxygen were given off in a single day. A similar observation has been made by Wöhler. In the saline springs of of Kur-Hesse, a green mucilaginous matter is formed during the summer, from which innumerable air-bubbles are evolved. These bubbles contain 51 per cent. of oxygen and 49 of nitrogen. This green matter consists almost entirely of species of *Navicula* and *Gallionella* mixed with fibres of *Confervæ.* This inquiry has been yet further carried on by Morren and Levy in subsequent memoirs; and there can, we think, be no doubt regarding the general correctness of their views. The oxygen thus evolved must, however, so rapidly mix with the surrounding air, as to cause all sensible difference in its ratio almost instantaneously to disappear.

But besides these chief constituents of the atmosphere, it contains "from two to five ten-thousandth parts of carbonic acid gas, a still smaller quantity of carburetted hydrogen gas, and, according to the important experiments of Saussure and Liebig, traces of ammoniacal vapours from which plants derive their nitrogenous contents."

The carbonate of ammonia which has been detected by some observers in the atmosphere, is not dependent on the direct union of the carbonic acid and the ammoniacal vapour, but seems to be the result of a more complex chemical action. Nitrate of ammonia is occasionally produced under the influence of atmospheric electricity, and, reacting on the carbonate of lime, thus gives rise to carbonate of ammonia.

With regard to the carburetted hydrogen, we are doubtful how far our author is justified in regarding it as a constituent of the atmosphere. That hydrogen and carbon in some form or other, besides that of water and carbonic acid, do exist in the atmosphere, is proved by the experiments of Boussingault, Verver, and others; but nothing has been ascertained, or *can* be ascertained by those experiments, regarding the state in which they occur. Although the humidity of the atmosphere is noticed as a separate subject in a future page, some reference should have been made to the direct chemical experiments of Ververs and others, in his summary of the ordinary atmospheric constituents. But besides these substances—

> "There are others accidentally mixed with them, especially near the ground, which sometimes in the form of miasmatic and gaseous contagia, exercise a noxious influence on animal organization. Their chemical nature has not yet been ascertained by direct analysis; but from the consideration of the processes of decay which are perpetually going on in the animal and vegetable substances with which the surface of our planet is covered, and judging from analogies deduced from the domain of pathology, we are led to infer the existence of such noxious local admixtures. Ammoniacal and other nitrogenous vapours, sulphuretted hydrogen gas, and compounds analogous to the polybasic ternary and quaternary combinations of the vegetable kingdom, may produce miasmata, which under various forms may generate ague and typhus fever (not by any means exclusively on wet marshy ground, or on coasts covered by putrescent mollusca and low bushes of *Rhizophora mangle* and avicennia). Fogs, which have a peculiar smell at some seasons of the year, remind us of these accidental admixtures in the lower strata of the atmosphere. Winds and currents of air caused by the heating of the ground, even carry up to a considerable elevation solid substances reduced to a fine powder. The dust which darkens the air for an extended area and falls on the Cape Verd Islands, to which Darwin has drawn attention, contains, according to Ehrenberg's discovery, a host of siliceous shelled infusoria." (Vol. i, pp. 318-9.)

We must, however, proceed to the consideration of other points connected with the atmosphere, and next in order we shall take the *variations of atmospheric pressure*. It is to the patient and laborious investigations of Humboldt, that we are indebted for our knowledge of the fact of the horary oscillations of the barometer. These oscillations (which, like all diurnally periodic meteorological phenomena, are most simple, and can therefore be best noticed under the equator,) in the tropics present two maxima, viz. at 9 or 9¼ a.m, and 10½ or 10¾ p.m., and two minima at 4 or 4¼ p.m. and 4 a.m. (occurring therefore in almost the hottest and coldest hours.) The regularity of these oscillations is so great under the equator, that the hour may be ascertained from the height of the mercurial column, without an error, on an average, of more than fifteen minutes; and this ebb and flow of the aerial ocean is unaffected by storms, hurricanes, rain, and earthquakes. The total amount of a diurnal oscillation amounts to rather more than one tenth of an inch at the equator, and gradually diminishes as the latitude increases. Although we have used the expression "ebb and flow," there is no actual analogy between the causes giving rise to these oscillations and to the oceanic tides; for the phenomenon now under consideration is a compound one, resulting from two distinct diurnal oscillations, one taking place in the true aerial ocean, and arising from its alternate heating and cooling; the other arising in what we may term the aqueous atmosphere, by the alternate production of vapour by the heat of day, and its destruction during the cold of night. This separation of a compound movement into two single ones regulated by distinct laws, is due to the analytical genius and sagacity of Dove.

With regard to the local distribution of barometric pressure, it appears that the mean height of the mercurial column is greatest in western Europe between the parallels of 40° and 45°. Since the publication of the COSMOS, it has been shown by the observations of Ross that there is a general depression of the mercury to the *extent of an entire inch* over the whole antarctic ocean—a fact for which, as far as we are aware, no satisfactory explanation has been given.

Climatology is next treated of; and here we must again express our regret that we cannot follow our author through his masterly sketch of a department of physical inquiry, which through his investigations has been elevated to the rank of a distinct science. We must confine ourselves to the following striking remarks on the contrast between the insular and littoral climates enjoyed by all articulated continents, having deeply indented bays and peninsulas, and between the climate of the interior of great continental masses.

"This remarkable contrast has been fully developed by Leopold von Buch in all its various phenomena, with respect to its influence on vegetation and agriculture, on the transparency of the atmosphere, the radiation of the soil, and the elevation of the line of perpetual snow. In the interior of the Asiatic Continent, Tobolsk, Barnaul on the Oby, and Irkutsk, have the same mean summer heat as Berlin, Munster, and Cherbourg in Normandy; the thermometer sometimes remaining for weeks together at 86° or 88°, whilst the mean winter temperature is, during the coldest month, as low as — 0°·4 to — 4°. These continental climates have, therefore, justly been termed *excessive* by the great mathematician and physicist, Buffon; and the inhabitants who live in countries having such *excessive* climates seem almost condemned, as Dante expresses himself—

"A sofferir tormenti caldi e geli."

"In no portion of the earth, neither in the Canary Islands, in Spain, or in the south of France, have I ever seen more luxuriant fruit, especially grapes, than in Astrachan, near the shores of the Caspian Sea (46° 21′). Although the mean annual temperature is about 48°, the mean summer heat rises to 70°, as at Bordeaux, whilst not only there, but also further to the south, as at Kislar on the mouth of the Terek (in the latitude of Avignon and Rimini), the thermometer sinks in the winter to — 13° or — 22°.

"Ireland, Guernsey, and Jersey, the peninsula of Brittany, the coasts of Normandy, and of the south of England, present, by the mildness of their winters, and by the low temperature and clouded sky of their summers, the most striking contrast to the continental climate of the interior of Eastern Europe. In the north-east of Ireland (54° 56′), lying under the same parallel of latitude as Königsberg in Prussia, the myrtle blooms as luxuriantly as in Portugal. The mean temperature of the month of August, which in Hungary rises to 70°, scarcely reaches 61° at Dublin, which is situated on the same isothermal line of 49°; the mean winter temperature, which falls to about 28° at Pesth, is 40° at Dublin, (whose mean annual temperature is not more than 49°); ·3°·6 higher than that of Milan, Pavia, Padua, and the whole of Lombardy, where the mean annual temperature is upwards of 55°. At Stromness, in the Orkneys, scarcely half a degree further south than Stockholm, the winter temperature is 39°, and consequently higher than that of Paris, and nearly as high as that of London. Even in the Faroe Islands, at 62° latitude, the inland waters never freeze, owing to the favoring influence of the west winds and of the sea. On the charming coasts of Devonshire, near Salcombe Bay, which has been termed, on account of the mildness of its climate, the *Montpellier of the North*, the Agave Mexicana has been seen to blossom in the open air, whilst orange trees trained against espaliers and only slightly protected by matting, are found to bear fruit. There, as well as at Penzance and Gosport, and at Cherbourg on the coast of Normandy, the mean winter temperature exceeds 42°, falling short by only 2°·4 of the mean winter temperature of Montpellier and Florence. These observations will suffice to show the important influence exercised on vegetation and agriculture, on the cultivation of fruit, and on the comfort of mankind, by differences in the distribution of the same mean annual temperature, through the different seasons of the year." (Vol. i, pp. 328-30.)

On the above grounds he proceeds to demonstrate the reasons why the temperature of Europe is more moderate than that of Asia, of which, as he observes, the former may be considered to constitute the western peninsula.

"Europe owes its milder climate, in the first place, to its position with respect to Africa, whose wide extent of tropical land is favorable to the ascending current, while the equatorial region to the south of Asia is almost wholly oceanic; and next to its deeply-articulated configuration, to the vicinity of the ocean on its western shores; and lastly, to the existence of an open sea, which bounds its northern confines. Europe would, therefore, become colder if Africa were to be overflowed by the ocean; or if the mythical Atlantis were to arise and connect Europe with North America; or if the gulf stream were no longer to diffuse the warming influence of its waters into the North Sea; or if, finally, another mass of solid land should be upheaved by volcanic action, and interposed between the Scandinavian peninsula and Spitzbergen. If we observe that in Europe the mean annual temperature falls, as we proceed, from west to east, under the same parallel of latitude, from the Atlantic shores of France, through Germany, Poland, and Russia, towards the Uralian mountains, the main cause of this phenomenon of increasing cold must be sought in the form of the continent, (which becomes less indented, and wider, and more compact as we advance,) in the increasing distance from seas, and in the diminished influence of westerly winds. Beyond the Uralian mountains these winds are converted into cool land-winds, blowing over extended tracts covered with ice and snow. The cold of western Siberia is to be ascribed to these relations of configuration and atmospheric currents, and not—as Hippocrates and Trogus Pompeius, and even celebrated travellers of the eighteenth century conjectured—to the great elevation of the soil above the level of the sea." (Vol. i, pp. 332-3.)

Passing without remark over the general subject of *Hygrometry*, we have a few words to say on the *electric condition of the atmosphere*. In an early part of the present article we have referred to the evidence which seems to connect atmospheric electricity with the phenomena of the distribution of heat, of the pressure of the atmosphere, of hydrometeoric exhibitions, and of terrestrial magnetism. We shall not enter into this subject on this occasion, and we shall follow our author's example in offering no opinion on the actual source of atmospheric electricity under a clear sky. The point to which we would direct attention, has reference to the production in the atmosphere of that mysterious imponderable, *ozone*. It is a long recognised fact, that when a body is struck by lightning, there is almost always developed a strong and pungent odour, which some have compared with that of burning sulphur; whilst others have thought that it rather resembled the smell of phosphorus. Without entering into the chemical peculiarities of ozone, we may mention the following experiment made a few years ago by MM. de la Rive and Marignac. "A series of electric sparks was passed by means of two platinum points through a tube containing dry and pure oxygen. The oxygen immediately assumed the properties of ozone, acquiring its penetrating and nauseous odour, its power of liberating iodine from iodide of potassium, &c." (See British and Foreign Medico-Chirurgical Review, Vol. i, p. 542.) This is a mere repetition in the laboratory, of the great experiment performed in the atmosphere, and it is more than probable that this occasional production of ozone, which is the most powerfully bleaching and oxidising matter in nature, disinfects the air from numerous miasmata, thus affording a scientific explanation of the long popular idea that thunderstorms purify the atmosphere. The reference to Vol. i, made above, will show that there are good reasons for believing that an excessive development of ozone in the atmosphere may give rise to epidemic catarrh.

We approach the close of the first volume. A physical cosmography would be incomplete without a glance at the wonders of organic life. Recent observations show that while vegetable life, which is so dependent on the action of the solar rays, is most prevalent on land,—animal life predominates amid the eternal night of the depths of ocean. Ehrenberg's brilliant discovery regarding "microscopic life in the ocean, and in the ice of the polar region," is here noticed :—

"'Not only in the polar regions is there an uninterrupted development of active microscopic life, where larger animals can no longer exist, but we find that the microscopic animals collected in the antarctic expedition of Captain James Ross, exhibit a remarkable abundance of unknown and often most beautiful forms. Even in the residuum obtained from the melted ice, swimming about in round fragments, in the latitude of 70° 10′, there were found upwards of fifty species of siliceous-shelled polygastria and coscinodiscæ with their green ovaries, and therefore living and able to resist the extreme severity of the cold. In the Gulf of Erebus, sixty-eight siliceous-shelled polygastria and phytolitharia, and only one calcareous-shelled polythalamia, were brought up by lead sunk to a depth of from 1242 to 1620 feet.'

"The greater number of the oceanic microscopic forms hitherto discovered have been siliceous-shelled, although the analysis of sea water does not yield silica as the main constituent, and it can only be imagined to exist in it in a state of suspension. It is not only at particular points in inland seas, or in the vicinity of the land, that the ocean is densely inhabited by living atoms, invisible to the naked eye; but samples of water taken up by Schayer on his return from Van Diemen's Land (south of the Cape of Good Hope, in 57° latitude, and under the tropics in the Atlantic), show that the ocean, in its ordinary condition, without any apparent discoloration, contains numerous microscopic moving organisms, which bear no resemblance to the swimming fragmentary siliceous filaments of the genus chætoceros, similar to the oscillatoriæ so common in our fresh waters. Some few polygastria which have been found mixed with sand and excrements of penguins in Cockburn Island, appear to be spread over the whole earth, whilst others seem to be peculiar to the polar regions." (Vol. i, p. 351.)

The following pages are devoted to the consideration of the geographical distribution of plants and animals (and here we must remark that the translator has judiciously introduced the recent philosophic views of Professor Edward Forbes, on this subject); and after a cursory glance at the *quæstio vexata* regarding the modes of origin of organised beings, the volume terminates with a brief notice of the natural history of MAN. This subject was, however, so fully considered in a comparatively recent volume of one of our predecessors (the last volume of the British and

Foreign Medical Review), that it is sufficient to remark that our author agrees with Prichard in maintaining the unity of the human species.

With the second volume of the COSMOS, we shall deal very briefly. It is divided into two great parts, the first being an essay, or rather a series of essays, on the 'Incitements to the Study of Nature,' whilst the latter is devoted to the 'History of the Physical Contemplation of the Universe.'

The incitements which promote the study and contemplation of nature are, according to our author, of three distinct kinds:—1, the poetic and vivid description of nature and natural scenery; 2, landscape painting, especially where it has caught the characteristic features of the animal and vegetable world; and 3, the more widely diffused cultivation of tropical plants, and the more strongly contrasted opposition of exotic and indigenous forms.

It is difficult to speak in suitable terms of the elevated views and profound scholarship of these essays. In reference to poetic descriptions of nature, Humboldt observes, that it has been his endeavour "to manifest in a fragmentary manner the different influence exercised by the external world on the aspect of animate and inanimate nature at different periods of time, on the thoughts and mode of feeling of different races;" and with this view he has collected examples from the literature of all ages and nations—from the earliest writings of the Greeks, Romans, and Indians, down to our own time. Landscape painting, in its animating influence on the study of nature, is traced from its earliest germs to its most brilliant period, the seventeenth century; and the importance of a more extended cultivation of exotic plants is feelingly illustrated by a reference to his own youthful experience. It was the sight of a colossal dragon-tree and of a fan-palm in an old tower of the botanical garden at Berlin, that first implanted in his mind the germ of an irresistible desire to undertake distant travels. "He who is able to trace through the whole course of his impressions that which gave the first leading direction to his whole career, will not deny the influence of such a power." But we must refer our readers to the volumes themselves for his various historical illustrations of the general accuracy of his views, and will merely add his concluding remark, that—

> "We may regard it as one of the most precious fruits of European civilization, that it is almost everywhere permitted to man, by the cultivation and arrangement of exotic plants, by the charm of landscape painting, and by the inspired power of language, to procure a substitute for familiar scenes during the period of absence, or to receive a portion of that enjoyment from nature which is yielded by actual contemplation during long and not unfrequently dangerous journeys through the interior of distant continents."

The *history of the physical contemplation of the universe* may be regarded in the light of a grand, and, we may add, a successful attempt at a *history of objective discovery*—a history of those epochs or momenta in the progress of scientific knowledge which have advanced it to its present state; and must not be confounded with the history of the physical sciences. To adopt our author's words, the history of the physical contemplation of the universe is the history of the recognition of the unity of nature, the representation of the efforts made by man to comprehend the combined action of natural forces on the earth and in the regions of space; and hence it designates the epochs of advancement in the generalization of views; being a portion of the history of our world of thought, in as far as it refers to objects manifested by the senses, to the form of conglomerated matter, and to the forces inherent in it. The following extract more definitively shows his exact aim:

> "In order to give some idea of what has been collected at separate epochs under this point of view, it appears most desirable to adduce separate instances, illustrative of the subjects which must either be treated of or discarded in the succeeding portions of this work. The discoveries of the compound microscope, of the telescope, and of coloured polarization, belong to the history of the Cosmos, since they have afforded the means of discovering that which is common to all organisms; of penetrating into the remotest regions of space; of distinguishing between reflected or borrowed light and the light of self-luminous bodies, or, in other words, determining whether solar light be radiated from a solid mass or from a gaseous envelope. The enumeration of the experiments which, since Huygen's time, have gradually led to Arago's discovery of coloured polarization, must be reserved for the history of optics. The consideration of the development of the principles, in accordance with which variously-formed plants admit of being classified in families, falls in like manner within the domain of phytognosy or botany; whilst the geography of plants, or a study of the local and climatic distribution of vegetation over the whole earth,—alike over the solid portions and in the basins of the sea,—constitutes an important section in the history of the physical contemplation of the universe." (Vol. ii, pp. 466-7.)

The principal momenta in the history of a physical contemplation of the universe, as laid down by our author, are the following:

I. The basin of the Mediterranean considered as the starting point of the attempts to extend the idea of the Cosmos.

II. The campaigns of the Macedonians under Alexander the Great, and the long enduring influence of the Bactrian empire.

III. The increase of the contemplation of the universe under the Ptolemies.

IV. The influence of the universal dominion of the Romans, and of their empire, on the extension of cosmical views.

V. The irruption of the Arabs, and the effects of the foreign elements thus introduced on the development of European culture.

VI. The period of oceanic discoveries, especially the discovery of the western hemisphere.

And VII. The period of great discoveries in the regions of space, by the application of the telescope.

But on these subjects, treated as no man save Alexander von Humboldt could treat them, we must not dwell. As an antiquary, a historian, and a man of science, his strength seems equally gigantic, and he has brought all his forces to bear on the consideration of these points.

A brief retrospect of these successive epochs, collectively embracing a period of upwards of two thousand years, from the time of the earliest traces of civilization amongst the nations dwelling around the basin of the Mediterranean and the fruitful river valleys of western Asia, to the commencement of the last century, when general views and feelings were already beginning to blend with those of the present age, terminates with a glance at the increasing difficulty of separating the branches of science, in consequence of the multiplicity of the links of connexion between them, and with the following remarks on the characteristic *genius* of our own times:

"That which has especially favoured the progress of knowledge in the nineteenth century, and imparted to the age its principal character, is the general and beneficial endeavour not to limit our attention to that which has been recently acquired, but to test strictly, by measure and weight, all earlier acquisitions; to separate certain knowledge from mere conjectures founded on analogy, and thus to subject all portions of knowledge, whether it be physical astronomy, the study of terrestrial natural forces, geology, or archæology, to the same strict method of criticism. The generalization of this course has, most especially, contributed to show on each occasion the limits of the separate sciences, and to discover the weakness of certain studies in which unfounded opinions take the place of certain facts, and symbolical myths manifest themselves under ancient semblances as grave theories. Indefiniteness of language, and the transference of the nomenclature of one science to another, have led to erroneous views and delusive analogies. The advance of zoology was long endangered from the belief that, in the lower classes of animals, all vital actions were attached to organs similarly formed to those of the higher classes. The knowledge of the history of the development of plants in the so-called Cryptogamic Cormophytes (mosses and liverworts, ferns, and lycopodiaceæ), or in the still lower Thallophytes (algæ, lichens, and fungi), has been still more obscured by the supposed discovery of analogies with the sexual propagation of the animal kingdom.

"If art may be said to dwell within the magic circle of the imagination, the extension of knowledge, on the other hand, depends especially on contact with the external world, and this becomes more manifold and close in proportion with the increase of general intercourse. The creation of new organs (instruments of observation) increases the intellectual and not unfrequently the physical powers of man. More rapid than light, the closed electric current conveys thought and will to the remotest distance. Forces, whose silent operation in elementary nature, and in the delicate cells of organic tissues, still escapes our senses, will, when recognised, employed and awakened to higher activity, at some future time, enter within the sphere of the endless chain of means which enable man to subject to his control separate domains of nature, and to approximate to a more animated recognition of the Universe as a Whole." (Vol. ii, pp. 741-2).

The works by Cotta and Reuschle, whose titles stand prefixed to this article, have been called into existence by the publication of the COSMOS; they differ, however, essentially from each other. Reuschle's volume is divided into four books, treating respectively of—

1. The position of our earth in the world of space, and its cosmical natural relations.
2. The natural history of the heavens.
3. The interior of our earth as contrasted with the surface, and the formative processes that have taken place in the earth's crust.
4. The surface of the earth as it now exists.

Cotta's work, of which at present we have only the first part, consists of a series of forty letters, on various points alluded to in the first volume of the COSMOS. We select the subjects of a few of these letters, as illustrating the character of the book: the fixed stars, double stars, our solar system, the moon, comets, terrestrial magnetism, volcanic activity, formation of rocks, erratic blocks, the tides, the atmosphere, the geography of plants and animals, man. Cotta's is the more popular volume of the two; but each is well adapted for the object that both authors had in view, namely, to supply to the general reader a sufficient preliminary knowledge of the physical sciences to enable him to read the COSMOS with profit and advantage.

It only remains for us to say a word in justification of our preference for the English translation which, out of three, we have deemed it our duty especially to recommend. We have carefully compared all the English versions with the original German. Two of them—those, namely, of Mrs. Sabine and Miss Otté—are entitled to the highest commendation. Their translations are not only accurate, but elegant; and the genial spirit of the venerable author breathes through every page. Proud, indeed, may we feel of our fair countrywomen, when from their ranks there issue such women as those to whom it is now our pleasing duty to offer the highest meed of praise in our power to bestow. The third, which we may characterise as Mr. Baillière's, is in every respect the least successful. The translator or translators of the first volume wisely preserve their *incognito;* to the second volume the name of Mr. Prichard is attached, and it is with extreme regret that we find ourselves unable to speak favorably of this, his first literary effort. Many very serious errors and misunderstandings of the original are to be found, as he is doubtless by this time well aware, in the earlier portion of his volume—errors, into which we feel assured he would not have fallen, had he allowed himself due and sufficient time for the proper execution of his translation. But publishers are ofttimes hard taskmasters, and, in such cases as the present, the desire to be first in the field leads to an over-taxing of the translator's powers, and gives rise to a system in all respects prejudicial to sound literature. That this has been the case in the present instance we do not entertain a doubt.

The following are the reasons why we prefer the translation of Miss Otté to that of Mrs. Sabine. In the first place, it is the more accurate, and we have not detected in it any blinking of puzzling words or expressions, as we have done in one or two instances in the other version. Thus, for instance, in the first note to page 534 of the translation we have adopted, the German word *Waller-see* has been a stumbling-block to both Mrs. Sabine and Mr. Prichard. Secondly, the reading of the work is much facilitated by the translation into corresponding English terms of *all* the foreign measurements, and by the notes being removed from the end of the volumes and placed beneath the text in their appropriate positions. In the third and last place, this edition is enriched by many valuable notes, having reference to researches in physical science, made subsequently to the publication of the original work.

We need scarcely add that no ordinary work would have given rise to three rival translations. If we have succeeded in conveying to the minds of our readers even a faint conception of the rich stores of classical, antiquarian, and scientific lore with which the COSMOS abounds, and can thus induce them to read and study it for themselves, the object that we proposed to ourselves is attained.

It is but seldom that we diverge from the path of strictly professional matter in the pages of this review. There are occasionally, however, cases in which a deviation from our regular course is expedient; and we would fain hope that none of those into whose hands our journal falls, will regret that we have directed their attention to "an attempt to delineate Nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating, ever-recurring alternation of physical metamorphoses."

Art. IV.

1. *Return of the Mortality in* 117 *Districts of England, for the Quarter ending December* 31*st*, 1847. Published by authority of the Registrar-General. No. IV, 1847.
2. *On the Influenza or Epidemic Catarrhal Fever of* 1847-8. By Thomas B. Peacock, m.d., Licentiate of the Royal College of Physicians, Physician to the Royal Free Hospital, &c. &c.—*London*, 1848. pp. 182.

At the time when a formidable epidemic disease is extending itself over these countries towards the south-west of Europe and the shores of the Mediterranean sea, and when the attention of the profession is strongly directed to the phenomena which attend its progress, and manifest more or less distinctly the laws which govern it, it may not be inopportune to turn for a moment from our study of this disease, and to place conspicuously before ourselves the phenomena exhibited by another malady equally entitled with cholera to the appellation of epidemic. It may be advisable to see whether there is anything in common between the two affections, or any peculiarity of the one which may throw light on the course of the other. But, apart from this, the disease which we propose now to pass in review may well claim careful and repeated investigation from the practitioner of medicine. It is of all epidemic diseases the one which, at the time of its prevalence, is most widely diffused; the one which is least restricted by conditions of locality; and which, compared with other affections of the same great class, is least affected by the peculiarities of the systems it attacks, whether these are produced by condition of life, age, sex, or vigour, imperfection, or peculiarity of frame. Consequently it invades almost all nations on the face of the globe, and it attacks a large proportion of the individuals of each nation. It has prevailed from time to time for 700 years, and probably from ages immemorial. Its ravages were recorded even by the ignorant chroniclers and physicians of the middle ages; and historians paused from their warlike recitals to note the occurrence of the disease, which spared neither the noble in his castle nor the peasant in his hut. Its successive invasions have caused a great destruction of life, although, from the vast number of cases, the relative mortality is low. In 1311 it was very fatal throughout all France. In 1403 the courts of law were closed in Paris on account of the deaths. In 1580 entire Europe was filled with mourning and sorrow. In 1590, 6000 persons died in Rome alone. In 1729 more people died of it in London in one week, than in any equal period of time during the great plague of 1665; and at Vienna 10,000 persons were ill at once. In 1743, 1000 persons died of it in London in eight days. Occasionally its ravages have not been confined to men, but, as in 1728, 1732, and 1775, it has attacked both dogs and horses. It is a disease of extraordinary rapidity of progress, and on some occasions has passed from Asia to Russia, Germany, England, France, Italy, and America, in the course of a few months. As its diffusibility is great, so are its periods of recurrence frequent. Admitting that some of the recorded epidemics in the thirteenth and fourteenth centuries are wrongly referred to this disease, there is no doubt, from the experience of the last two centuries, that so long a period as ten years has seldom or never elapsed between any two visitations. Every second or third visitation seems to have been of extraordinary severity; and while occasionally the disease has appeared to have been confined within narrow limits, and to have arisen in various places, as it were independently and from conditions proper to those places at the time, in the far greater number of instances it has originated in definite localities, and has then seemed to possess an inherent power of progression, or to fall under the influence of some peculiar force which impelled it over vast tracts of the earth.

The characteristic onset and subsequent symptoms of the disease are to be recognised in the descriptions of the writers of all times and countries. The mode in which it suddenly attacks half a community; the intense headache, and occasional stupor of its early stage; its effects upon the mucous membrane of the nose and pulmonary passages; and the extraordinary debility experienced during the attack, or in convalescence, are circumstances noticed in the earliest and most imperfect descriptions. Everywhere also by the physicians and the laity the special character of the affection has been fully recognised. It has always been distinguished from the simpler sporadic affections which bear to it an external resemblance. As each successive generation has encountered its assaults, and has partially or entirely forgotten the experience which its forefathers had of it, the disease has received a fresh appellation. Occasionally it has been named from some striking symptoms, as in the old English writings, "Pose," from the Anglo-Saxon gepose (heaviness), a term marking the stupor of its early stage; "blitz katarrh," from its rapidity of attack; "quinte," from the paroxysms of cough returning every five hours; "grippe," from *gripper*, marking both its suddenness and its universality, or perhaps from the Polish "crypka-raucedo,"* marking its supposed origin; "die schaffhusten," because the cough resembles that common among sheep; "huhneizipf," because the cough was compared to the clucking of a hen. Sometimes it has been named from its supposed source. In Russia it has been named in some epidemics, "le catarrh chinois." In Germany, "die russiche Krankheit." In Italy, "Morbo Russo." Often it has received a name dictated by the caprice of the moment: Tac, Horion, Coqueluche, Ladendo, Petit Courier, Baraquette, Follette, &c. The term Influenza was first used in the seventeenth century, and has passed into the medical language of England and Germany. In France the term Grippe is used popularly and professionally.

The profession, like the laity, have also designated this disease by terms which plainly indicate that its special nature has never been overlooked. Sydenham named it Tussis epidemica; Stoll, Pleuritis humida; Cullen, Catarrhus à contagio, thus marking distinctly his opinion that it arose from a special agent, and not from mere atmospheric vicissitudes; Sauvages designated it Catarrhus epidemicus; Hoffman, Febris catarrhus. It has received a multitude of other names, but all indicate the same great fact that systematic writers have never failed to place this disease under a special and distinctive heading. Moreover, every writer on influenza, urging this fact to its legitimate conclusion, has distinctly laid down the proposition that the symptoms were so uniform as to lead irresistibly to the belief that the disease sprang from a formal and specific cause, which was in its essence, invariable in all countries, in all seasons, and in all vicissitudes of atmospheric condition.*

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* Elements of Medicine. By Robert Williams, M.D. Vol. ii, p. 665.

This proposition was stated with perfect precision in 1761, by Gilchrist. Its correctness has never been questioned in England. In France it has been also admitted, and the following quotation expresses exactly the general opinion on this point.

Delorme, the writer of the article "Grippe" in the 'Dict. de Médecine' (Vol. xiv, p. 282), thus expresses himself:

> "When we consider the assemblage of the symptoms of the 'Grippe,' its exclusive development under the epidemic form, the march of the epidemics which pass over a great surface of country, we cannot but recognise a malady *sui generis*, produced as the Black Death of the fourteenth century, as the cholera of our own time, by a cause unknown but general; a malady affecting, although in a very slight degree, the vital functions, thus to speak, as these two last epidemics, and as all those which, produced by infection,† such as typhus, yellow fever, plague, epidemic dysentery, are allied to a miasmatic poisoning; finally a general malady which manifests itself by certain local symptoms, important doubtless as specific characters, but which, signs of the organic condition, are purely secondary and accessory."

This quotation is both a definition and a generalization. Influenza is declared to be a disease with specific characters, and is thereby separated from all other diseases; but this separation does not isolate it; it is admitted into the great class which is made up of those diseases which all spring from specific agents, and whose symptoms are the effects produced by the reception into the system, under certain conditions, of those foreign and special forces. If to this class we add smallpox, scarlatina, pertussis, and measles (affections which cannot be separated from typhus and plague), we complete the series of diseases which arise from agents capable of such wide diffusion as to be occasionally epidemic.

A disease of this class must evidently be studied from different points of view.

1. The symptoms which invariably manifest the action of the special agent, must be determined, and the normal type of the disease thus isolated. Round this typical form, the several varieties and modifications must be grouped, whether they arise from various normal or abnormal states of the recipient system, or from variable degrees of intensity in the cause, or from alteration in its action from coexistent circumstances.

2. The diagnosis of the disease being thus settled by the discovery of its essential symptoms, the coincident phenomena which, whether extrinsic or intrinsic to the body, aid the action of the cause, can be determined by observation. In elaborating its normal type, the physician regards the cause chiefly as it affects individuals; in determining its accessory conditions, he studies it more especially in relation to communities and masses of men. Thus he learns what features common to the class, whether resulting from locality, habits, trades, or other obvious conditions, affect the prevalence of the disease. He observes, also, its relation to all physical phenomena.

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* Dr. Robert Williams, indeed, went further than this, and questioned whether every catarrh did not arise from a specific agent, and not from atmospheric changes.

† The term infection, as used by French writers, merely implies the existence of a special agent, without connecting it with the animal frame. It must not be confounded with the English contagion.

A perfect knowledge of all these particulars would constitute as complete a history of any disease, as in the present state of knowledge we can expect to form. But it is also not unreasonable to inquire—

3. What is the nature and essence of the cause, what circumstances produce it, what are the primary laws which it obeys, and what is the mode in which it produces those symptoms which we have before empirically determined? Rightly considered, these questions do not belong to an impracticable transcendentalism, but only to the most difficult and inaccessible portions of our science. They may not, indeed, be determined in this age; but there is reason to hope that, guided by an advanced physical science, our descendants may explore the gloomy recesses of this labyrinth, and illumine what to us is now almost impenetrably obscure.

We have made these introductory remarks for the purpose of indicating the method and spirit in which we shall examine the works before us. They are both valuable additions to the extensive literature of this subject, and are marked by many of those qualities of accurate observation and research, which speak so favorably for the advance of medical science in these countries.

At the present time we shall confine our remarks almost entirely to the influenza of 1847, and shall refer to other epidemics merely for elucidation and comparison. We do this especially, because it is understood that the Sydenham Society are now engaged in compiling a history of catarrhal epidemics, and many points will therefore be profitably deferred until this great work appears;* we shall proceed at once, therefore, to our subject.

1. *The determination of the typical form of influenza.* That form by which the presence of the active cause can be certainly recognised, is evidently the fundamental and necessary condition of all other inquiries. If we are not certain about the diagnosis of the disease, we are continually liable to serious errors, when we seek to determine from statistics the laws of diffusion of the specific agent. It is absolutely essential that a definite term shall correspond to a definite disease in the mind of each observer; otherwise statistics collected from numerous sources become but a mass of figures, whose apparent accuracy is as fatal, or even more fatal to science than conclusions drawn from a narrow and circumscribed field of inquiry. And although in the case of influenza it might be supposed that there can be no mistake about the grand facts, that there can be no doubt that such a disease is or has been prevailing, and that it has prostrated half a community with symptoms too remarkable to be mistaken; still it does appear that a great looseness exists in the use of this term. This has been especially apparent in former epidemics; thus in 1833, when the weekly mortality of London was trebled for two or three weeks, and doubled for three or four more, from influenza, the total number of deaths directly ascribed to this disease, during the whole time, was only 89.† In other epidemics every bronchitic attack has been called influenza, and every death in which influenza has played a subordinate share has been entirely ascribed to it.

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* This promises to be one of the most important works sent forth by the Sydenham Society. We regret, however, to be informed that it is intended to carry down the history only to the end of 1837. We must question the propriety of leaving out the recent epidemics, respecting which so much material has been brought together. We should hope, if not too late, that this determination might be reconsidered.

† Registrar-General's Report, p. 9.

Both these errors may have vitiated the records of the last epidemic. Thus it appears, that although in nine weeks the excess of deaths in London, over the average number for the same time, reached the astonishing number of 6066, only 1157 were actually returned as attributable to the epidemic disease. On the other hand, we shall have occasion to point out that several distinct diseases have been included under this appellation.

These facts show the importance of an accurate diagnosis, if we are desirous of availing ourselves of the admirable machinery for registering the causes of death, which is now in use. But a correct diagnosis can only be formed by analysing the results of careful observations, displaying the disease in its simple and normal form, and then by synthesis, adding to this outline the several pathological states which more or less intimately ally themselves with it. And in a correct statistical return, these allied pathological states, as well as the accidental complications, ought to be stated.

Not, however, to dwell too long on these preliminary questions, we will proceed at once to lay Dr. Peacock's views before our readers.

After an introductory review in which the outset of the epidemic is sketched out, and to which we shall presently return, Dr. Peacock proceeds to detail the symptoms of the disease. Adopting to its fullest extent the view that influenza depends on a specific agent, he first isolates the elementary form of the disease, which he terms "simple catarrhal fever." He then in the following chapters discusses this catarrhal fever "with pulmonary complication," and "with predominant disorder of the abdominal organs."

The "simple catarrhal fever" of 1847 generally attacked suddenly with chilliness and shivering, which was rapidly succeeded by an immediate and evident impression on the mucous membrane of the nose, mouth, frontal sinuses, trachea, and bronchial tubes to a greater or less extent; accompanying this, there were severe darting neuralgic headaches, aching of the limbs, listlessness, great mental depression, complete anorexia, and an extraordinary weakness, which, according to our observations, bore a close ratio to the extent of pulmonary affection, and consequently to the severity of the disease. The average duration of illness was from 3 to 5 days in the mildest cases, and from 7 to 10 in the more severe form. Towards the termination of the complaint, rheumatic affections, especially of the face and head, assuming an intermittent type, often occurred. Occasionally there was diarrhœa, and the complaint subsided with free perspiration. The auscultatory signs were those merely of bronchitis, dry rhonchi in some parts, and harsh vesicular murmur in others. This form required only gentle diaphoretic medicine; or, if there was much dyspnœa and tightness across the chest, an emetic followed by Dover's powder and nitrate of potash was found to be serviceable. Colchicum and quinine were given in the rheumatic sequelæ.

The pulmonary complications are divided into four forms:

1. Capillary bronchitis. 2. Bronchitis supervening on tuberculous disease of the lungs. 3. Bronchitis with disease of the heart or aorta. 4. Pneumonia.

Acute or sub-acute capillary bronchitis was the most frequent complication. Out of twenty-eight cases of influenza, with severe pulmonary affection, in the Royal Free Hospital, eleven presented this variety. Out of these eleven, four patients only were, previous to the attack of influenza, in perfect health. The symptoms were at first those of ordinary influenza; then the chest symptoms increased in severity, the cough became paroxysmal; there was dyspnœa quite disproportionate at first to the cough, or to the physical signs; the expectoration was scanty, and consisted of small yellowish pellets, forming tenacious masses of a peculiarly nodulated form. The pulse was rapid (120-40), the tongue covered with a whitish-brown fur, and the prostration was extreme. The only auscultatory signs were roughness of the inspiratory murmur, with occasional sibilus, and slight crepitation at the back. There was soreness and constriction at the chest, but no acute pain.

The crepitation soon extended over a greater or less extent of both lungs, but was unattended by dullness on percussion. Speedily the dyspnœa became so intense as sometimes to prevent the patient from lying down; the lividity of the lips and face increased, and the eyes became prominent; the cough was now very frequent, and the sputa were viscid, of a greenish yellow colour, without air-bells, and often streaked with blood. The respirations were quickened—in one case they reached 72 per minute—the pulse was either exceedingly quick (140-60) or intermittent, and consequently rather slower.

On referring to the illustrative cases, it appears that the case in which the respirations reached 72 per minute, was that of a man aged 26, labouring under old hemiplegia, and who was suddenly attacked with capillary bronchitis after an anomalous epileptic seizure; the pulse was 136 per minute, therefore the respirations were to the pulse as 1 to 1·8. On the following day the respirations were 40, the pulse 72 per minute, the ratio being thus exactly preserved, 1 to 1·8; in this case the cerebral symptoms possibly modified the ratio. In calculating, however, the relation between the number of respirations per minute, and the pulse, in all the cases of capillary bronchitis in which the point is noted, we do not find any uniform connexion between the extent of the disease, and the disturbed ratio of the pulse and respiration-movements. Thus the first recorded case was one of slight capillary bronchitis; on the fifth day, when the observation was made, the ratio of respiration to the pulse was as 1 to 2·9. In the second case, also one of slight bronchitis, the ratio on the fourteenth day was as 1 to 3·8. In the third case, which was very severe, the ratio on the eighth day was also 1 to 3·8. In the 5th case, four days before death, the ratio was 1 to 3. In the sixth case, four days before death, the ratio was 1 to 3·4. So that the general rule was, that the respirations were relatively more quickened than the pulse in all the cases, but not appreciably more, as far as these very few cases indicate, in the severe than in the slight cases.

After the disease had lasted some little time, the physical signs were modified by rapidly developed emphysema of some portions of the lung, while others became dull from more or less extensive hepatization. This emphysema, as far as could be made out by physical examination during life, or post-mortem examination after death, often occurred in a slight degree throughout the whole extent of the lungs. It is not difficult to conceive, that the laboured inspiratory movements would tend to force the air past the viscid mucus, in a greater degree than the feebler expiratory movements could again expel it. Consequently, the residual air accumulating in the cells and smallest bronchial tubes, and pressing upon membranes which perhaps had suffered alteration from the inflammatory action, produced a slight but universal dilatation, which no doubt increased the dyspnœa, already sufficiently urgent on account of the universal and equal affections of both lungs. It has been long known that influenza often left emphysema as a sequel, and, in fact, this must evidently be a common result; Dr. Peacock, however, has noticed that the emphysema was often transient; during convalescence the abnormal resonance gradually disappeared, and was even in some cases succeeded by slight general dullness.

This capillary bronchitis was distinguished from pneumonia by the greater severity of the general symptoms; by the tendency of the fine crepitation of the early stage to pass into subcrepitant and mucous râles rather than to give place to evidences of condensation; and by the peculiar characters of the cough, which was paroxysmal and not attended by pain, and of the expectoration,* which consisted of whitish viscid pellets cohering into irregular masses, and was destitute of the glairy adhesive character, russet colour, and small air-bubbles of pneumonic expectoration. These characters, however, Dr. Peacock mentions, will distinguish only the most marked forms; and there is no doubt that almost every case of capillary bronchitis depending on influenza, is combined with more or less lobular or even lobar pneumonia on the one hand, and with general inflammation of the larger bronchial tubes and trachea on the other.†

Out of eleven cases of influenza, complicated with capillary bronchitis, no less than five died. Two of these patients, however, had been ill for some time previous to the attack, and after death "presented such advanced organic disease of the lungs as would have terminated existence had not life been more rapidly destroyed by the bronchial inflammation." Two of these other three cases could not be examined after death, so that there is only a single post-mortem examination of this sole complication. And even here there was old standing emphysema of both lungs, at least if the case referred to be that of Ann Gilbert (case 6, p. 131); so that we cannot discover any account of the post-mortem appearances in an uncomplicated case of capillary bronchitis and lobular pneumonia. But judging from the appearances presented by the lungs in other cases complicated with capillary bronchitis, in addition to still more important lesions, Dr. Peacock gives the following account:

> "The peculiar feature presented by examination after death from this form of disease was the extremely inflated and emphysematous condition of the lungs, which in lieu of collapsing when the chest was laid open, in some cases even protruded from the cavity. This condition was not limited to certain portions of the lungs in which there were larger or smaller dilated cells, as in ordinary cases of emphysema, but was evidently due to a general, and tolerably equal inflation of the entire pulmonary tissue. . . . The mucous membrane of the bronchial tubes was reddened, and the injection increased from the larger toward the smaller tubes. . . . Pneumonic condensation, either of the lobar or lobular form existed to a greater or less

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* The chemical and microscopical examination of the sputa in the bronchitis and pneumonia of influenza is well worthy of study. (Magendie.)

† M. Beau has lately (Archives Générales de Médecine, Septembre, 1848) called attention to catarrhal inflammation of the trachea, as distinguished entirely from plastic tracheitis or croup, and from the common forms of bronchitis. In influenza, the whole laryngo-bronchial tube is more or less profoundly affected.

extent in every instance. In the lobular form, the solidified masses varied from patches involving two, three, or more lobules, to others of much greater extent, and were in all cases distinctly bounded by the interlobular septa." (pp. 31-2).

In the three cases in which there was no phthisis, death occurred on the ninth, tenth, and twelfth days. In the cases of recovery, the ordinary duration of the cases was from fourteen to twenty-one days. When *pneumonia* was the complication, there was always more or less coexistent bronchitis; there was also present an unusual degree of depression of strength; the cough was severe; the expectoration consisted of brownish coloured and viscid masses, with small air-bells, intermixed with glairy transparent or opaque fluid, containing large bubbles of air; the physical signs were those of pneumonia. Both the pulse and the respiratory movements were slower than in the cases with capillary bronchitis. In one case the respiration was to the pulse on the seventh day of the disease as 1 to 2·2; in another, on the fourth day, as 1 to 3; in a third, of uncertain duration, as 1 to 4. The average of the respirations was from 28 to 44 per minute; and of the pulse, from 80 to 100. The variations of the ratio were from 1 to 1·8, to 1 to 3·5. This complication, which occurred in six out of forty-eight severe cases, did not prove fatal in a single case, and yielded, indeed, to tolerably mild treatment.

A *laryngeal* complication existed in many cases, although no case which could be termed actual laryngitis came under Dr. Peacock's notice.

Neither pleurisy nor pericarditis appear to have been common complications; we find an incidental mention of one case of circumscribed pleurisy, and in a second case, marks of recent pleurisy were found after death. In two fatal cases, effusion into the pericardium was suspected in one, and in the other recent pericarditis was found.

Dr. Peacock then proceeds to a general "review of the results of catarrhal fever with pulmonary complication:" 48 cases were admitted into the Free Hospital, 24 of each sex, and of these 3 men and 6 women died. We must confess, however, that we cannot make out how these numbers are determined. We have been informed that 5 cases of capillary bronchitis died, but we find no statement of any other deaths from pulmonary complaints, except 1, which case, however, Dr. Peacock, at page 64, expressly excludes. At page 56 we have, it is true, a reference to 4 fatal cases, complicated with disease of the heart and bronchitis, and we presume that Dr. Peacock has included these in the table at page 46. Admitting that this surmise is correct, it appears that 2 persons died from the supervention of influenza on phthisis; 1 from the supervention of influenza on old emphysema; 2 from pure influenza, that is, considering the capillary bronchitis as an intrinsic part of it; 2 from influenza complicated with peri- and endo-carditis; 1 from influenza supervening on old heart-disease; and 1 from aneurism of the aorta after the influenza had disappeared. Excluding this last case, we have 8 cases of influenza which proved fatal out of 48, and of these 4 were labouring under previous severe organic disease. The other 4 then represent the actual mortality of the influenza *per se*, irrespective of the influence exerted by it on organic diseases whose course it accelerated, or which caused it to prove fatal. But it must be remembered that this number (4 cases out of 48) does not express the number of deaths to the whole number of cases of influenza; as only the severest cases were admitted into the wards, and the vast majority of cases were treated at their own homes, and, being of slighter character, presumably recovered: and even these 4 cases do not appear altogether free from ambiguity. One of them was in a woman formerly of intemperate habits, and attacked first with vomiting and jaundice before the bronchitis; another was in a girl who had previously suffered from cough and debility; and in both these cases there was no post-mortem examination. In the other 2 cases there was heart-disease. It follows, therefore, that, although the actual mortality of pure influenza cannot be laid down, according to Dr. Peacock's experience it must have been very small; and it is very important to remember, for reasons to which we shall presently allude, that the previous health of the subjects bore a close relation to the mortality. So that, in judging of the severity of an epidemic by the number of the deaths among a given population, two distinct elements enter into the calculation, viz. the actual power of the morbific cause, and the previous condition of health of those on whom it acted.

The treatment of the capillary bronchitis attending influenza is most judiciously given by Dr. Peacock. In accordance with general experience, he found bloodletting of little use, except in the early stage; it increased prostration, without benefiting in any commensurate degree the pulmonary disease. Leeches were sometimes useful, and counter-irritation of various kinds. At the commencement of the attack an antimonial emetic was generally given, and in some cases antimonials were persevered in, but Dr. Peacock thinks with less success than in common sthenic pneumonia. In other instances, mercury, to gentle salivation, with opium was given. In a later stage, anodynes, antispasmodics, and diffusible stimuli were necessary. Dr. Peacock points out the importance of early treatment; although the disease, depending upon a specific poison, necessarily runs a course, yet this may be much mitigated by appropriate measures; if, however, the disease proceed to copious effusion into the smaller bronchial tubes, it is obviously in a state very unfavorable for treatment. During convalescence, in addition to general tonic measures, the sulphate of zinc was found to be very serviceable when the expectoration was thin and spumous; and, on the other hand, when it was very viscid, glairy, or "vitreous," alkalies, combined with expectorants and anodynes, were useful.

In the pneumonic complication, which, as already said, was very amenable to treatment, antimonials were of greater use; depletion was used in only one case, mercury in two, and some form of counter-irritation in all. In the other chest affections, pleurisy, or old or recent heart disease, the treatment was of the usual kind.

We must now pass on to the chapter on the "abdominal complications" of influenza. We confess we were curious to know what Dr. Peacock had to say upon this point. We were aware that in many epidemics, in 1676, 1729, 1732, 1737, 1761-2, and others, and even in 1803, 1833, 1837, the occurrence of bilious purging, vomiting, and evident derangement of the digestive organs, had been more or less carefully noted. In the earlier epidemics the bilious diarrhœa is usually described as coming on towards the termination of the complaint, on the fourth, fifth, or sixth day, and it is often regarded in the light of a critical discharge, like the profuse perspirations which have been in many epidemics such marked accompaniments of the later stage of the disease. Even in the epidemic of 1847, we had ourselves occasionally noticed a manifest implication of the liver and the digestive mucous membrane; and it did not seem improbable to suppose that these phenomena, secondary and consecutive in all cases to the primary impression of the poison on the laryngo-pulmonary mucous membrane, might yet occasionally acquire an unusual development and importance. We considered indeed these phenomena, as well as those of the skin and kidneys, to be of great interest, as testifying to the *general* action of the cause, whose elective choice of locality, so to speak, was nevertheless on that particular mucous membrane, whose deranged actions constitute the pathognomonic symptoms of the disease. We had never ourselves worked out the exact relation of bilious diarrhœa, or vomiting, to the primary symptoms, nor were we aware that it had been done by others; we were therefore all the more anxious to know what an acute observer like Dr. Peacock had to say on this interesting point. But, after a careful consideration of this chapter, and of the light which is thrown on it by the general opinions of the writer as testified throughout the whole work, we are compelled reluctantly to differ altogether from Dr. Peacock, and to record our dissent from his opinions of the nature of the abdominal complications of influenza, and of the connexion between this affection and other epidemic diseases prevailing at the same time.

In order, however, to do full justice to Dr. Peacock, and to enunciate clearly the grounds on which we differ from him, we shall briefly state the point of view from which we are disposed to observe influenza and all affections of the same class, and then contrast with it the opinion of Dr. Peacock.

The first aim of the practical physician of the present day, is the discrimination and diagnosis of diseases. He does not, it is true, make the multiplicity of divisions and varieties which some of our old nosological writers delighted to enumerate, and under which the typical form of the disease was often buried and concealed; but he seeks to determine the essential and primitive elements of each disease, as the foundation of all treatment, of all inquiry into laws, and of all preventive measures. This is especially the case with diseases which originate from the introduction of foreign agents and forces into the system, from whatever sources they may be derived. Those diseases which severally spring from special and uninterchangeable poisons, are themselves equally distinct and uninterchangeable. They may intermix; they never pass into each other. No one ever saw smallpox deviate into measles, or scarlet fever into cholera. No one in the present day believes that our English typhus can become the bubo plague of the Levant, or the marsh fever of Batavia the typhoid fever of Paris. And, in respect to the continued fevers of this country, and of northern Europe generally, the researches of the last twenty years seem gradually, but certainly, tending to the opinion, that, under the term "continued fever," several perfectly distinct diseases have been included. From the heterogeneous and contradictory accounts of the systematic writers of the last generation, it is clear that they had no idea of this separation. But already three, if not more, distinct forms of disease have been designated, which are distinguished by differences in course, differences in symptoms and treatment, and differences as to the influences exerted upon them by locality and general or special atmospheric conditions. It is certainly not yet absolutely *proved*, that there is a continued fever with a special eruption, with a certain duration, and with particular anatomical signs, derived from certain abdominal organs, and particularly from changes in the patches of Peyer; that there is another disease, with a totally distinct eruption, an entirely different course, and which never presents the same anatomical signs, but which still has been included in the term "continued fever;" that there is a third, characterised by a most remarkable feature, viz. a tendency to relapse on particular days, and unmarked by eruptions, or a course at all analogous to the two former affections; and yet, although not proved, there is the highest probability that these distinctions are real ones, and that, in another ten years, practitioners will look upon the way in which they have been confounded together with the surprise with which we remember the singular confusion, formerly prevalent in the minds of the most eminent physicians, as to the differences between measles and scarlatina. As another disease of this class, having laws of its own, a special mode of spread, pathognomonic symptoms, and a certain course, influenza is, by the supporters of this opinion, as much distinguished from typhoid, maculated typhus, or relapsing fever, as any of these are from each other, or all from smallpox. It may combine with them, modify their symptoms, alter their treatment, or increase their mortality; but it still preserves intact its own individuality. And this holds good, even if we adopt the opinion, that the continued fevers of this country are but modifications of one stock.

The best way of describing Dr. Peacock's opinion will be to say, that, as far as continued fevers and influenza are concerned, it is exactly the reverse of the one above stated. He admits influenza into the class of fevers, and calls it "catarrhal fever;" and this term is not used in the simple sense of the older writers, to distinguish influenza from common catarrh, but has a deeper meaning, and expresses a relation to the continued fevers. Dr. Peacock appears to consider, that all the forms of this class of fevers are the modifications which one great entity undergoes, from the influences to which it is subjected by the prevailing epidemic constitution. He does not, certainly, in so many words, formally adopt this opinion, but he does virtually profess it, with more or less precision, in various parts of his work. This will more evidently appear when we have proceeded a little further with the analysis. At page 17, Dr. Peacock writes thus:

> "It is my intention to treat of the several varieties of the disease under the heads of—
>
> "1st. Simple catarrhal fever.
>
> "2d. Catarrhal fever with pulmonary complication.
>
> "3d. Catarrhal fever with predominant disorder of the abdominal organs.
>
> "Of these the first and second comprise those to which the term influenza is commonly applied. The third class I shall further subdivide into:—1st, Cases of simple enteric fever; 2d, Cases with bilious disorder, and characterised by a tendency to relapse, or assume a remittent form; and 3d, Gastro-enteritic and bilious fever, complicated with rheumatism."

In the chapter on the abdominal complications, these views are worked out. We will quote the description of the more severe cases of the "catarrhal fever with gastro-enteric disease:"

> "The cases usually commenced very gradually, so that it was difficult to ascertain the precise period of invasion. The symptoms at first of a mild character,

> after the disease had continued to advance for a week or ten days, began to assume a more threatening aspect; the pulse became feeble and quick, rising to 120, 130, or 140 in the minute; the tongue became dry, acquired a thick, brown coating, and was morbidly red at the tip and edges, and often chapped and painful. The teeth were covered with yellowish-brown sordes; the skin was hot, dry, and harsh, and continued free from moisture when its temperature had declined. From an early period there was much restlessness and inability to sleep, often combined with torpor, so that the patient was with difficulty aroused, and subsequently delirium made its appearance, generally of the low muttering form, but in several cases so active as to require restraint. With the progress of attack the usual comatose tendency appeared, and in one case death was preceded by convulsions. The abdomen was generally at first somewhat tympanitic, and afterwards became tense and tender to the touch, and frequently a gurgling sound and feeling of fluctuation were caused by pressure in the ilio-cæcal region. In the early stages the bowels were not unfrequently confined, though usually more relaxed than in a state of health, and at a later period there were frequent liquid stools, with occasionally some blood in them. A slight jaundiced tinge was usually present. Some degree of pulmonary disorder, characterised by hurried respiration, cough, and the usual signs of febrile bronchitis, was also very generally observed. (pp. 59-60.)

The duration of this form, we are told in the next paragraph, may be estimated at from 20 to 25 or 30 days.

Now, we think all our readers will at once say, this is a good description of typhoid fever; but how comes it to be called influenza? It commences insidiously, it has a long course, it is attended with the abdominal and the pulmonary symptoms of typhoid fever; but it wants altogether the signs of influenza.

On turning to the chapter on the mortality (p. 75), we find this view confirmed. There were five fatal cases of what Dr. Peacock terms "epidemic catarrhal fever with abdominal complication;" and yet, immediately after this, we read: "Of the five fatal cases, four were examples of continued fever with intestinal disease." (p. 75.) Of these, one was in a man who had been found lying ill in the streets, and was dying when admitted. The second case died on the twenty-sixth day of the disease; the third on the thirtieth; and the fourth on the twenty-seventh day of the disease. In all these cases, the usual post-mortem appearances of typhoid fever were found, viz. ulceration and sloughing, following deposit into the patches of Peyer and the solitary glands; but the account (pp. 76-7) is too long for us to extract. These were undoubtedly, then, cases of common typhoid fever; that is to say, of the disease so well described by Louis and Chomel. Nay, even Dr. Peacock recognises them as such; he is far too good an observer to confound them with the common form of influenza; only, in the spirit of his system, as they occurred during the prevalence of catarrhal fever, he considers them as allied to this disease. He seems to attach some importance to the fact, that "they had not the usual form of typhoid eruption;" and, indeed, only in one case any eruption at all.

In the following section, which is entitled "Catarrhal fever characterised by tendency to relapse," we find also known forms of disease described, which practitioners generally are not at all disposed to regard as influenza. There are several undoubted cases of the fever which, during the last five years, has attracted so much attention in Edinburgh and elsewhere, from its tendency to relapse between the eleventh and fourteenth day after seizure, and when convalescence had apparently set in. Cases apparently of more or less regular remittent fever are also described. Our space will not allow us to extract these sketches, but on reference to the book (pp. 61-2), it will be seen that Dr. Peacock has drawn them very accurately.

In the third section, which is headed "Catarrhal fever, with affection of the gastro-enteric mucous membrane and liver, complicated with rheumatism," we have several distinct diseases brought together, which we are not certain that we can refer to any head, except that it does not appear to us that their connexion with influenza is clearly made out. Many of them (pp. 63-5) seem to have had an intermittent or remittent character, and to have been attended with severe rheumatic pains and profuse perspiration.

The way in which Dr. Peacock is disposed to blend all these febrile states, and to consider them but modifications of the same disease, is shown still more clearly by a general exposition of his views of fever and influenza, which occurs at page 104; here he distinctly refers to fever as a single affection, undergoing alteration in symptoms and post-mortem appearances from the influence of localities and seasons. He then says, "these views, if applicable to the more intense forms of febrile disease, no less apply to those of a slighter description," that is, we presume, to influenza.

Although in this passage Dr. Peacock seems to include all fevers in one general term, he has previously expressed an opinion somewhat at variance with this; thus, at page 9, he writes, in reference to the increase of typhus during the epidemic, "the deaths registered as from typhus should not be relied on, as showing the prevalence of *true typhus* characterised by the roseolous eruption on the skin, but be viewed rather as cases of typhoid fever with intestinal and other complications, or as examples of the remittent form of fever."

Dr. Peacock advances as evidence of the correctness of his description of the abdominal complications, the two following statements: 1. That in his practice the cases merged one into the other. In reply to this, we can only say that although we should have expected that influenza might attack fever patients, yet Dr. Peacock has described the cases so well, that we have no difficulty in distinguishing them. 2. That the complications had certainly a near relation to the epidemic disease. Thus he writes:

> "When therefore we consider the remarkable coincidence in the period of accession, general prevalence, and decline of the several forms of pulmonary, enteric, hepatic, and rheumatic affections; the very common occurrence of symptoms referable to the stomach, intestines, and liver, in the cases with predominant affections of the lungs, and of those of bronchitis and pneumonia in the abdominal form of the disease; and when we further find that these symptoms were features not peculiar to the recent influenza, but have characterised all its former appearances (?), we can scarcely refrain from acknowledging that these several affections are not merely coetaneous, but correlative, and types and modifications of one disease, with which they have a common origin." (p. 103.)

Now certainly it does appear that, during the prevalence of influenza in London, the deaths from typhus rose from a weekly average of 70 or 80 to 130 and 136, but the same increase occurred in the deaths from hooping-cough (which rose from 12 and 25 to 65 and 71), and from measles (which rose from 43 to 96).* An increase likewise occurred in the deaths from smallpox, erysipelas, scarlet fever, as well as from almost all pulmonary and some cardiac affections. It appears, therefore, that the increased mortality of continued fever observed to influenza·the same relation merely as many other diseases, its course and termination being unfavorably influenced by the catarrhal epidemic disease, either directly or indirectly; but this no more connects influenza with typhus, or typhoid fever, than it does with smallpox or scarlet fever. In some of these cases influenza may have directly attacked a patient labouring under typhus or smallpox, and as the Registrar-General expresses it, "two zymotic processes may have proceeded simultaneously." It is, however, apparent that, in addition to this, the general health of the community must have been lowered, and the progress of all disease unfavorably impressed by the presence of those conditions which accompanied the spread of the influenza poison. This is one meaning of the term "epidemic constitution." The epidemic constitution implies such more or less diffused atmospheric conditions, as—1st, may impress a type on diseases; and as, 2dly, may possibly give to some particular poison a more or less extensive power of development. But the *disease* remains the same, although its *type* is altered. And this distinction between the epidemic disease and the epidemic constitution has been defined with great care by several writers, particularly by Ozanam,* and is evidently of the highest importance. It has never been supposed that specific and distinct diseases merged into one another under the influence of such a constitution, but simply that one was more prevalent one year, another the next; and that in each year a type more or less inflammatory, more or less low, &c., marked the specific evolution of each distinctive series of symptoms.

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* Registrar-General's Report, p. 3.

If this explanation be erroneous, if year after year the essential forms of disease vary, and do not merely undergo such modifications as can be ingrafted upon a recognisable and, in its main features, an unchangeable stock, if each year a fresh portrait has to be drawn of the mutable and ever-changing fever of the period, then it appears to us that the medical art can be based only on conjecture, and that the attempt to form laws which may embrace such shifting phenomena, is like building houses on quicksands, which the next flow of the ocean may engulf. The symptoms of fevers then become but like the pieces of glass in the kaleidoscope; every revolution of the seasons causes a fresh arrangement in the elements which the observer gazes at; but what man would trouble himself with noting a form which is the next moment to disappear, and with recording an arrangement which it is highly probable he will never again witness? But until repeated observation has proved that it is impossible to make a philosophical arrangement of the phenomena of fevers, we will refuse our assent to a doctrine, which, if pushed to its just conclusion, seems to us to strike a fatal blow at a science like ours, which is founded wholly on observation.

It must not be supposed, however, that we accuse Dr. Peacock of such an error. He is too acute an observer, and too philosophical a reasoner, to hold such an opinion, if pushed to the extreme we have indicated. And yet, if we are not misinterpreting his opinions, he has, without what appears to us sufficient warrant, extended the use of the term influenza to affections merely incidentally and for the time connected with it. This has evidently arisen from a wish to describe, not merely the epidemic, but all the phenomena witnessed during the epidemic constitution; and no doubt, in due subordination to the principal disease of which he was treating, this might have been a most important addition to our knowledge. But by approximating too nearly to influenza, affections which are daily seen when no influenza is present, he has to our minds made an erroneous generalization. There are many men whose opinions on such a point might be passed over in silence, but Dr. Peacock is not one to be so lightly dealt with. Whatever he advances has, we are sure, been well considered; and we are only sorry that in the discharge of our duty we are compelled to express dissent from the opinion of one who is proved to be by the book, from which on this matter we dissent, a most able and conscientious observer.

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* Op. cit., tome i, p. 40.

One evil directly appears, if we adopt Dr. Peacock's view. The influenza of 1847 must have been a very Proteus, lurking under all disguises, and assuming forms which we fancied we had seen before. It is in vain then to inquire whether there is not some typical form, some assemblage of effects which must invariably flow from an invariable cause. On what rule are we to term a disease influenza? And it is a matter of no light importance to know this, if we are to make returns of cases, and to arrange statistical tables, from whose stringent conclusions there is to be no appeal. Are cases of typhoid fever to be termed influenza? are cases of relapsing fever to be termed influenza? If so, let us know it; but do not let one man restrict the term to a particular manifestation of symptoms, and another extend it to what would be considered, by the first practitioner, as separate diseases.

We have occupied so much space in this discussion, that we must pass very briefly over the two following sections.

2. The first of these comprises the accessory conditions which favour the action of the cause.

In 1847 influenza assumed the character of an epidemic between the 10th and 22d of November.* It was at its acme in the second or third week, and declined slowly for four or five weeks subsequently. The mortality of London immediately before the outbreak was not great; the deaths in the three weeks ending the 20th of November, were 1052, 1098, and 1086. In the first week of December, 2454 deaths were registered, and in the second week 2416. The excess of mortality during nine weeks was 6066 over the average. Of these only 1157 were directly referred to influenza; but there was a great increase in the mortality of asthma, bronchitis, and pneumonia, which was no doubt attributable altogether or partly to influenza; and in that of several zymotic and other diseases, as already mentioned. The epidemic was most fatal to adults and the aged. The mortality in childhood was raised 83 per cent., in manhood 104 per cent., and in old age 247 per cent. From the age of 4 to 25 the mortality was comparatively not much increased; at the age of 10 to 15 it was scarcely increased at all in girls. With regard to sex, the difference in the mortality of males and females was but slight. (Registrar-General's Report.) In Geneva, M. Marc d'Espine endeavoured to prove that females were more liable to the disease than males.†

The epidemic was much more fatal in some districts of London than in others; it killed twice as many people in the insalubrious parts of London, as it did in those less unhealthy. The ordinary rate of mortality in Lewisham, which includes Blackheath, Sydenham, and Eltham, is 17 per 1000 annually; during the epidemic it rose to 27. The ordinary mortality in St. George's in the East is 29 per 1000 annually; during the epidemic it rose to 73. The increase in the first district was therefore 10, in the last 44; so that the unhealthy district suffered four times as much as the healthy. (Registrar-General's Report.) It is necessary to consider this fact a little more minutely. We have seen that, in Dr. Peacock's experience, the mortality of influenza was owing more to the condition in which the disease found the patient, than to any inherent power in the poison itself. And this is not only conformable to general experience, but seems to be confirmed by the returns of the Registrar-General. During the healthiest period of life (from 10 to 15), the mortality was scarcely increased; even between 5 and 25 "it was comparatively not very much increased."[*] And there is no evidence to indicate that persons between 5 and 25 show such a diminished susceptibility to the poison as would account for the small mortality. On the contrary, in the healthy period of life (between 10 and 15), the susceptibility is, if anything, greater than at other periods. At least this appears from the fact that at Christ's Hospital about 350 boys were affected,[†] being, we presume nearly one third of the whole number of boys, whose ages are generally between 10 and 15. Now the Registrar-General calculates that one fourth of the inhabitants of the metropolis were attacked; so that we may fairly conclude that the increased mortality among certain ages does not necessarily prove that influenza affected such subjects more frequently, but only that it affected them more severely.

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* Peacock, p. 1.

† Ibid., p. 14.

With regard now to the contrast between Lewisham and St. George's in the East. Did influenza attack more persons in the unhealthy than in the healthy district; or, in proportion to the population, was the prevalence of the disease the same, and was it only the mortality which acquired such a frightful increase in one place? We have no returns of cases which can guide us to an answer, and yet the answer is indispensable before we can attempt to lay down the laws for the influenza poison. We have seen at any rate that it does not necessarily follow that because the mortality was low among certain classes, the prevalence of the disease was in a corresponding degree circumscribed. The mortality alone is no test of prevalence, because it is dependent on coincident conditions.[‡]

The difficulty of this subject may be more clearly put, by attempting in different ways to account for the mortality in St. George's in the East.

1, It may be assumed that the specific cause of influenza found in the fetid and insalubrious air of this densely-populated, ill-drained, and ill-cleaned locality the materials by which it might increase in quantity and power. The disease would, therefore, be more prevalent and more fatal, by reason of the augmented force of the cause. This we presume is the opinion of the Registrar-General. He believes that influenza may even be generated in such a locality. "Influenza," he says, "appears to be generated in ill-organized camps, in crowded, ill-drained cities." (p. 5.) And in the next page, his mode of viewing epidemic agents is explained in the following eloquent passage:

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[*] Report, p. 2.

[†] Peacock, p. 3. In some former epidemics, the boys at Christ's Hospital have been less affected. In 1782, out of 700 boys, only 14 had the disease; and in the same epidemic, among the boys at the Hospice de Vaugirard, near Paris, out of 40 children, not one took the disease. (Elements of Medicine, by Robert Williams, M.D. Morbid Poisons, vol. ii, p. 672.)

[‡] The Registrar-General states (p. 6), that, from Dr. Bain's Report of the Influenza of 1837, and from the Report by the Provincial Medical Association, data might now be procured for determining the exact mortality at different ages, of complicated and uncomplicated cases. We trust the Sydenham Society will look to this important point.

> "The earth, it is well known, is surrounded by an atmosphere of organic matter, as well as of oxygen, nitrogen, carbonic acid, and watery vapour. This matter varies, and is constantly undergoing transformations from organic into inorganic elements; it can neither be seen, weighed, nor measured. The chemists cannot yet test its qualities. Liebig, with all the appliances of the Giessen laboratory, cannot yet detect any difference between the pure air of the Alps and the air through which the hound can tell a hare, a fox, or a man has passed, or the air which observation shows will produce smallpox, measles, scarlatina, hooping-cough, dysentery, cholera, influenza, typhus, plague. These matters may be either in a state of vapour that is elastic or inelastic, or, like water, they may exist in both states. They are most probably in the state of suspension; hang like the smoke in cities over the places in which they are produced, but are spread and driven about like vesicular water in clouds. A stream of aqueous vapour of the same elasticity, from the Atlantic, passing over England, is in one case perfectly transparent; in another mist; in another rain; so clouds of infectious matter may fleet over the country, and in one place pass harmlessly, in another (as influenza in London) destroy thousands of lives. The emanations from the living, the graves, the slaughter-houses, the heaps of filth rotting, the Thames, into which the sewers still empty, raise over London a canopy which is constantly pervaded by zymotic matters; in one season this, in another that preponderating."

This lucid passage seems to us to express the general, and probably the correct opinion. It might have been as well, however, to have referred to the reproduction of these poisons by the living body; it seems impossible to allude even to smallpox, scarlatina, or typhus, without noticing the material increase which the poisons in this way decidedly undergo.

It will be seen that, when examined, this passage really implies an increase of the influenza, as of the other specific poisons, under the influence of these conditions. And with this we are disposed to agree, partly from direct evidence, partly from analogy. The direct evidence is, however, at present imperfect, and analogical reasoning is in these cases to be received with caution; therefore we shall leave the point undecided, and inquire whether the excess of mortality in St. George's in the East could have been produced by other causes, independent of actual increase in the quantity of the poison.

2, It may be assumed that the influenza poison all over London was the same; that it penetrated all parts equally; but that three accessory causes concurred in aiding its manifestation in St. George's in the East rather than in Lewisham. It may be supposed that the air, less freely changed in the narrow courts and among the crowded buildings of the first locality, permitted the poison to accumulate in a higher degree than the fresh breezes blowing over the comparatively open country about Lewisham would allow. So that, without actual multiplication of the poison, it might be in greater quantity in one locality than another, from mere physical and not from chemical or vital reasons. It may also be supposed that the inhabitants of St. George's in the East were predisposed by the constant inhalation of their unhealthy air, and by the state of body thence resulting, to the reception of the poison thus allowed to accumulate. Both these suppositions are probable; but they are unsupported by direct evidence in this particular case, as we do not know the number of cases to population in St. George's. It must also be remembered that the persons inhabiting poor districts of this kind will, from exposure, be more liable to influenza. In 1708, Lancisi noticed that those who exposed themselves to the inclemency of the season were attacked; while those who kept themselves from cold escaped. In 1732, while the whole population of Edinburgh was attacked, it was noticed that neither the prisoners nor the children in Heriot's Hospital were affected. In the late epidemic there was not a death in any prison in the metropolis, and very few cases among the prisoners, or the lodgers in some of the model lodging-houses. So that, supposing that the number of cases in St. George's in the East was in proportion to its excessive mortality, it may be argued that, apart from actual multiplication of the poison, more persons were attacked from the want of fresh air to drift away the poison, from the increased predisposition to the disease, and from the increased exposure to the active cause, which must have occurred among a badly-housed and badly-clothed population.

3, In addition to all these several points of view, the following proposition is evident: If the mortality of typhus and typhoid fevers, of measles, pertussis, scarlatina, bronchitis, and heart-disease is raised by the influence of the epidemic conditions attending influenza, it follows that, in places where these diseases are most abundant, the mortality, as compared with other places where they are less common, will be raised in proportion to their prevalence.

In estimating the intensity of influenza simply from the increase of mortality during its prevalence, these pre-existing diseases cannot safely be overlooked. Now among the poor population of St. George's in the East, all these diseases must have prevailed in a far higher degree than among the scattered and healthier inhabitants of Lewisham. Therefore the expression, that "the track of influenza is identical with the track of typhus," is not proved to be absolutely true, by the fact that in the typhus district the mortality has been thus raised. By destroying typhus entirely, we should not necessarily save one person from being affected with influenza; but we are certain that we should save many from dying of it. We might possibly, by removing the conditions which aid typhus, lessen the prevalence of influenza; that is a matter for further evidence to decide; there is no proof at present that we should do so.

To sum up our inferences.—It appears that, in accounting for the increased mortality in any particular district during an epidemic of influenza, the following circumstances at the least, may be influential:

1. The poison may undergo a more or less rapid increase.
2. Or from physical conditions, it may accumulate without actual increase.
3. Or the inhabitants of the district may be particularly predisposed to it.
4. Or they may be particularly exposed.
5. Or they may be subject to diseases, which may be unfavorably impressed by it.

We do not think it unlikely that all these circumstances may have influenced the result; but the measure of their respective influence cannot, it appears, without much additional evidence, be determined.

Leaving, however, this question, another point presents itself for consideration. The Registrar-General represents the mist of organic matter which overspreads the places where men are densely congregated, as the medium through and in which a morbid poison traverses and extends itself. Admitting this, what is the reason why one year scarlatina pervades this mist, measles another year, typhus a third, hooping-cough in the succeeding year. The mist is common to all the agents; the human body, which also multiplies them, is common to all; why, then, is one developed to so intense a degree every now and then? Are there not some more latent conditions yet unknown, which favour the increase of one rather than that of another. This is a difficult question, and cannot be hastily considered; but on many accounts it appears to us necessary to answer it in the affirmative. We have merely alluded to it now, in order to link with it one very curious point about the influenza of 1847. Although unhealthy districts suffered much more than healthy in certain cases, yet there were some districts in which the mortality was much greater than in districts usually considered as unhealthy. In the five great divisions of the metropolis, the increase in the mortality in the six weeks following the 20th November was, for the western districts as 1; for the central districts as rather more than 2; for the east districts as 3; and for the north and south as 4.* Now the north and west districts are, in point of average mortality, nearly on a par; so that it is not, as in St. George's in the East, unhealthy conditions plus a fresh source of disease, but equally healthy conditions equally plus the fresh source of disease, which produce effects so dissimilar. We have nothing to guide us in coming to a solution of this "remarkable fact," as the Sanitary Commissioners term it. An analogous case was, that three of the suburban districts were more heavily visited even than the densest part of London.† Here, possibly, the neighbourhood of marshes may have had some effect.

Passing, however, from the local condition of the places which influenza chiefly ravaged, let us very briefly allude to the general atmospheric phenomena which preceded and followed its attack.

Influenza has so frequently been ushered in, in Europe, with violent meteorological changes, that some have been inclined to believe it entirely dependent upon them. When a retrospect is taken of its several epidemics, however, this is at once seen to be impossible. It has appeared in all seasons and in all climates, from Southern Africa, China, or Bengal, to Finland, Russia, or the two Americas. Dr. Peacock gives a summary of its several appearances in this country, and concludes with the remark, "that though the most frequent visitations of the disease have been in autumn, winter, and spring, they have nevertheless happened in all seasons, and under every possible variety of temperature and humidity."‡ This is no doubt correct, and yet it is singular enough how frequently its invasions in northern latitudes have been apparently preceded by intemperature of season. This is well illustrated in the elaborate record by Ozanam; and this great writer, while he expressly distinguishes the cause of influenza from atmospheric vicissitudes, evidently believes that there is some peculiar connexion between them.

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* Second Report of the Metropolitan Sanitary Commissioners, p. 24.

† Ibid.

‡ Peacock, p. 109.

Our present purpose, however, is merely to note the attendant phenomena of the epidemic of 1847. The following were those of London:

"In October the weather was unusually warm; the wind S.S.W.; a brilliant aurora was observed, and shook the magnets on October 24. On Tuesday, November 16th, there was a remarkable darkness; the wind changed to N.W. The mean temperature of the air suddenly fell from 11° above to 10° below the average; on Monday it was 54°; Friday 32°. On Friday night the air was 27°; the earth was frozen; the wind was calm three days, and on Saturday evening a dense fog lay over the Thames and London for the space of five hours. No electricity stirred in the air during the week." (Registrar-General's Report.)

There seems to be some doubt as to the exact day on which influenza commenced. At page 3, Dr. Peacock says about the 16th or 18th, but in the opening sentence of his work, he states that it may be considered to have become epidemic between the 6th and 22d. At Wanstead, eight miles north-east of London, it began on the 14th.* We were ourselves in a favorable position for observing cases, and we could not be certain that we observed any before the sudden change from warmth to cold on the 16th.

The above meteorological phenomena were common to the whole of England, but it appeared at very different periods in different places. In Bradford it was present on the 27th of November, in the south of England in December. The Registrar-General says it evidently pervaded the whole of England; in many places it appeared later than in London, in some it was so slight as hardly to attract attention, in others it was not seen at all.

In Edinburgh it burst out twice violently, on the 18th and on the 28th of November. In both cases it was preceded or accompanied by a dense thick fog, and a keen frost, which came on after a few days of very mild weather.†

The Registrar-General and Dr. Peacock give no account of its appearance in Germany or Russia. In Paris it appeared at the latter end of November and December; at Madrid in January; on the coast of Portugal and south coast of Spain in January and February; on the coast of Syria in April; on the west coast of Africa in July and August; at Hongkong in August; in Newfoundland and New Zealand in January, February, and March.‡

The influenza of 1847 thus traversed the globe. Its duration was in every place from three to eight weeks, and its symptoms, as far as we can learn about them in this country, France, and the Mediterranean, were very uniform in kind, though not in intensity.

In concluding this section it may be affirmed that our knowledge of the accessory causes of influenza, whether general or special, is at present imperfect. Sudden changes of temperature certainly appear to assist, and in the same way exposure to cold predisposes the individual. It is also apparent, that if it needs a predisposition, this is found in a state of body common to almost every one. We have already indicated the difficulties which attend the attempt to clear up the influence of local circumstances.

* Peacock, p. 3.

† Dr. Stack, quoted by Registrar-General.

‡ Returns in Sir Wm. Burnett's office, quoted by Registrar-General.

Many and accurate observations are yet needed, before conclusions can be safely arrived at.

3. The third section, viz. the possible nature and primary laws of the cause, we shall on this occasion dismiss in a very few words.[*]

It has long been known that epidemics of influenza pursued a certain course; that appearing usually in Russia, they passed into Germany, Scotland, and England, and then turned to the south-west through France to Italy. The first seat of the disease has been considered to be China, Tartary, or the adjacent part of Asia. Of late years, however, it has been considered that the most northern part of Russia is the fountain or birth-place of the disease.[†] Occasionally, however, it has pursued a retrograde course: and having apparently originated in Spain or Italy, has passed towards France, England, and then to Germany. The regularity of this course points evidently to the action of some force or power, which, independent of the "catarrhal constitution" which seems to favour it, impels it from place to place. The rapidity of its movement has been very variable in different epidemics, but it has never been otherwise than exceedingly rapid. The particles which compose its active principle are of such extraordinary volatility and lightness, that they pass with streams of air easily over the broadest seas; as the fine particles of red sand which the trade winds take up from the African deserts will redden the sails of a ship, hundreds of miles from the place where they were first lifted into the air.

Although not confined to Europe, and although there seems to be evidence that it has originated independently in America, in Hindostan, and in southern China, there can be no doubt that it is a disease especially of the higher latitudes.

The poison can exist in the hottest countries, but it seems not willingly to enter them, and it soon dies away. Although known in India, there is no question that, compared with Europe, it is, as a general epidemic, seen there comparatively seldom. As to its severity and mortality in different countries, we are yet not sufficiently informed.

Whether or not this poison is, as that of smallpox or measles, capable of increase and multiplication during its passage through the body, is even now not determined. But it cannot be supposed that if so, this phenomenon will explain its progress and extension.[‡] Would any communication between individuals cause it to take this regular course time after time, to attack half a community at once,[§] to have a stated duration of from four to eight weeks in each place; and in the intervals of its prevalence nearly or entirely to disappear?

It is an interesting point to inquire whether this poison or power is an organic body, or simply an atmospheric element in some peculiar allotropic state, or whether it needs some atmospheric condition for its diffusion, or is connected in some way with the electricity of the earth and atmosphere. We shall, however, defer these inquiries, as we are anxious to devote our few remaining lines to a parallel between influenza and the disease which most closely resembles it, viz. Asiatic cholera.

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[*] All points connected with influenza are excellently discussed by Dr. Holland, in the Medical Notes and Reflections.

[†] On this point, Schönlein (Allgemeine und Specielle Pathologie, &c., Zweiter Theil, p. 102) expresses himself positively: "Die Krankheit beruht auf einem bestimmten Contagium. Der Ort, wo die Krankheit in Europa sich bildet, ist der hohe Norden. Die Krankheit bildet sich nur bei einer bestimmten atmosphärischen Constitution, die wir als catarrhalische bezeichnet haben." The word contagion means merely a specific poison.

[‡] It is singular enough to read over the arguments of Haygarth and others, who thought influenza contagious, and argued this opinion with no little ability. Haygarth's arguments are very much like those of some writers of the present day, for the contagion of cholera. (See Haygarth's paper, in Falconer's Account of the Epidemic Catarrhal Fever of Bath in 1803, pp. 44-5.)

[§] At St. Petersburg, in 1782, 40,000 persons were attacked in one day, immediately after an extraordinary fall of the thermometer. This epidemic was called La Russe throughout all Europe.

In comparing these two diseases, it is necessary to remember that their habitats are not the same, and that though each may spread over the world, yet that cholera is, for the most part, an affection of tropical climates, and influenza of the colder regions. Cholera in Asia must then be compared with influenza in Europe.

Each malady seems to originate in several places in the localities favorable and native to it. Influenza most commonly in Northern Europe, or in North America; cholera in the plains of India and of China. Each, when produced, is endowed with a power of movement, and passes over more or less of the space which may be considered as the ordinary sphere of its action; at intervals it passes over the whole world, or over a great part of it. Each then pursues a nearly identical track, and is probably therefore impelled by an analogous force. Each, in its purest course, viz. in that followed in its proper locality, and with the fewest disturbing circumstances, has a rapid development to an acme, and then a gradual decline. Each seems to act primarily on the blood, and secondarily on particular mucous membranes; in the one case, on that of the nasal and pulmonary organs; in the other, on that of the digestive canal. Each may occasionally be reproduced by the human body, but neither can possibly owe its diffusion to this single property.

So far these are important resemblances; the differences are no less important.

The cholera poison is infinitely less volatile or less diffusible; consequently, perhaps, its movement is slower; it is influenced in an extraordinary degree by soil and conditions of locality, and is capable of being detained in places distant from its habitat for a variable time. Although it affects all nations, it attacks only few individuals, for it requires a peculiar predisposition. It recurs much less frequently than influenza, and may even apparently be absent for a long term of years.

Although most closely allied to influenza, cholera shows also closer marks of alliance to diseases produced by other morbid poisons, as to yellow fever, and perhaps to typhus, than influenza exhibits. Both the Registrar-General and Dr. Peacock allude to the coincidence between the appearance of influenza and cholera. This is a very interesting point, although, from the frequent recurrence of influenza, it may be accidental.

But, after all, many more facts must be accumulated, before the alliances and distinctions between influenza and cholera can be properly determined. It is, perhaps, hazardous even yet to draw a parallel, although the attempt to do so cannot be considered unphilosophical. Of one thing we are certain,—that if the laws which govern any morbid poison can be distinctly made out, we shall have a much better chance of knowing in what direction to look for the laws which govern any analogous agent. Now, influenza returns so frequently, that proper investigations can easily be made, and would infallibly lead to conclusions of great interest and value. We shall hope, then, that the next epidemic will find us ready prepared systematically to recognise the phenomena. And among those to whom we shall look forward for future contributions on this subject, we must place the author of the dissertation on the present epidemic. We have differed from Dr. Peacock on some points; but as we hope that, in his useful and honorable career, he may live to record many epidemics of influenza, we shall have future opportunities of showing him that we are not too stubborn to be convinced of being in error, if he brings additional evidence to bear on the points of difference.

ART. V.

1. *Lectures on the Causes and Treatment of Ulcers of the Lower Extremity.* By GEORGE CRITCHETT, Esq., F.R.C.S., Surgeon to the Royal London Ophthalmic Hospital, Assistant-Surgeon to the London Hospital, and Lecturer on Practical Anatomy, &c. &c.—*London*, 1849. 8vo, pp. 121.
2. *A Treatise on the Cure of Ulcers by Fumigation, in which a Rational Treatment is deduced from the Physiology of Ulceration, and Proofs afforded that the New Method produces more Speedy, Certain, and Permanent Effects than any other in General Use, &c. &c. &c.* By GEORGE ALFRED WALKER, Surgeon, Member of several Learned and Scientific Societies, and Author of 'Gatherings from Grave-yards,' &c. —*London*, 1848.

AFTER perusing the title-pages of these two works, our readers will not be surprised to learn that, though treating of the same disease, ulcers of the lower extremity, they differ as widely from each other as if they referred to totally distinct subjects. In one circumstance, however, they do resemble each other: both their authors commencing by regretting the imperfection of existing methods of cure, and both of them expressing a confident hope that they are adding something to our stock of knowledge. Beyond this there is no parallel between them. Mr. Critchett adopts a method of classifying ulcers; Mr. Walker considers "all ulcers (not specific) under one common head." Differing so greatly as they do, we must notice each treatise separately.

At the request of the pupils of the London Hospital, Mr. Critchett, the assistant-surgeon, undertook to deliver a course of lectures on ulcers of the lower extremity, which were published in the 'Lancet,' and are now collected together in a very readable and interesting volume. The main object which Mr. Critchett has had in view, has been to advocate the use of regular strapping and bandaging from the toes upwards in most of the forms of ulcer, and in so doing, to place "the principle of mechanical support in the treatment of ulcers of the lower extremity on a correct and scientific basis." In this he does not profess to be original; to quote his own elegant and striking language, "he claims not the high honour of having discovered a new window whereby light may be admitted into our surgical temple, but simply to have rendered transparent one that had become obscured and useless." Our author divides all ulcers into two great classes—simple or local, and specific or constitutional. Each of these is again subdivided into varieties: thus, the simple or local class includes the acute or spreading ulcer, the subacute, chronic, healthy, irritable, and varicose; whilst the class of specific or constitutional ulcers comprehends the strumous, syphilitic, phagedenic, periosteal, menstrual, œdematous, and malignant.

Each of these different forms of ulcer is graphically described by Mr. Critchett, and their treatment scientifically discussed. It may suffice, however, to state that all of them are capable, at some period or other of their progress, of being benefited by regular mechanical support. Concerning the share which Mr. Baynton had in the introduction into practice of the plan of strapping ulcers, Mr. Critchett makes several statements, which briefly resolve themselves into the following:

1. That Mr. Baynton advocated the practice on a false principle—that of bringing the edges of the ulcer nearer together by means of strips of adhesive plaster, instead of doing so to facilitate the return of blood through the limb; 2d, that he limited his plan to *old* ulcers; and 3d, that by applying his strapping merely to the surface of the sore, and to two or three inches above the diseased part, he very imperfectly realized the advantages of mechanical support, and in many cases did a great deal of harm, inasmuch as his strapping often acted like a tight garter, and directly impeded the circulation through the limb.

On these accounts the credit due to Mr. Baynton must be very considerably limited; and we must here seek the explanation of the fact, that some surgical authorities reject the proposal altogether, while almost all in recommending it speak dubiously, and inculcate great caution in its employment. The full advantages of mechanical treatment can only be reached by affording complete support to the entire limb, by strapping and bandaging it from the toes up to the knee, and occasionally beyond this, in a manner very clearly described by Mr. Critchett. The credit of this improvement is due to Mr. John Scott, and is described in his work on the treatment of diseased joints; and the London Hospital is the only institution, we gather from Mr. Critchett, where this method has been understood and properly carried out. To this statement we must take exception. It is probably true that Mr. Scott was the first to give a *written* account of this improvement on Mr. Baynton's proposal; but with respect to its application, we can assure Mr. Critchett that it is a plan which, to our own knowledge, has been effectually, fully, and efficiently carried out for years past in some of our provincial hospitals; the applicants to which, from among a badly-nourished and over-worked population, are more than ordinarily subject to ulcers of the legs. This is not perhaps a matter of much consequence, and is not stated with a view of detracting from the value of Mr. Critchett's labours, but rather as affording confirmation to them, in the shape of independent testimony to the value of the treatment he recommends.

As affording a good example of Mr. Critchett's style, and also as containing in itself some very valuable information, we subjoin his remarks on the acute form of ulcer, extracted from the third lecture.

"The acute form of ulcer is almost invariably attended with severe suffering, which is aggravated to an intolerable degree when the limb is placed in a depending position; there is a sensation of great heat and tension, and the pain is of an aching gnawing character. The ulcer looks uneven and glassy, the edges are irregular and undefined, sloughs varying in size are often seen on the surface; the discharge may be slight or abundant, but it is invariably thin and ichorous, sometimes mixed up, however, with the débris of the ulcer; the surrounding parts are of a bright red colour, or sometimes of a peculiar speckly red-and-white aspect, difficult to convey in words, but very characteristic of this condition: the swelling may be slight, or it may extend over a considerable portion of the limb. On making continued and firm pressure upon the parts in the neighbourhood of the sore, they pit, showing a state of local œdema, which I have almost invariably found to exist in acute ulceration of the lower limb, and which is very rarely found in the subacute and chronic stage, except when there is general anasarca—a condition of things which does not belong to the present inquiry. Some authors speak of applying pressure to get rid of the œdema of old ulcers; I can confidently affirm that such a condition rarely, if ever, accompanies chronic sores; it is a sure indication of acute inflammation and ulceration, and becomes a very useful and unerring guide in practice, more particularly in determining the point as to whether the limb will bear mechanical support, inasmuch as wherever this state is found, support is contraindicated. The bright red colour of the surrounding parts is another important indication; when these inflamed vessels are pressed upon and emptied, they refill with such rapidity that the interval is scarcely appreciable. It will be useful to contrast this with a similar experiment during the chronic stage, which I shall hereafter allude to. I dwell upon these diagnostic marks, because they are most important in practice. Any attempt to apply mechanical support during the acute stages of ulceration is invariably attended with most injurious effects, and has a tendency to bring a remedy that is of the utmost value when judiciously selected into unmerited discredit. In regard, then, to treatment: the recumbent position must be constantly maintained, and is in general the only one that can be borne; additional benefit is sometimes derived from the use of the inclined plane, by which the limb is elevated more or less above the rest of the body, thus facilitating still further the return of blood through the limbs. Soothing applications are the best suited for this condition; water dressing, either warm or cold, as is most agreeable to the patient. Poultices of various kinds (though it is the fashion for young surgeons to condemn them wholesale) often afford relief when all else fails to do so, and are still, I am convinced in some cases—in spite of the obloquy and ridicule now cast upon them by the modern school—by far the best method of obtaining uniform warmth and moisture to an inflamed surface. If the sore be foul and sloughy, a solution of the chloride of lime, or soda, is a useful application; the strength must be regulated by the feelings of the patient, the object being to produce a slight tingling sensation, not amounting to pain. I generally commence with half an ounce of Fincham's solution of the chloride of lime to a pint of water; in foul, angry, sloughing sores, I have met with no application so useful; it rapidly removes all unpleasant odour, and produces a healthy granulating surface. In ulcers of an acute character, with a considerable amount of surrounding inflammation, some surgeons recommend local depletion, applied to the circumference of the sore, either by means of leeches or small lancet punctures. I do not recommend this plan of treatment. I do not deny that occasionally temporary benefit and relief from pain may result from it; but, on the other hand, I have so frequently seen cases in which these artificial wounds, slight though they seem, have become troublesome sores, and in which no improvement has been produced in the original ulcer, that I am convinced that the possible advantage is very much more than counterbalanced by the probable injury that may result from this plan." (p. 39.)

The following passage occurs further on:

"Mr. Skey's pamphlet, emanating from so high a quarter at St. Bartholomew's Hospital, promised such great things, and with so much confidence, from the use of opium in the treatment of ulcers, that I was surprised to find, after a prolonged and extensive trial, that in no one instance was I able to trace the slightest benefit from its use. I have carefully administered this drug in above forty cases of the kind, and I have been compelled to arrive at the conclusion that it is utterly valueless as a healing agent in the treatment of ulcers of the lower limb." (p. 51.)

In this, and in a statement made further on, that he has not found black wash useful unless the ulcer were a specific one, Mr. Critchett's experience differs from our own; and of the *taste* with which the passage we have above quoted has been written, we cannot altogether approve.

We have not space for copious extracts from this work; but we feel it incumbent upon us to express our dissent from Mr. Critchett's doctrine respecting the menstrual ulcer. His axiom is "Heal the ulcer, and the uterine function will speedily be restored to health and regularity." Common sense and common experience teach the opposite of this. Here our author is riding his hobby a little too hard.

To conclude our notice of Mr. Critchett's work, it only remains to express the gratification which its perusal has afforded us. As an example of patient observation, legitimate induction, and praiseworthy diligence, it is deserving of high commendation; and as a manual of practice, and, above all, as a model of the true and enlightened mode of conducting an original investigation, we can cordially recommend it to students, for whose benefit principally it has been written.

Mr. Walker's work, which rejoices in a crimson cover, and a large gold label, "Geo. Alfred Walker, on the Cure of Ulcers," is about the same size as Mr. Critchett's, but presents greater pretension to originality. This credit, to which it is fairly entitled, it will probably long retain. We do not think the author need be under much apprehension of the envious pilferings of the would-be discoverer; his claim to having produced an original book is undoubted.

The "application of the fumes of sulphur and iodine to the surface of the ulcer, and to the adjacent tissues,"—a process to be effected through the medium of mahogany boxes of a peculiar construction, which communicate with the "engine-room" of Mr. Walker's establishment,—has produced results beyond the expectation of the most unreasonably sanguine. For the details of these cases, and for some remarkable drawings which illustrate them, we must refer our readers to the work itself. They truly afford examples of what may be called *surprising cures*. We have before hinted that Mr. Walker thinks very little of the ordinary treatment of ulcers; and he considers that we are all wrong, when we rejoice to see the surface of an ulcer covered by granulations. "They are," he says, "the product of an artificial stimulus, and spring from an ill-conditioned soil." Their unmanageableness affords an opportunity to introduce the following elegant and appropriate simile: "Between stimulants to elevate, and escharotics to depress, the surgeon finds himself nearly in the same predicament as the perplexed *petit maître*, who employs straps to keep down his inexpressibles, while he has recourse to braces to keep them up."

Mr. Walker is not fond of poultices—but our readers shall judge for themselves of the forcible style in which his book is written. "Linseed meal is used in very considerable quantities in many hospitals, for making that disgraceful, filthy, and non-scientific combination called a poultice—a mess better suited for the stomach of a pig than for the leg of a human being."

There's no mistake about this. Dear reader, is it not an *original* book?

ART. VI.

Nosologia Positiva. Scritta da VINCENZO LANZA. Tomo III, IV.—*Napoli,* 1845-7. 8vo, pp. 568, 696.

Positive Nosology. By V. LANZA. Vols. III and IV.—*Naples,* 1845-7.

IN the Forty-fifth Number of the 'British and Foreign Medical Review' will be found a critical analysis of the first and second volumes of the work before us, which contains a summary exposition of the general doctrines of the author. The third and fourth volumes, since published, are almost entirely practical; being the application of the general principles dealt with in the former portion of the work to various forms of disease, and indications for treatment.

When a man has confessedly held the first place in his profession in a large city for as many years as Dr. Lanza has done in Naples, it would be difficult for him to write a practical work destitute of interest, or undeserving attention. His knowledge of morbid anatomy might not place him among those whose labours have almost perfected the physical diagnosis of disease during the last half century. He might even be unacquainted with the most important of their discoveries. His observation of disease, although most extensive, might be deficient in precision; he might generalize from facts, the universality of which remained unestablished; his theoretical writings might evince originality, ingenuity, and profound thought, while they failed to convey to the reader any clear or definite addition to his previous knowledge; even in practice, or rather in practical inferences, a somewhat inconsequential habit of thought and reasoning might be traceable; but the recorded results of this practice and practical inference would still possess considerable interest and importance from the peculiar position and circumstances of the author.

These remarks apply with more or less force to Dr. Lanza and his writings. He has for many years been the first practising physician in Naples, and the only Italian consulted by English medical men residing in that city. Until very lately he was professor of practical medicine in the University; but at present he is suspended by government, being "sotto giudizio" as ex-president of the late chamber of deputies, and one of the leaders of the liberal movement in Italy. He is upwards of seventy years of age; and during the greater part of his professional life has been clinical professor in different Neapolitan hospitals. As a lecturer he is said to be fluent and forcible, and as a bedside physician, remarkable for the undefinable quality called *tact.* Notwithstanding an extreme disregard to personal appearance or conventional propriety in the article of dress, his aspect, if not that of a man of genius, is still that of an acute and able man—one both of thought and action. His years sit lightly on him; his memory is as exact, his professional enthusiasm as great as ever; and in a visit we recently paid him, his eyes sparkled with delight when he spoke of Sir James Clark's visit to Naples, thirty-four years ago, bearing with him the sulphate of quinine, then a new medicine, and of the first trials of its efficacy which the two old friends conjointly made.

It is now time, however, to turn from the author to his book, and we do so with the intention of laying before our readers rather a summary of its contents, than anything like a close or critical analysis of them. The third volume opens with a section upon contagious diseases, in which the doctrine is ably supported that these diseases depend upon specific and distinct morbific causes, or morbid poisons. Several forms of contagious disease, of which Dr. Lanza has no personal experience, are passed over, and in the present volume he confines himself to the exanthemata, and some other species of what he calls *transitory contagions*. The regular course, period of latency, protection from a second attack, and definite termination of these diseases, are pointed out; the various modes of transmission, and the relations between the specific and ordinary causes of disease, are considered at length; the general practical conclusion being, that as we are not possessed of a specific remedy for any of the exanthemata, or for any transitory contagion, nor of any remedy which is certainly known to correct or mitigate the violence of the poison or to expel it from the system, any preparatory treatment during the existence of an epidemic must consist simply in means which tend to promote general health. Simple palliative measures may be employed if required by any unusual symptom or local lesion during the course of the disease, remembering that while art neither can nor ought to do anything to determine to the skin, or what is called "to bring out the rash," still, when the eruption has appeared, great care must be taken that it is not checked or thrown back; those who have interfered in either way, having been taught by experience that the patients they had most neglected recovered far better than those most assiduously or officiously attended.

Variola is the subject of the succeeding chapter, and is exceedingly well treated, with all the minute practical observation which characterise the writings of Sydenham on the same disease. A curious fact is stated with regard to the coexistence of two specific diseases at the same time in the same person; the author having seen variola attack a patient who was at the time in hospital under treatment for a well-defined tertian intermittent fever. One disease did not obscure or interfere with the other, and both went on to a successful termination. With regard to treatment, Dr. Lanza says that for several years past very few cases have occurred in Naples; but his former experience and study have convinced him that the most opposite methods of treatment are followed by a very similar amount of mortality, the disease under all circumstances completing its due course; and that it is far more mild in those least officiously medicated; poverty thus frequently injuring less than polypharmacy. Bland, unirritating diluents, and a regulated diet, are all that is required in the treatment of a simple case of variola. When inflammation or congestion of an internal organ occurs, it is to be treated in the same manner as in other acute febrile diseases; and in the gangrenous form, or in that attended by great nervous depression, Dr. Lanza thinks that no method hitherto adopted has been successful either in the hands of himself or others.

In a chapter on vaccination which follows, the author states, from personal observation, that when a cow is inoculated, and pus resulting from this inoculation is used upon the human subject, inflammatory or febrile action follows, and a deranged state of general health, resembling that so often observed after the exanthemata.

In the treatment of measles, while recommending diluents, the author insists upon the danger of *cold* drinks, and keeps his patients six weeks in their rooms, a practice few would be inclined to submit to in England.

The seventh chapter is an important one upon the petechial typhus of Naples, the description of which is most minutely given; but our space prevents us from citing more than the most remarkable statements. Dr. Lanza says, he has seen this form attack persons suffering at the same from well-marked intermittent; and in such cases obscure, although not altogether conceal, the periodicity. In 1817, 1828, and 1830, the contagious nature of this disease was abundantly proved by the effect of segregating patients, and by the numbers of their attendants attacked. Dr. Lanza believes that, under the superintendence of an active medical police, the ravages of typhus might be reduced almost to a nullity. The treatment is not detailed, the author referring to his general doctrine laid down in the second volume; but he follows a simple expectant system, giving diluents, watching any untoward symptom which may arise during the inevitable course of the disease, and combating it by the mildest means. He says that public and severe experiments made in the Clinical Hospital have clearly proved that, under treatment without any medicine whatever, the disease terminated much more favorably than when many drugs were employed; and that comparing the results of the practice of the polypharmacologists with those of the simple method now generally adopted in Naples, it is easily understood how the petechiæ, which thirty years ago were regarded as a sign of certain death, at present excite comparatively little alarm, the diseases of which they are the evidence being known to generally terminate favorably.

A chapter on aphtha follows; Dr. Lanza stating that in Naples it is observed as an acute febrile disease, of which the papulæ, pustules, and ulceration of the mouth are the sole pathognomonic signs, or anatomical character. He has been convinced that it is contagious, and has never seen any one attacked by it a second time, that is to say, when it has been a primary disease; the secondary aphthous ulcerations about the mouth, which occur so often in the later stages of chronic diseases and in phthisis, very frequently reappear. In the treatment, he regards local applications as cardinal remedies, with which almost any medicine appears to succeed, and without which all are useless. He mixed from one to two drachms of the nitrate and sulphate of potass (sal prunello) with an ounce of syrup of mulberries or *mel rosato*, and has the whole of the mouth covered with this mixture at least every hour. At first the application is painful, but it is soon borne with indifference. Acid drinks are useful. For diet, asses' milk and barley-water sweetened. If aperients are required, clysters of water with olive-oil should be given; if diarrhœa is troublesome, they should consist of starch or mucilage.

The tenth chapter is on the *Orecchioni* of the Italians, our cynanche parotidea, or mumps. Dr. Lanza believes that in certain seasons and places this disease arises spontaneously, and that having so arisen it may become contagious, and that convalescents often convey it to their schools, to the theatres, churches, or other assemblies. He has never met with a person who has suffered from it more than once. A mild diet and warmth must be prescribed during the course of the disease; and as it subsides, a purgative is useful with some simple means of exciting perspiration.

Dr. Lanza is firmly convinced that *hooping-cough* is contagious, and he has never seen a person who had suffered from it twice. In the treatment he insists that "the sole and only remedy for the hooping-cough is coughing," and that every method of suspending or checking the cough is always injurious. The principal trust must be in a strict hygiene, and in simple cases time and nature effect the cure. In cases of very severe convulsive cough, with danger of hemorrhage, hernia, &c., Dr. Lanza says he has generally found it sufficiently easy to moderate the urgent symptoms, by merely directing the patients to leave off the remedies they were taking with the view of checking the cough, and to drink freely of mucilaginous or saccharine fluids with milk. In cases complicated with other diseases, the rule is to treat such diseases without regard to the pertussis.

Scripsi quæ vidi is the becoming motto of a chapter on the cholera in Naples in 1836 and 1837, in which a most elaborate account of the symptoms is drawn up from the observation of 300 cases of the most severe forms of the disease treated in the clinical wards. We need not pause here, but proceed to extract the following sensible passage on the statistics of this disease:

> "Statistics, so useful and even necessary to the progress of the science of other diseases, have been hitherto useless with regard to cholera, and the cause of the defect we believe to be this; that the cholera, from its lowest to its highest degree, passes through innumerable intermediate grades, and according to these its curability or fatality varies immensely. Thus every one sees, that if it be not first established how statistics can be formed on equal grades of the same disease, the proportion of deaths and cases must vary as greatly as the grades do in inequality.
>
> "We cannot supply this defect better than by separating the cases into four classes. The first consists of cholera patients with laborious respiration; and of these we did not see a single cure by ourselves or others. The second includes patients completely in the cold stage (*con pieno algore*), with imperceptible or almost imperceptible pulse, and of these we lost 80 per cent. . . . . . . The third class comprehends cases in which choleric matter was abundantly evacuated, vomiting, diarrhœa, epigastric angina, and all or almost all the remaining choleric symptoms, especially the spasmodic, as sighing, cramps, aphonia, ischuria, &c., were present, but in which the cold stage and cessation of pulse were not complete; and of such cases the deaths were nearly 10 per cent. In cases of the fourth class the choleric matter was small in quantity, not bad in quality, and transitory, that is to say, speedily followed by common bilious matter; and of such cases not one died.
>
> "From this explanation every one may see how it happens that the statistical returns of different physicians in various countries, or in the same, present such opposite results. However, laying aside the returns which give results too successful or unsuccessful as unaccountable, and confining ourselves to those which present medium results and agree with each other, we may believe and assert that, uniting the patients of all the above four classes, the cholera attacked a tenth part of the inhabitants of Europe, and half those attacked died. This agrees with observations made in Naples." (Vol. iii, pp. 198-9.)

Naples suffered from two visitations or invasions of cholera, the first from October to March, the second from May to September, and Dr. Lanza states, that in the first invasion, it was most prevalent in the low, crowded, dirty portions of the city; but in the second, it raged in the most clean, salubrious, and best-ventilated districts, and made such progress that at last the fatality was almost equalled.

It is a curious fact that worms, of the species Tricocephalus dispar, were very constantly found in the intestines, especially in the cæcum and first half of the colon, not only of cholera patients, but in those who died of other diseases during the prevalence of cholera, and were not found after the cessation of the epidemic. The frequent appearance of these worms at that time Dr. Lanza thinks could not have been altogether accidental.

> "Our patients morally and physically supported cold better than heat; they took cool or cold drinks with appetite, and disliked warm; they liked a moderate and rather cool covering to the bed, and generally detested external calefacients; they desired gentle friction, especially of the limbs affected by cramps, and very generally wished for a bath of moderate temperature. At first we were in favour of warmth, but experience convinced us that the same number recovered and died, whether treated by cold or heat; so that in the temperature of drinks, coverings, and the bath, we followed the instinct of the patient, and found this by far the best counsellor. Extreme cold, and the cold bath, we found not less injurious than too great heat. Fasting was tolerated in an extreme degree, and was extremely beneficial. The tolerance of remedies is so extremely obscure, that throughout Europe, as in Naples, so many different methods and specifics were in vogue, exalted by vulgar credulity and denied by experience, sometimes with the death of the inventors.
>
> "All polypharmacy, specifics, and meddlesome therapeutics appeared to us most injurious, and at last we were constrained to agree with the public belief, that of the deaths in cholera one third might be referred to the disease, one third to the tardy assistance afforded to the patient, and the remainder to the rewarded empirics of the day.
>
> On the whole, we conclude that experience has taught us that cholera, like all acute contagious diseases, destroys a certain number of victims in defiance of all human art; and yields the smallest number to art which is the least officious, not polypharmaceutic or precipitous, and which moderates (blandisca, caresses) the disease and assists nature.
>
> "The cholera is as much irritated by drugs as it is mitigated by hygienic measures and local applications. And we are certain that the treatment without drugs certainly affords the smallest proportion of deaths, nothing more being given than remedies which can do no harm, and may hasten the appearance of favorable symptoms, and not altogether check, but only impede or moderate, the unfavorable. With this intention we propose our plan of treatment, and it may excite wonder perhaps, that while so many physicians have proposed so many remedies, we assert that the best remedy is the least physic." (Vol. iii, pp. 210-11.)

With regard to any preparative or preservatory treatment during the progress of an epidemic, Dr. Lanza observes that the grand point is to avoid dyspepsia; and for this purpose, not to abstain from any accustomed article of food, but to observe strict moderation in quantity and care in quality. At Naples, it was observed that those suffered most who had restricted themselves to certain articles of diet, and especially those who persevered in eating only farinaceous substances and beef. Perfect cleanliness is the best disinfectant; he very soon abandoned the use of the articles so called, as they did not preserve from the disease, and were often injurious to those who used them.

We need not detail what is said of the treatment of the different stages of cholera, as the plan of the author was simply to interfere as little as possible with the disease, and to place the patient in the most favorable circumstances during its inevitable course. He gave a saline mucilaginous drink freely composed of the liquor ammonia acetatis with barley-water, and an occasional emetic of ipecacuanha, avoiding stimulants, and keeping the patients on very low diet. We give his concluding remarks, not omitting the last rather amusing sentence.

> "Having seen the intolerance and inutility of remedies, not only pharmaceutical but also surgical, we were convinced that the whole treatment ought to be to confide in hygiene. That is, during the epidemic, sparing and severe regimen, both in food and drinks, and in other *non-naturals;* but after the epidemic, more liberality than ever, not only indulging, but exciting the appetite for food and drink: exercise, open air, diversion, no attention to the quantity of excrements, little fatigue, and a perfect return to former habits in which the person had enjoyed the most robust health. With this regimen we have seen the use of a grain or two of ipecacuanha, in the morning or before dinner, beneficial." (p. 224.)

We have now arrived at the eighth book, that on the *Sarconosi,* or diseases of the *flesh,* in which alteration of sensation and motion are not so much the predominant signs as structural alteration of some tissue or organ. The author combats the doctrine of those who look to structural alteration as the primitive essence of all diseases; and his object is to show that consumptive diseases, as he calls them (*morbi di consunzione*), possess certain peculiarities of great practical importance, which distinguish them not only from other diseases, but from each other. He commences by pointing out what he considers to be the distinction of every consumptive disease into common or cachectic, and into tubercular or tabid; or, as we should perhaps express it, as emaciation produced by common causes, or by the effects of tubercular deposition. His chief point is the presence of fever, in the one case of a peculiar type, and not in the other. The chief characteristics of this form are great velocity of pulse, pungent heat of skin, no corresponding depression of the patient, who is often unconscious of his feverish state, good appetite and digestion before the organs of nutrition are attacked, and increase of fever by stimulants, while it is not diminished by lowering means. We do not stop to inquire how far such symptoms are trustworthy in diagnosis before the physical signs of tubercle become evident, but proceed to explain the author's views of the nature of tubercular matter. He argues that it is not a simple effusion or separation of morbid matter from the blood, but that it consists of a species of phytozoa which he calls tabific (*fitozoi tabifici*), and which are special and peculiar beings or growths (*singolari nascenze*), not depending upon common causes of disease, but upon one unknown special cause, which differs altogether from any other. He argues that scirrhus, fungus, hematoid, and encephaloid, are all phytozoa, which have certain relations with each other, and with tubercle, possessing a special anatomical and physiological existence, being implanted in certain tissues, at the expense of which they live, and which they injure or destroy.

Consumptive diseases are divided by Dr. Lanza into five species: hepatic, mesenteric, nervous or dorsal, mucous or gastro-enteric, and pulmonary. Consumptive disease of the larynx, trachea, pancreas, spleen, uterus, bones, and articulations never kill of themselves, but first become changed into one of the above five divisions. Any one of these five may arise and kill by itself, although most of them before death become converted into pulmonary consumption. It will be at once evident that the above propositions might be expressed in more precise terms, and in language more in accordance with modern pathological writings; but we wish to convey the meaning of the author in his own way, and therefore give his own words, or their nearest equivalents in our language.

"Consumption of the liver, commonly called *hepatic tabes,*" is the title of a long chapter, which does not contain much important matter, or the kind of practical information we expected to find. The symptoms and signs of tubercular deposit in the liver, as distinguished from those of common hepatitis, are the very points upon which a man of large experience might be supposed to be able to define with more or less accuracy. Dr. Lanza merely says that they are identical, modified, however, by the slowness of the disease, and the emaciation of the patient. Less acute pain, more diffused swelling, and less active fever are the chief grounds of his diagnosis.

"Mesenteric consumption, or tabes mesenterica," is treated principally by hygienic means; but Dr. Lanza says that hemlock is very useful in doses of a few grains of the extract, or from a quarter of a grain to two grains of the seed,—friction with the diluted extract upon the abdomen being performed at the same time. The iodide of potassium has been useful in some few cases; the chloride of gold was tried without success, while arsenic, in doses of $\frac{1}{100}$ of a grain, appeared of the greatest service. A mineral water of Ischia, which contains lime and iodine, has also a great reputation.

"Gastro-enteric consumption, or tabes mucosa," as described by our author, is merely the result of chronic inflammation of the gastro-enteric mucous membrane, and not any special condition dependent upon tubercular deposit into or upon this membrane. In the treatment, Dr. Lanza advises arsenious acid in doses of $\frac{1}{100}$ of a grain in an ounce of distilled water.

"Sarconosi of the nervous centres and ganglia, commonly called nervous consumption, or nervous tabes," as far as we can learn from Dr. Lanza's description, appears to be a morbid condition of the whole system, resulting from any alteration from the healthy state of the nervous centres and ganglia, from simple congestion to the most profound degeneration. He does not pretend to predicate, from the symptoms, the particular seat or nature of organic alteration; and his treatment is simply hygienic and palliative. The principal symptoms of the condition he describes are defective nutrition and consequent emaciation, absorption of fat from the orbits, muscular debility, sensation of emptiness in the head, with weight and dull pain, diminution of mental power, excited imagination, unpleasant sensations, fatigue from study, annoyance from trifles, indolence, or general indifference to all things. Chills and heats alternating between the spine and epigastrium; suffering about the seat of the semilunar ganglion; hunger and thirst often excessive. Spasms, tremors, wandering pains, palpitation, febrile pulse, intestinal meteorism, great irregularity in the excretions, and frequent spermatorrhœa. When any particular nerve, or part of the nervous centres, is affected, special signs are superadded, as tic douloureux or sciatica, amaurosis or tetanic convulsion, monomania or fatuity. All this may be unaccompanied by fever; but when fever supervenes it is habitual, not transitory or accidental. For a time such a condition may continue, without evident local or organic disease; but it is very rare that death occurs before some of the other forms of consumption show themselves. Such is the information we have gathered from this chapter, and we leave it to some of our readers to pursue the inquiry into the symptoms which distinguish primary lesions of the nervous system, and to endeavour to determine the conditions upon which deranged nervous function depends. It is too much the fashion to regard nervous disorders as secondary effects of derangements in the digestive and circulatory organs; and we are convinced that the very reverse would often be found to be the case, dyspepsia, defective nutrition, deranged circulation, and their attendants being in very many cases entirely dependent upon deranged nervous function, and this upon physical changes in the nerves or nervous centres, which, though appreciable, are generally unnoticed.

"Pulmonary consumption, or phthisis," is divided by Dr. Lanza into five species: mucous, glandular, parenchymatous, ulcerous, and tubercular. He considers that it is of great importance in practice to clearly distinguish these varieties from each other, but we can scarcely gather from his remarks in what the diagnosis consists. He does not say whether he considers the mucous variety as emaciation dependent upon chronic bronchitis; or whether it is a condition in which tubercular matter being formed in the blood is thrown off from the surface of the mucous membranes. The symptoms he describes, as far as regards cough, expectoration, dyspnœa, hæmoptysis, pain, and emaciation, are simply those of chronic bronchitis. He adds:

> "The fever in mucous phthisis is very peculiar. It shows an evident accession in the evening, with excessive heat, cold, depression, and sadness. The heat increases in the first half, and diminishes in the second half of the night. Slight, partial, and inconstant perspirations succeed in the morning, and then the fever has a marked remission. In the remission, the axillæ, groins, popliteal spaces, the affected part of the chest, and sometimes the abdomen, remain rather warm; the rest of the body is of natural coolness, and the palms of the hands and soles of the feet are frequently moist or perspiring. The pulse is weak, rarely a little tense, rather small, and not very fast. The patient fears phthisis, but is easily persuaded that he will speedily recover. The excretions, as we have said, abound in mucus, and generally recovery is modified by affection of the gastro-enteric mucous membrane. Indeed morbid diffusion always occurs, varying, in different cases, to the supra-diaphragmatic or sub-diaphragmatic mucous membranes. Thus in mucous phthisis we often observe lachrymation and tendency to weeping, sneezing, alterations of taste and smell, white fur on the tongue, difficult deglutition, catarrhal voice, &c. In addition, anorexia, thirst, every kind of difficult digestion, extreme and troublesome flatulence, constipation alternating with mucous diarrhœa, cloudy urine, leucorrhœa, œdema, &c. . . . . . Pure and simple mucous phthisis is a very chronic disease, and subject to acute exacerbations under the form of bronchitis, pleuritis, or rheumatic pneumonia. These are rare in the early stages, and generally occur at the change of season, especially in autumn. In the intervals the disease is so much quieted, that the ignorant believe themselves cured. But the very mild fever, the first degree of consumption, the persistence, although obscure, of thoracic symptoms, and the cachectic habit of body, show the hidden existence of the disease." (pp. 397-8.)

As the disease progresses it becomes complicated with some other form of consumption. It will be observed that Dr. Lanza says nothing of physical diagnosis; and we have given the above extract as a tolerably accurate description of a morbid condition frequently observed in Mediterranean climates, and which we have long regarded as evincing an effort of nature to throw off morbid matter from the blood through the medium of the mucous membranes. Mr. Spencer Wells, in his reports of cases treated in Malta Hospital, published in the 'Edinburgh Medical and Surgical Journal,' has supported this view, instancing particularly an affection of the kidneys very common in these climates, which he regards as the result of exudation from the renal capillaries of morbid matter existing in the blood, this matter having many points of resemblance to tubercle. Our own investigations appear to be leading to the conclusion that when tubercular matter has been formed in the blood, it is not necessarily deposited in any organ, gland, or tissue, but that it may be thrown off or out of the system through the capillaries of the mucous membranes of the air-passages, stomach, and bowels, or kidneys, exciting at the same time more or less disorder or disease of the parts of the membrane employed for the purpose of excretion. We believe Dr. Lanza's mucous phthisis to be exactly the condition we speak of, although he himself does not allude to any such doctrine.

Dr. Lanza describes a disease which he calls glandular phthisis, which he says is neither a tubercular nor a scrofulous condition of the bronchial glands, but we cannot discern what he does consider it to be, the symptoms he describes being merely those of incipient phthisis. What he terms parenchymatous phthisis is distinguished by extreme dyspnœa, dense, semi-solid, livid, fuliginous or sanguineous, opaque, and more or less fetid expectoration, with a nauseous saline flavour; severe cough, increased by lying on the sound side; more or less fixed pain; hæmoptysis; the emaciation rather that of acute disease than of phthisis, the tissues being flaccid and relaxed rather than dried up (esiccati); œdema; and fever varying with the condition of the lung. We cannot discover upon what state of the lungs all this is supposed to depend, except that there may be "alterations and lesions without organic degenerations," rather an indefinite proposition. The "ulcerous phthisis" is a particular form which from the first depends upon "an ulcer in the lung, so inflamed that it produces habitual fever and consumption," the prevailing symptom of which is said to be extreme fetor of the breath and sputa. In the section on "tubercular phthisis" we find the symptoms and stages described with sufficient accuracy, while the physical signs occupy little more than a couple of pages. With regard to statistics, Dr. Lanza only gives the relative numbers of his five species of phthisis in 100 cases in private practice, and states that he only made 33 autopsies. We need not examine statements made upon such foundations, and therefore pass on to the treatment. This, as prophylactic, consists in promoting general health by proper hygienic measures, and removing known causes of disease. As to eradicatory measures, the author says:

"Now what can be tried against tubercular phthisis not to allow it to irremediably destroy humanity? We have been observers and imitators of all systematic rational methods of practice, from that of Boerhaave to that of Broussais, and have abandoned none of them until it had constantly proved vain or pernicious. We have therefore concluded that the power of known and so-called resolvents is either trivial, or injurious rather than not, by accelerating the softening of the tubercles. Perhaps this accounts for the truth of the maxim of Neapolitan practice, that mercury is exceedingly injurious in tubercular and cancerous diseases.

"I am at present experimenting against the tubercular disease by the daily use of $\frac{1}{100}$ of a grain of arsenic, never increasing this dose. The analogy between cancerous and tubercular disease which led to this practice is so loose that I was not sanguine as to the result, but experience has offered cases of success so satisfactory, that I have not yet determined to abandon the plan.

"I recommend to those who are least capable of being seduced by systems never to cease inquiring in the path of positive induction, if there really be in nature a remedy to destroy such invincible phytozoa." (p. 455.)

The iodide of potassium, alone or combined with hemlock, is said to have proved exceedingly useful in Naples. Dr. Lanza generally prescribes a milk diet, with farinaceous and mucilaginous substances, vegetables, and fruit, not allowing meat, fish, or eggs, unless absolutely necessary. Moist country air, in places where intermittents are endemic, is said to prove very advantageous, so that patients are sent from Naples and the sea-shore to inland villages and gardens near marshes, avoiding all places situated upon the lava of Vesuvius. Leeches to the chest, arms, or groins, irritating ointments, or the seton, are often employed to combat urgent symptoms, but the bath is said to be generally injurious. To calm urgent cough, Dr. Lanza prefers the preparations of lettuce, henbane, aconite, or belladonna, to opium; and believes that small doses of ipecacuanha render the sputa less viscid, and more easily thrown off. The Lichen islandicus is used when there is much irritation of the mucous membranes generally, various other simple demulcents and diluents being given freely, as almond emulsion, barley water, &c. Dyspnœa is relieved by inspiration of the vapour arising from hot decoction of lettuce or mallow, or by smoking stramonium, according to the condition of the mucous membrane, as shown by increased or diminished moisture. In the colliquative diarrhœa of the latter stages, it is often useful to omit all medicines, or to give small doses of ipecacuanha, or the mildest refrigerant astringents.

The remainder of the third volume consists of chapters on other forms of sarconosi, which Dr. Lanza calls sarconosi of the spleen, pancreas, larynx, kidneys, bladder, male and female organs of generation, the bones and joints. Being rather theoretical than practical, we pass over these chapters without further notice.

The fourth volume opens with a section on diseases of the heart and blood-vessels, the author treating first of palpitation, dyspnœa, angina, and syncope, as special symptoms of cardiac disease, and then giving a few observations on physical diagnosis. He believes that the relations between the first symptoms or indications of disease and organic alterations are not yet sufficiently determined to be usefully applied to practice, and that the same morbid alteration may produce very different forms of cardiac disease. His observations on diagnosis and treatment do not afford anything likely to interest those acquainted with the writings of English and French authors on heart disease, and the same remark will apply to the chapters on diseases of the arteries and veins. The second section of the fourth volume is occupied by a treatise on hemorrhage, under the head of primary, secondary, traumatic, and mixed. In the primary form, blood is effused without morbid alteration in the tissues or vessels which are the seat of the hemorrhage. The vessels, however, cannot be in the same state as when they do not permit extravasation; and to determine in what the alteration consists, Dr. Lanza for the last fifteen years has carefully examined the tissues in every case of primary hemorrhage which has fallen under his notice, and has discovered what he considers to be a special morbid alteration, most observable in the mucous membrane. The tissue appears as if painted with a bluish or yellowish-red colour, and spotted with red stellated points or centres, from which short tortuous little vessels go off like rays, and the blood escapes from these points or centres. Dr. Lanza designates this condition as hemorrhagic angionosis, that is, a special and primitive disease of the vessels, of which hemorrhage is the sole result. The titles of secondary, traumatic, and mixed hemorrhage are sufficiently explanatory, and we find nothing in the description or treatment of any of these varieties which differs from that laid down in British works on practical medicine. Epistaxis, stomatorrhagia, or hemorrhage from any part of the mouth and fauces, hæmoptysis, hæmatemesis, enterorrhagia, proctorrhagia or flow of blood from the anus, melæna, hæmaturia, and metrorrhagia, form respectively subjects of separate chapters.

Hæmoptysis as a primary disease, or simple flow of blood from the mucous membrane of the air-passages, is occasionally observed in Naples in plethoric persons, and is apt to recur at stated seasons for several years. This form is recognised by the absence of thoracic or other disease to which the hemorrhage could be attributed; the absence of fever; the usual marks of plethora; tendency to hemorrhage in other parts; absence of cough, except what is necessary to throw off the blood, the blood at first being red, afterwards brownish, and then almost colourless; and the rapid recovery of strength, although much blood may have been lost. The treatment of this form must be left rather to nature than to art, unless the quantity of blood lost is so great that danger is imminent. The greatest care should be used to prevent a return, by altering the habits of life which produced the state of general plethora.

Secondary hæmoptysis is considered,—1. As vicarious of another hemorrhage, as hemorrhoidal or uterine discharges, and is said to be rare at Naples. 2. As symptomatic of acute disease, as thoracic inflammations. 3. As symptomatic of a transitory contagious disease, measles, scarlatina, &c. 4. As a concomitant of endemic periodic fever, it is sometimes, but very rarely, observed in the tertian agues about Naples. 5. As a symptom of some radical or specific disease, as scurvy. Dr. Lanza alludes to a form which he calls *scabbiosa*, and says, that while many persons affected with itch are not subject to hæmoptysis, it is observable that most persons subject to hæmoptysis have at some time been affected with itch—a singular observation, if true. He says that this *scabbiosa* form of hæmoptysis is generally preceded, accompanied, or followed by some very severe disease of the air-passages. 6. As a symptom of *sarconosi*, whether ulcerative or tubercular. 7. As a symptom of disease of the heart and great vessels, or angionosis. 8. As a symptom of nervous diseases, whether apoplexy, paralysis, or spasmodic affections. These are the varieties of the secondary hæmoptysis.

Traumatic hæmoptysis is produced by wounds, foreign bodies in air-passages, inhalation of irritating vapours, &c. On tubercular hæmoptysis the author makes the following remarks:

> "Hæmoptysis reasonably causes alarm, since of a hundred successive cases brought under our notice the following sad statistical report may be given: Of 100 cases, 90 are tubercular, and are followed by death, caused by the tubercular disease; and of the ten other cases not tubercular, but of the common primary or

> secondary forms, five proceed from disease that is also fatal. We would only observe that the suffocative hæmoptysis, that is to say, the form which is so violent as to kill by itself, is not included in these statistics, because it is much less common than one per cent. Indeed, we have only met with it in three cases among several hundred cases of hæmoptysis which we have observed in the course of our practice." (pp. 169-70.)

Of ninety cases of tubercular hæmoptysis, seventy are evidently owing to the existence of tubercle, and generally are observed in the early stage of the disease. In other cases it occurs and recurs at times for years, the tubercles remaining in an incipient stage, without producing the general symptoms of phthisis. Sometimes, also, the existence of some common cause leads the physician to overlook the presence of tubercles.

The other forms of hemorrhage are treated in the same order, but we find nothing particularly worthy of remark. The next classes of disease are the *flussi* and *rattenimenti;* the former consisting in superabundant, the latter in defective discharge from the body of any humour or secondary fluid. Dr. Lanza denies the truth of the hypothesis, that the profluvia depend upon relaxation of the solids or tenuity of the fluids, and the opposite condition upon contraction of the solids or density of the fluids; arguing that the only practically useful distinction of these orders of disease is, like the hemorrhages, into primary, secondary, traumatic, and mixed. In the primary forms, the flux or retention constitutes the sole symptom of disease, and does not depend upon morbid conditions of distant organs, nor upon any known traumatic cause—no other disease remaining when either the flux or retention has passed away. In the secondary forms, organic disease may be detected; in the traumatic, mechanical or chemical causes are evidenced; and in the mixed, the degree of flux or retention is out of proportion with the amount or intensity of visible disease or traumatic agency; astringents or aperients also having a direct action upon the fluids discharged or retained, to the prejudice of the principal disease or cause. After the exposition of these views, constipation, diarrhœa, and dysentery are treated at some length; gastro-enteralgia of various forms being also included in this class of diseases, as invariably connected with some deranged secretion. Lead colic, hepatalgia, splenalgia, and common (as distinguished from the epidemic) cholera are the subjects of an appendix. The only extract we think it necessary to present is the following:

> "When, towards the end of summer, common cholera appears epidemically among us, if we except the smaller mortality, and the comparatively few persons attacked, in no symptom or other character does it differ from the Indian cholera. Neither in the treatment have we found any difference; indeed, experience has taught us the following curative analogies between the two choleras: 1. That in both the use is beneficial, and the abuse injurious, of emetics of ipecacuanha. 2. That polypharmacy is equally injurious to both; and that the specifics hitherto vaunted have proved deceitful. 3. That simple lenitives and laxatives relieve gastro-enteric pains. 4. That opiates are more useful than other sedatives, provided the dose be moderate. 5. That mucilages and gums are the only astringents which are tolerated or useful. 6. That artificial warmth is useless and ill-supported. 7. That fasting is the grand remedy." (p. 282.)

Vomiting, volvulus, and diabetes are the subjects of successive chapters. In the treatment of diabetes Dr. Lanza restricts the diet as much as possible to milk, sends the patient to a dry, warm country place, and ad-ministers the carbonate of iron. Opium is given when sleeplessness occurs, and castor-oil when constipation renders an aperient necessary. Chapters follow on increased and diminished secretion of urine, perspiration, and saliva, nasal and bronchial mucus, tears, milk, and semen.

Another class of disease is denominated *collections*, the term implying abnormal retention of any fluid within the body, as serum, bile, air, fat, blood, &c. The volume is concluded by chapters upon different orders of this class of disease; the various species of dropsy, collections of air, jaundice, and obesity; but we have gone through them without observing anything particularly remarkable.

We here conclude our notice of this work. We have endeavoured to put our readers in possession of all the practical observations of importance which a careful perusal has afforded us, and at the same time to give an abstract of the contents of the work, without entering into any critical examination of the views and doctrines advanced, or of the propriety of the classification of diseases adopted by the author. The general impression upon our own minds on laying down the book is, that it is the production of a man of singular natural ability, and an acute observer, who is not well acquainted with, or has availed himself too little of, the published experience of others.

ART. VII.

1. *The Elements of Materia Medica and Therapeutics.* By JONATHAN PEREIRA, M.D., F.R.S., &c. Third Edition. Vol. I.—*London*, 1849. 8vo, pp. 898.
2. *Traité de Thérapeutique et de Matière Médicale.* Par A. TROUSSEAU, et H. PIDOUX. Troisième Ed. Tome I et II.—*Paris*, 1847. 8vo, pp. 896, 898.
3. *Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine.* Par le Docteur MIALHE.—*Paris*, 1845. 12mo, pp. 220.
4. *Lectures on the Chemistry of Pathology and Therapeutics, showing the application of the Science of Chemistry to the Discovery, Treatment, and Cure of Disease.* By ALFRED B. GARROD, M.D.—(Lancet, Vol. I and II, 1848.)
5. *Lectures on the Influence of Researches in Organic Chemistry on Therapeutics, especially in relation to the Depuration of the Blood, delivered at the Royal College of Physicians.* By Dr. GOLDING BIRD, A.M., F.R.S., &c.—(Medical Gazette, 1848.)

IN the former part of this article we presented our readers with a general view of the present state of our knowledge as regards the chemical composition of the various solids and fluids of the animal body; and we endeavoured to show, first, that this knowledge is not yet so complete as to enable us to construct what may be termed their standard formulæ of chemical composition; and secondly, that notwithstanding great advances have been made in pathological chemistry, yet, from the circumstance of our being in want of those accurate data to which we have referred, we are not yet in a condition to make the fullest application of chemistry to the study and cure of disease. We also took a survey of the facts which have been made out with respect to the alterations which remedies produce in the chemical composition of the animal structures and fluids. And now we come to the consideration of our fourth question, namely, *What is the nature of the chemical changes to which the remedies themselves are subject during their passage through the animal economy?*

A very slight glance at the writings of our leading therapeutists will show, that this extremely important question has not yet received its proper share of attention. Trousseau and Pidoux, for example, have hardly referred to it, and Dr. Pereira has dismissed it most hastily, saying that—

> "The precise nature of the changes which medicines undergo when they first come into contact with the body has been carefully investigated in a very few instances only. For the greater number of medicines accurate observations are still wanting; and we form our opinion of the nature of the changes principally by analogy, aided, in particular cases, by some isolated fact." (p. 88.)

We cannot, however, regard this as a very complete expression of the truth; for, as we shall presently find, by taking a review of the labours of Mialhe, Golding Bird, Wöhler, Bernard and Barreswill, Bouchardat and Sandras, and some others, a great deal has been done towards the elucidation of this most interesting subject. In fact, we may regard the work of Mialhe as a very tolerable attempt to represent the changes to which medicines are subject during their passage through the animal system, and we shall therefore especially direct the attention of our readers to it; premising that although we are far from admitting the truth of every proposition which has been laid down by our author, or from admiring the manner in which he has too often arrived at a conclusion, yet we shall not stop to censure him for so doing, believing that our time might be better occupied, and that, as Dr. Whewell states, false theory often proves more advantageous to science than the absence of theory, or, as Lord Verulam expressed it, "truth more readily emerges out of error than out of confusion."

It is our duty, moreover, to say that, considering the difficulties which Mialhe has had to encounter in the pursuit of his very original inquiries, and the manner in which he has surmounted these difficulties, he is deserving of more respect than has been awarded to him by any of the authors which are now before us. The researches of Mialhe have chiefly been made since the month of July, 1841, at which time he announced in the Paris Academy of Medicine, that he would one day demonstrate by means of experiment—

> "That, for an internal remedy to have a general action on the organism, it must be soluble, or susceptible of becoming so, by reason of chemical changes taking place in the interior of our organs." (p. 17.)

Since then, he has done everything in his power to fulfil his promise; for he has not only been occupied in inquiring into the nature of the chemical changes which take place when medicines are mixed together, but he has also been engaged in studying the nature of the changes to which the various remedies are subject, when they are exposed to the action of acids, alkalies, and salts, under circumstances similar to those which exist in the interior of the living body. And from a careful consideration of the many facts which have thus been brought to light, it may be said that Mialhe has done much towards establishing the truth of the following propositions:—

1. That a remedy must be absorbed, before it can exert any remote action on the animal economy.

2. That the remedy must be soluble, or susceptible of becoming so, in the fluids of the living body, before it can be absorbed.

3. That most remedies are subject to chemical changes during their passage through the animal system.

4. That these changes are regulated by ordinary chemical laws, and that they may therefore be foretold, and even made available in the cure of disease.

Acting on his faith in these propositions, Mialhe has undertaken to give an interpretation to many hitherto unexplained phenomena, and to place the art of prescribing upon a sounder and more scientific basis than it has hitherto occupied. He has, for example, endeavoured to explain how it is that many individuals are remarkably prone to the operation of particular drugs; why it is that certain insoluble remedies, such as calomel, oxide of iron, &c., act more powerfully when they are administered in small and repeated doses, than when they are given in larger doses at longer intervals; why some medicines are apt to remain in the system, and to become localized in their action, while others are rapidly carried off by the secretions; why some compounds act quickly, and others require time for their operation; and lastly, by constantly keeping his first two propositions in view, he has been enabled to construct formulæ, whereby he secures to every medicine its complete absorption, and therefore its full amount of remedial action.

If, says Mialhe, a medicine is soluble, and is not decomposed by the fluids of the alimentary canal, it is absorbed at once; but if it is insoluble, then it requires the intervention of the acids, alkalies, or saline compounds contained in the digestive fluids, in order to obtain an entrance into the circulation. It is in this way that the metals, the greater number of the metallic oxides, and certain salts, are dissolved by the acids of the gastric juice; the metalloids, the insoluble acids, certain insoluble salts, oils, resins, balsams, &c., are rendered soluble by the alkaline matters contained in the intestinal fluids; whilst other insoluble salts, such as calomel, sulphate of lead, chloride of silver, &c., are acted on and dissolved by the alkaline chlorides which are present throughout the whole course of the alimentary canal.

After considering the changes to which these substances are individually subject, Mialhe comes to the following conclusions:

1st. That *the metals* are not endowed with any therapeutical action while they retain their metallic form; but he asserts that, with the exception of the noble metals, they are all readily acted upon by the air, acids, and alkaline chlorides contained in the digestive canal, and they then acquire the faculty of operating on the animal body. In this way metallic arsenic is rapidly converted into arsenious acid; and in the same manner antimony, mercury, lead, tin, bismuth, zinc, iron, &c., are first oxydized by the fluids of the stomach, and then, by being transformed into chlorides or double chlorides, they become soluble, and gain access to the circulation. By viewing the subject in this light, Mialhe is enabled to explain the action of the perpetual pile of the ancients, of metallic mercury when it is admitted into the system, and of iron filings when they are given for the cure of chlorosis.

2d. Mialhe concludes that *the metalloids*, like the metals, are entirely without action while they remain in a free state; but that some of them, as, for example, sulphur and phosphorus, on meeting with the alkaline matters contained in the intestines, are resolved into soluble compounds, such as sulphurets and hyposulphites, phosphurets and hypophosphites, which on getting into the circulation are further oxydized, and are thereby converted into sulphates and phosphates, in which form they appear in the urine. But if, says Mialhe, the sulphurets or hyposulphites are not completely oxydized while they remain in the blood, they will reach the surface of the body, and will be decomposed by the acid cutaneous secretions, and resolved into sulphur, sulphurous acid, and sulphuretted hydrogen. In this way he accounts for the fact that the administration of sulphur always gives rise to the formation of sulphur and sulphuretted hydrogen upon the cutaneous surface. Millon and Laveran, however, assert that sulphur is not soluble in the intestinal fluids, and that it cannot therefore gain access to the circulation; but an appeal to clinical experience will at once exhibit the fallacy of their opinions.

One of the metalloids, viz. carbon, is completely insoluble in the gastro-intestinal secretions, hence it cannot be absorbed, and the action of this substance must therefore be entirely of a local character.

3d. That with regard to *the haloids*, chlorine, bromine, and iodine, they never operate in an uncombined state; but, on being admitted into the interior of the animal body, they immediately seize on the alkalies which are contained in the fluids, and give rise to the formation of chlorides, bromides, iodides, and their complementary oxy-compounds. In these states they act as irritants, they liquefy the tissues, and are rapidly absorbed into the circulating system, being ultimately expelled by the glandular apparatus.

4th. That, with respect to *saline compounds*, Mialhe states that those which are soluble, and are not decomposed by the fluids of the stomach, are generally admitted at once into the blood, there to produce a variety of chemical changes; while the insoluble salts are commonly converted into chlorides or double chlorides by the action of the gastro-intestinal fluids, by which means they are dissolved and carried into the circulation.

In commenting upon these salts individually, Mialhe remarks—

A. That all *the mercurial preparations* used in medicine are reacted on by the solutions of the alkaline chlorides, either with or without the access of atmospheric air, and that they thus give rise to the production of a certain quantity of corrosive sublimate.

B. That the quantity of corrosive sublimate so produced is dependent first, on the quantity of alkaline chloride present, and, secondly, on the nature of the mercurial compound employed; he having found that the binary or rather deuto-mercurial compounds produce at once, by means of double decomposition, their equivalents of corrosive sublimate, while the protoxide and protosalts of this mineral are first converted into proto-chloride of mercury, and then, by a subsequent reaction, into a very small quantity of bichloride.

C. That, as clinical observation has shown, the ingestion of any of the mercurial compounds will give rise to the same series of physiological effects, though these effects may differ in their degrees of intensity; this difference being due, according to Mialhe, to the different proportions of bichloride to which the different salts of mercury give rise. And judging from some experiments which he has performed, he thinks he is justified in saying, that the protosalts of this mineral are much less active than practitioners imagine; for he considers that the protoxide, the protiodide, the proto-tartrate, the proto-sulphate, and nearly all the neutral protosalts which are not decomposed by water, are not endowed with those eminently energetic properties which are possessed by the persalts, such as the bichloride, the biniodide, the bibromide, bicyanide, bisulphate, &c. These observations are, however, in many respects opposed to the opinions which are commonly entertained; most physicians, for example, believe that the protiodide of mercury is a very active preparation; but Mialhe, after pronouncing this to be an error, says that the error has arisen from the circumstance that the protiodide is a very uncertain and impure preparation, for it often contains as much as 9 or 10 per cent. of the active biniodide.

D. That the experiments of Lassaigne and others have perfectly demonstrated, that corrosive sublimate and albumen are susceptible of forming a compound which is insoluble in water, but which in its hydrated state is soluble in the alkaline chlorides; and Mialhe states that this soluble compound, consisting of albumen, corrosive sublimate, and an alkaline chloride, is really formed when bichloride of mercury, or any mercurial, is administered internally, and that it is in this form that mercury gains access to the circulation, the compound being carried over the body without undergoing any molecular change. He remarks, in fact, that the mineral is in such a state of intimate combination with the organic matter of the blood, that neither the strong alkalies nor the alkaline sulphurets have the power of decomposing it.

Such a union of this metal with the fundamental elements of the blood must necessarily affect the most important properties of this nutritious fluid. It must modify its plasticity, and influence all the organic functions to which it is subservient. It is thus, perhaps, that the compounds of mercury operate in checking inflammatory action; an action which, as we have shown, is accompanied by an increase in the proportions of the more plastic elements of the blood. Somewhat in support of these opinions are the following remarks by Dr. Pereira:

> "Mercury has been detected in the blood by Zeller, Buchner, Schubarth, Colson, and Dieterich. It appears to be in such intimate combination with this vital fluid that it cannot be recognised by the ordinary tests. Destructive distillation is, in most cases, necessary for its detection." (p. 819.)
>
> "*After absorption mercury effects changes in the qualities of the blood, and in the action of the whole organization, but especially the apparatus of organic life.* Soon after salivation has been established, the blood exhibits an inflammatory crust. At a later period its colour deepens, and its coagulability is diminished; the portion of clot, and therefore of fibrin, to serum becomes smaller. 'The formation of albumen and mucus,' says Dieterich, 'sinks to that of serum; the whole organic formation of the patient is less consistent and cohesive.'" (p. 820.)

The preparations of silver are, according to Mialhe, instantly converted into the chloride when they reach the gastric cavity; and the chloride of silver thus produced unites with another portion of alkaline chloride to form a soluble compound, which is readily absorbed. This double chloride, circulating with the blood, becomes exposed at the surface of the body to the action of light, whereby a coloration takes place, which is precisely similar to that which occurs when an inert organic substance impregnated with chloride of silver is presented to the solar beams. Believing therefore, that the coloration of the skin is the result of ordinary chemical action, he advises that, during the administration of any silver compound, the exposed parts of the patient should be washed daily with a solution of iodide of potassium, so that a silver compound may be formed which is more soluble than the chloride, and less likely to be darkened by the solar rays.

The preparations of gold and of platinum also owe their curative powers to the formation of soluble double chlorides of these metals and the alkalies.

The antimonials act by becoming converted into a highly irritating compound, namely, the hydrochlorate of the chloride of antimony. Moreover, according to Mialhe, all those preparations of antimony which either contain the hydrated oxide of the metal, or which easily give rise to the formation of this compound, are exceedingly energetic in their action. It is by reason of this circumstance that tartar emetic, metallic antimony, and the oxides of antimony, which have been prepared in the moist way, are readily attacked by the fluids of the alimentary canal, and produce the soluble acrid salt before referred to; while all those preparations of antimony which have been prepared by a dry process, as for instance James's powder, and certain forms of kermes, are not so soluble in the gastric secretion, and hence their effects are milder and more uncertain. In this way our author explains the anomalies which occasionally present themselves in the practice of such physicians as are accustomed to employ these preparations of antimony. Thus, to take an example, Trousseau states that the irritating local action of the antimonials is in a direct ratio with their solubility; but he adds that—

> "We have not been a little astonished in the course of our experiments at finding that metallic antimony, perfectly pure, and in the state of fine powder, had an action nearly as energetic as tartar emetic. It is, we acknowledge, very difficult to explain this anomaly; for by admitting that it becomes quickly oxydized in the digestive cavities, in order to pass into the state of a salt, yet we cannot conceive how it is that the oxides of antimony (meaning the artificial preparations) have an action so different from that of the metal."

Now, according to Mialhe, the explanation of this apparent anomaly is very easy, for he shows by experiment that metallic antimony is quickly acted on by the air and acids of the stomach, while the artificial oxides are not so readily attacked.

Mialhe states that the compounds of antimony produce two classes of effects—first, nausea, vomiting, and diarrhœa; and secondly, a feeling of faintness, a lowering of the pulse, and a general retardation of the circulation. He says, moreover, that both of these classes of effects may be accounted for on chemical principles. For, when a compound of antimony reaches the stomach, it is reacted on by the hydrochloric acid of the gastric juice, and is converted into an acid chloride of the metal; this acid chloride is a very irritating compound, and it thereby occasions the first set of symptoms to which we have referred. But, on gaining access to the circulation, this compound is decomposed, its acid is neutralized by the alkalies contained in the blood, and the antimony is precipitated in the form of an insoluble hydrated oxide, which stops up the first set of capillaries through which it endeavours to pass; and in this way, according to Mialhe, it obstructs the circulation and produces the second set of phenomena to which we have alluded. After a short time the hydrated oxide thus formed is dissolved by the liquor sanguinis, and is carried on into the general circulation; but it is still a matter of doubt as to what change the antimonial preparations produce in the composition of the blood. Dr. Pereira says,—

> "I am not acquainted with any chemical observations as to the state of the blood after the protracted use of antimonials, but from analogy it is inferred to be similar to that caused by mercurials." (p. 182.)

The preparations of iron have been most carefully examined by Mialhe, and he concludes that they are all rendered soluble by the acids of the gastric juice, and that the potent agent in all the martial preparations is the per- or sesquioxide of the metal.

Having gained access to the circulation, the ferruginous salts are, according to Mialhe, acted on by the albuminate of soda contained in the blood, and a double decomposition ensues. On the one hand, albuminate of iron is formed (a compound which he says is the basis of the blood-globule), and on the other, a new soda-salt is produced. Mialhe, however, does not pretend to say that this is the true and only mode in which iron operates in the production of new blood-corpuscles; but he merely refers to the fact that such a decomposition does occur when a persalt of iron is added to the blood, and that it may have something to do with the *modus operandi* of iron in the cure of chlorosis. He believes also that this opinion is strengthened by the results of the following experiment, viz.: that on pouring a neutral persalt of iron into an albuminous solution containing a certain quantity of chloride of sodium, there is a copious deposit of the red albuminate of iron, a compound which has a great resemblance to the matter composing the blood-discs, for while it is insoluble in saline solutions, it is freely soluble in pure water.

The preparations of lead are all acted on by the alkaline chlorides contained in the alimentary canal, and the metal gains access to the circulation in the form of a double chloride of lead and of alkali. According to our author, sulphuric acid does not prevent these changes from being effected, and he therefore condemns the use of acid lemonade and of alkaline sulphates, as prophylactics or antidotes to lead poisoning. He states also that those individuals who make use of a large quantity of common salt are very liable to the toxic influence of saturnine preparations.

When any soluble compound of lead is applied to the surface of the body, it combines with the albuminous elements of the tissues, and forms a coagulum which is subsequently absorbed by means of the solvent action of the alkaline chlorides contained in the animal fluids.

The preparations of tin, bismuth, and zinc are also dissolved by the fluids of the alimentary canal, and on gaining access to the circulation they are, like the compounds of antimony, lead, and iron, decomposed by the alkaline matters contained therein, and are precipitated within the tissues and blood-vessels of the intestinal tube. They thus act in checking secretion, and in arresting the flow of blood through the capillaries of the part.

The preparations of copper may, by reason of their different actions, be divided into two series, viz. into those which are absorbed immediately, and into those which require the intervention of the gastro-intestinal fluids, in order to enable them to gain an entrance into the circulation. To the former series belong all the organic salts of copper, and to the latter all the inorganic.

Mialhe states that all the salts of copper exert a local action, by combining with the protein elements of the tissues, and by forming coagula, which, in the case of the first series to which we have alluded, are readily dissolved by an excess of the cupreous salt; while in the case of the second series, the coagula are not so soluble, for they require a great excess of the inorganic reagent in order to be brought into a liquid state.

The alkalies combine with the acids of the gastric juice, and thus form salts, which are readily admitted into the circulation. But besides being antacids, the alkalies are powerful liquefacients and antiplastics; they soften the tissues, and, as we have before said, they dissolve those products which are but weakly organized. Taking advantage of these properties, the alkalies may be made useful in conveying morbid products out of the system.

The preparations of the earths and alkaline earths are for the most part freely soluble in the gastric juice; but after getting into the blood, or passing beyond the pyloric opening of the stomach, they meet with alkaline fluids, which instantly occasion their decomposition. In this way the insoluble bases, alumina, lime, and magnesia, are precipitated, either in the blood-vessels, or in the tissues of the intestines, or on the surface of the mucous coat. All these preparations act therefore as astringents or absorbents, for they either mechanically arrest the flow of blood through the capillaries of the intestine, or they stop up the pores of the intestinal tissues, and in this way check secretion. Mialhe attaches a great value to those calcareous preparations which contain phosphate of lime; for he states that this salt is readily converted by the fluids of the stomach into a soluble acid phosphate—a compound which, while it does not exert any caustic or irritating action on the albuminous tissues, is readily decomposed by a small quantity of alkali, and converted into the insoluble gelatinous basic phosphate. It is to this property that the therapeutical value of the calcareous phosphate is to be ascribed; for the moment the acid phosphate permeates the intestinal membrane—that is, the moment it becomes absorbed—it is brought into contact with the alkaline fluids of the blood, and is thus deprived of the small quantity of acid by which the phosphate was held in solution. In consequence of this chemical reaction, an abundant gelatinous precipitate of the basic phosphate of lime is formed, which instantly blocks up the intercellular spaces through which the fluid dejections were poured out; and as this basic phosphate is almost completely insoluble in the intestinal fluids, the mucous membrane remains incrusted by it for a considerable time, and the organic structures are thereby enabled to return to their normal condition.

The preparations of magnesia are similarly dissolved by the acid gastric liquor, and passing the pylorus, or gaining access to the blood, they are decomposed by the alkaline matter with which they are brought into contact, and an insoluble compound of magnesia is deposited. Should such a precipitation take place in the intestines, the magnesia may act either as an absorbent, or as a mechanical irritant. Again, some of the soluble forms of magnesia, as calcined magnesia, or the simple hydrate, may, like lime-water, become absorbed at once from the alimentary canal, and, getting into the blood, may react on its salts and set an alkali free. This is probably the *modus operandi* of both lime and magnesia when they act in the cure of gout and stone.

When the salts of baryta enter the circulation, they are instantly decomposed by the alkaline carbonates, sulphates, and phosphates, contained in the blood; and the barytic salts are thus resolved into one or more insoluble compounds, which block up the channels of the capillaries, and thus permanently obstruct the circulation of the blood through them.

The salts of the vegetable alkaloids are in a somewhat similar condition to the preceding; for, in the first place, they require the intervention of the gastric juice in order to be dissolved; and, secondly, they are decomposed by the alkaline matter contained in the blood or intestines, by which means the alkaloid is precipitated in the form of an insoluble hydrate, which, according to Mialhe, arrests the flow of blood through the capillary vessels.

Lastly, *the salts of the vegetable acids* are for the most part consumed in the system, and their bases appear in the urine in the form of carbonates, sulphates, &c.

5th. Mialhe states, that when *the free acids* are administered internally, they combine with the alkaline matters contained in the blood and alimentary canal, and produce salts which are for the most part carried out of the system by means of the kidneys. The salts thus formed, doubtless modify the chemical and physical properties of the blood-discs; and it is, perhaps, in this way that the acids are enabled to check the oxydizing processes which are going on in the organism, to reduce the animal temperature, and to control the formation of the higher protein compounds.

Some of the vegetable acids are partially oxydized in the system, while others reappear in the urine as supersalts. This fact may explain how it is that many beverages, containing free vegetable acids, as, for example, claret, cider, perry, &c., have the power of arresting the formation of insoluble phosphates in the urine, and hence they are frequently used as lithontriptics.

Benzoic acid ($C^{14} H^{5} O^{3}$), however, passes out of the system in combination with the elements of glycocoll ($C^{4} H^{4} N O^{3}$), and it is thus converted into hippuric acid ($C^{18} H^{9} N O^{6}$), in which form it appears in the urine.

Again, when uric acid is administered internally, it is said to disappear from the system, and to become oxydized. In fact, Wöhler and Frerich have found that it is transformed into urea and oxalic acid, results which are quite in accordance with the well-known theories of Liebig.

6th. It appears, also, that *the volatile oils, alcohol, ether, chloroform, &c.*, are in part consumed or burnt during their progress through the animal economy—the excess of them being exhaled by the breath, or carried off by the secretions. The truth of this statement has been amply confirmed by experiment. Bouchardat and Sandras have found that alcohol, like fat, is at once absorbed from the intestines. They state, moreover, that it is taken up by the veins, not the lymphatics, and getting into the circulation, it appropriates the oxygen of the blood and forms carbonic acid, water, and sometimes acetic acid. Dr. Bence Jones has also remarked, that the quantity of phosphates excreted by the kidneys, is diminished during the use of alcoholic liquors; from which it may be inferred, that the alcohol appropriates the oxygen of the blood, and so prevents the full combustion of the effete tissues.

Again, MM. Ville and Blandin have noticed that, during the insensibility which results from the inhalation of ether, there is more carbonic acid given out by the lungs than there is during the natural play of these organs; and they have also remarked, that the amount of carbonic acid exhaled under these circumstances is in a direct ratio with the insensibility produced.

Moreover, from the fact that the volatile oils are in part exhaled by the lungs and excreted by the kidneys, we are enabled to perceive the rationale of their action in the cure of chronic affections of the bronchial membrane and urinary passages.

7th. Mialhe concludes that *the resins* are not acted on by the acid matters of the stomach; but that, on passing the pylorus, they are brought into contact with the alkaline intestinal secretions, by which means their solution and absorption are effected. Resins, therefore, exercise an action on the lower part of the alimentary canal, and chiefly on the large intestines.

A review of these chemical changes brings us to the consideration of another part of our subject, viz. the "*modus operandi*" of medicines; and a very slight glance at this subject will show that it involves two important questions, viz. What is the *form* in which a medicine acts upon the system? and, secondly, what is the *manner* in which it exerts its action? Thus, with respect to the first question it may be asked, to what, in any particular case, are we to attribute the effects of the medicine? Are they due to the operation of the entire and undecompounded remedy—that is, to the whole of the constituents of it acting together; or are they due to the action of one or more of its components acting separately; or are they due to the formation of a new compound? These are intricate questions; and, it must be admitted, that the present condition of our knowledge does not furnish us with the means of answering them satisfactorily. Mialhe, however, has endeavoured to throw some light upon this subject; for he states, that when a medicinal or toxic agent is introduced into the living body, one of two things happens,—the agent is either unaffected by the acids, alkalies, salts, and protein elements of the fluids, and then its therapeutic action is due to the entire substance; or, as is much more frequently the case, the remedial agent is acted on by one or more of the constituents of the animal fluids, whereby a new compound is formed, whose therapeutic action is dependent upon those elements which are derived from the remedy.

Now, in order to determine whether a substance is or is not acted on by the fluids of the body, Mialhe has recourse to several modes of inquiry. Thus, he first examines the question theoretically, being guided by the known laws of chemical decomposition. He then has recourse to experiment, and by this means he learns what change actually takes place when the medicine is mixed with the animal fluids. And, lastly, he appeals to nature, and endeavours to ascertain in what condition the remedy is expelled from the animal system. Judging from the results which are thus furnished, it may be said that certain remedies, such as the alkaline carbonates, sulphates, nitrates, chlorates, chlorides, bromides, iodides, sulphocyanides, ferrocyanides, the whole of the vegetable alkalies, most vegetable colouring matters, urea, mannite, and a portion of the volatile oils, act of themselves; for they are quickly expelled from the system, and appear in the urine in an unaltered condition.

The greater number of remedies, however, undergo some change during their passage through the body; and they then doubtless act in consequence of the formation of new compounds. Thus, to take a few examples, it appears from the investigations of Mialhe, Wöhler, and Stehberger, that sulphur, phosphorus, and the alkaline sulphurets become oxydized in the system, and act as salts; for they present themselves in the urine as sulphates and phosphates. Again, the salts of the vegetable acids appropriate oxygen, and become carbonates; and, according to the recent researches of Wöhler and Frerich, tannic acid combines with oxygen in the system, and becomes gallic and pyrogallic acids; while uric acid is transformed into urea in the blood of the rabbit, and into oxalic acid in that of the dog. Now, as these are the forms in which such bodies present themselves in the urine, it is not improbable that these are also the forms in which they operate on the animal economy. It appears, moreover, that as all the free acids, both vegetable and mineral, combine in the system with alkalies, they must necessarily act in the body as salts. Some authors, however, say that the vegetable acids are excreted by the kidneys in an uncombined state, and they quote Wöhler's experiments in order to verify their opinions; but, as Dr. Pereira has remarked,—

> "Wöhler's statements do not bear the construction which has been put on them. This eminent chemist experimented with oxalic, tartaric, citric, malic, gallic, succinic, and benzoic acids. In one case only (viz. in the experiment with tartaric acid), he observes, that 'the urine appeared unusually acid.' In the case of oxalic acid, he says, it was not more acid than usual; and in that of succinic, he found it to be alkaline. The acidity or alkalinity of the urine is not stated in the cases of citric, malic, and gallic acids; though he observes that the odour, taste, and proportion of phosphate of lime of the urine appeared to him to be natural in the animals to whom citric and malic acids had been given. With respect to the benzoic acid, he observes that it was found in the urine in combination with a base. In a subsequent part of his paper (p. 307), he concludes, that, 'from the foregoing experiments with oxalic, tartaric, and benzoic acids, it is probable that these, and perhaps all acids, are never eliminated in the urine in the free state, but always in combination with a base.'" (p. 173.)

Again, many metallic substances have been recognised in the urine, but chemists are not yet agreed as to the mode in which these bases are combined or made soluble therein. Mialhe, however, states, that those mineral substances which are not rendered insoluble by the fluids of the body, but which form soluble compounds with the alkaline chlorides, are always easily carried off by the kidneys. This is the case with the salts of antimony, arsenic, lead, tin, zinc, iron, gold, platinum, silver, &c.; while those metallic substances which are not capable of being transformed into such soluble double chlorides, but which are precipitated by the alkaline fluids of the body, are rarely or never detected in the renal secretion. As examples of these may be mentioned, the salts of manganese, bismuth, copper, baryta, &c.

From a consideration of such facts, Mialhe has been led to think that the alkalies combine with the albumen and carbonic acid contained in the blood, and that they, therefore, act in the form of carbonates and albuminates,—that the acids, when they are absorbed, combine with the alkaline matters of the blood, &c., and act as alkaline salts,—that the salts of iron operate in the form of albuminate of iron,—that the salts of silver, mercury, gold, platinum, lead, &c., are converted into double chlorides by the animal fluids, in which form they exert their influence upon the body,—and that the salts of zinc and antimony act as zincates and hypoantimonites.

As regards the *manner* in which medicinal and toxic substances operate upon the animal system, Mialhe says, that the caustics act by combining with the tissues, and by thus forming compounds which are unsusceptible of vitality. Some of the caustics act as coagulants, and others, as we have already said, as liquefacients.

It is found, moreover, that the coagula which are formed by many caustic agents are subsequently dissolved and absorbed. Such phenomena take place either by means of the solvent action exerted by an excess of the caustic, or by reason of the liquefying power of the alkaline chlorides contained in the animal fluids. In this way the coagulants may subsequently act as liquefacients, or they may even give rise to more general secondary effects. Indeed, according to Mialhe, creosote is the only caustic substance capable of producing a coagulum which is perfectly insoluble in the animal fluids; and hence this styptic is truly applicable to the cure of hemorrhage. He states, moreover, that the coagulum produced by a solution of the protonitrate of mercury is not so likely to be absorbed, as that produced by any other salt of this metal. Such a solution may, therefore, be employed as a caustic when we wish to excite the local, but not the general effects of mercury. Again, our author says, that one of the best of all the caustic preparations is a mixture of powdered sulphate of copper and white of an egg,—the two substances being mixed in such proportions as to form a soft paste. This compound forms, according to Mialhe, a very superficial eschar, and it acts as a caustic and astringent, without ever becoming a fluidifacient.

The *modus operandi* of purgatives is twofold. Some of them act by reason of a physical power, and others on account of a chemical influence which they exert on the walls of the intestines. Mialhe believes that the insoluble purgatives (that is, purgatives which are insoluble, or nearly so, in the intestinal fluids), such as charcoal, salicina, quinia, magnesia, &c., operate merely as mechanical irritants; while those which are soluble, or capable of becoming so in the fluids of the alimentary canal, act either by reason of their sapidity, or their density as compared with the blood, or their chemical influence on the mucous tissues. As instances of the first of these, our author mentions colocynth, colchicum, aloes, and other bitter purgatives. As examples of the second, he alludes to the strong solutions of the neutral salts, to syrup, manna, &c., all of which determine a flow of blood to the part in which they are located, and occasion an excretion of intestinal mucus, simply in virtue of their endosmic action. Lastly, as examples of purgatives which act on account of their chemical influence, he mentions croton oil, the vegetable acrids, and calomel; the last-mentioned compound being transformed into corrosive sublimate, which acts on the mucous tissues as a coagulant and chemical irritant.

The resinous and oily purgatives operate by combining with the alkaline matters contained in the intestines, and by thus forming acrid soapy salts, which irritate more or less strongly the lower portions of the alimentary canal.

A consideration of the manner in which the several purgatives operate, will, according to Mialhe, enable us to make choice of those aperients which are best suited to any particular case. Thus, to take an example or two, if it be required to relieve the intestinal tube of matters remaining after digestion, such active medicines should be employed as have no need of the chemical intervention of any other substance to produce their purgative action, among these are the neutral salts, croton oil, magnesia, &c. If, on the contrary, a slow continuous action is desired, as in cerebral congestion, meningitis, or other disorders which especially affect the nervous system, then calomel, the resins, and oils will be indicated; for they undergo a slow transformation in the digestive canal, and hence they produce a slow and continuous purgative effect. In particular cases, when the stomach suffers from excess of acid, as in pyrosis, gout, &c., magnesia is the best purgative, for it neutralizes the action of the stomach, and is mild in its action. Again, when the stomach is inflamed, it is proper to avoid all the soluble and irritating cathartics, and to employ those aperients which act solely on the intestines. And, lastly, if the intestines be affected, as in typhus, &c., we should make use of such mild aperients as act by themselves, and are not prone to produce colic, as, for example, manna and the neutral salts of magnesia, the effects of which are manifested throughout the whole course of the intestinal tube.

Many substances produce a constipating effect, and they do so by checking secretion from the intestinal surface. Among such substances, are the vegetable astringents, certain mineral compounds, as alum, the salts of iron, and bismuth, the acid phosphate of lime, and, indeed, most of the calcareous salts. The *modus operandi* of these substances is not in every instance the same; for the vegetable astringents act by coagulating the fluid matters contained in the walls of the intestines; others, as the mineral and calcareous salts to which we have referred, operate by being decomposed by these fluids, and by being precipitated within and upon the tissues of the mucous membrane, by which means the pores of the intestinal membrane are blocked up, and secretion is checked.

With respect to the more general action of remedies, it does not appear that chemistry has, as yet, been enabled to furnish a very satisfactory explanation. It is, however, affirmed by Mialhe, that most substances, on gaining access to the circulation, act upon the serum of the blood. Some, he says, coagulate the albumen of it, while others exert an opposite effect, and render it more fluid. Among the former agents are the so-called tonics, all true astringents or hæmastatics, the mineral acids, a large number of metallic salts, creosote, alcohol, ergot of rye, savine, &c. And among the latter are all the true diuretics, a considerable number of alteratives or general stimulants, the alkalies and their carbonates, iodurets, sulphurets, chlorides, &c.

Again, there are other substances, such as bichloride of mercury, chloride of silver, &c., which effect a peculiar combination with the albuminous elements of the blood, and which thus, perhaps, exert an influence over the nutritive processes.

Lastly, there are many substances, such as the salts of baryta, antimony, lime, and the vegetable alkalies, which undergo decomposition at the moment they enter the circulation, and thus give rise to the formation of precipitates, which mechanically arrest the flow of blood through the capillaries of the parts.

This leads us to consider the views which are entertained by Mialhe respecting *the localization or stagnation of poisons.* The term localization of poisons has been applied by MM. Flandin and Danger to indicate the fact, that toxic agents are frequently found in greater abundance in certain viscera than in others; and they believe that this circumstance is due to a sort of affinity or elective power which those viscera exert over the poison. M. Orfila, however, has objected to their term; and recently, Mialhe has endeavoured to show that the localization of a poison in any organ might, with more propriety, be called the stagnation of it.

"I have established," says Mialhe, "two classes of stagnations: the first is dependent on an arrest of the circulation, and it may be designated by the term *physico-organic stagnation*, or localization.

"The second is due to a chemical decomposition, and it may be named a *chemico-organic stagnation.*" (p. cclxv.)

The first kind of stagnation is dependent on the intricate vascular structure of organs; thus, to take the liver by way of example, Mialhe states that, in consequence of the complex arrangement of the portal circulation, and the small amount of force which is exerted in propelling the blood through this organ, it follows that, when any poisonous agent is absorbed from the alimentary canal and admitted into the portal vessels, a considerable time must elapse before it can traverse their complex system of capillaries; and hence it is that the poison becomes stagnated, or as it were localized, in the liver. This circumstance, says Mialhe, is sufficient to explain how it is that those soluble poisons which are not decomposed by the constituents of the blood, are found in great abundance in the liver, even for a long time after they have been ingested into the alimentary canal.

The phenomena of chemico-organic stagnation are dependent on the chemical reactions which take place between the constituents of the poison and the elements of the blood, in consequence of which an insoluble precipitate is formed, which is arrested by the first set of capillaries through which it endeavours to pass.

In this way Mialhe explains the facts which have been brought to light by the researches of Orfila, Danger, Flandin, and Devergie; namely, that certain poisons, such as antimony and lead, become located in the liver, spleen, and kidneys; while copper and a few other metals are found to exist in greatest abundance in the tissues of the liver, spleen, and intestines.

Tartar emetic, according to Mialhe, on being absorbed and conveyed to the liver and spleen, meets with oxygen and alkaline salts. The former consumes the organic acid of the compound, and so occasions a precipitation of the antimony, in the form of the hydrated oxide. The greater portion of this precipitate is mechanically arrested by the capillaries of the liver and spleen; while another portion of it, by combining with the alkaline matters of the blood, is thereby rendered soluble, and is conveyed by means of the general circulation to the kidneys, where it is again arrested, and finally excreted.

A salt of lead, on gaining access to the circulation, is partly precipitated by means of the alkaline sulphates and phosphates of the blood. This precipitate is arrested by the capillaries of the liver and spleen. The rest of the lead salt is converted into a soluble double chloride, in which state it arrives at the kidneys, where it is excreted.

The salts of copper are also precipitated by the alkaline and protein matters contained in the blood, and hence a portion of this metal is deposited at once within the tissues of the intestines; while the rest of it is carried on to the liver and spleen, where a total stagnation of the metal is effected. Mialhe states, moreover, that the formiates which result from the digestion of saccharine substances, have the power of deoxydizing the salts of copper, and of thus rendering the metal so insoluble that little or none of it can pass into the general circulation.

The peculiar views entertained by Mialhe, with regard to the *modus operandi* of medicines, have enabled him to offer an explanation of those physiological phenomena which pass under the name of *idiosyncrasies*, or of *tolerance and intolerance of remedies.* For, finding that the action of a medicine is often not at all proportionate to the quantity administered, —that calomel and most resins, for example, will frequently operate quite as powerfully when they are given in moderate quantities as when they are exhibited in large and very irrational doses,—he has been led to think, that the peculiar solvents to those remedies exist only in limited quantities in the intestinal canal, and hence that only a limited quantity of the medicine can be brought into an active state.

Mialhe feels very confident of the truth of this opinion, and he puts it forth as a means of explaining the peculiarities in question. Thus, to take an instance, some persons are especially liable to the action of calomel; this is the case with sailors and with the inhabitants of maritime districts; and, says our author, it is because the fluids of their bodies contain large quantities of alkaline chlorides, so that there is a more complete conversion of calomel into corrosive sublimate than under ordinary circumstances. On the other hand, it has been noticed that children, especially those who are fed on milk, easily support very large doses of calomel; this is dependent on the fact that the fluids of their alimentary canals contain only a small quantity of alkaline chlorides. So, again, the sick, who have been living for some time on low diet, such as broths and tea, have the fluids of their bodies so exhausted of chlorides, that they bear large quantities of calomel without inconvenience. Indeed, it has been affirmed by M. Godefroi, of Caen, that patients who are thus dieted, can easily take as much as a drachm and a half of calomel for a dose; while patients who are kept on a salted diet, can hardly bear a dose of six grains, without being violently purged.

Many individuals are remarkably prone to the action of resinous substances. This, according to Mialhe, arises from the circumstance of their intestinal secretions being of a very alkaline character; and hence a large proportion of the resin is brought into a soluble form. This view of the subject is partly confirmed by some observations, made by M. Willemin, who states that the administration of acids with resins will prevent the nausea which is sometimes attendant on the action of these organic compounds; and that, on the contrary, the admixture of a few grains of soap with a dose of scammony will always increase the nauseating influence of it. In fact, says Mialhe, the administration of one grain and a half of carbonate of potass with eight grains of jalap has determined terrible nausea, followed by abundant vomiting and prolonged purging.

Mialhe explains the tolerance of large doses of tartar emetic, by supposing that, when the medicine is given in considerable quantities, there is not enough muriatic acid in the stomach to displace the tartaric acid of the remedy, and to produce that irritating compound, the hydrochlorate of the chloride of antimony; but that there is sufficient to do so, when the medicine is exhibited in small doses. Again, it is known that the stomach is better able to bear the action of tartar emetic when it is empty, than when it is full; and this, says Mialhe, is because there is less of the acid gastric juice present, and so there is less of the remedy converted into the acid chloride. It is found, also, that, during the existence of disease, the powers of remedies are often very considerably modified; and Mialhe believes that this is dependent on the influence which disease exerts over the composition of the various animal fluids. Thus he states, that the gastric secretion is unusually acid and abundant in chronic gastritis, in hypochondriasis, pyrosis, diabetes, and gout. In all such conditions of the body we are sure, therefore, to get the maximum effects of magnesia, chalk, iron, zinc, bismuth, antimony, mercurials, and the vegetable alkaloids; and the minimum effects of oils, resins, and all substances which require the intervention of alkaline matters in order to acquire activity.

It will be evident, likewise, that when, by reason of disease, the intestinal fluids contain less of the alkaline chlorides than usual, or even when, from a greater amount of fluid matters present, the solution becomes more diluted, then calomel would not be freely transformed into corrosive sublimate, and its action would, therefore, be below par. May not the latter circumstance account for the very slight effects which result from the administration of calomel during an attack of Asiatic cholera?

> "I have now before me," says Dr. Pereira, "reports of eighteen cases of spasmodic cholera, admitted, in the year 1832, into the cholera hospital at Bethnal Green, in this metropolis, in which enormous quantities of calomel were employed by the house-surgeon, Mr. Charles Bennett (formerly one of my pupils), with very slight physiological effects. When a patient was brought into the hospital, two drachms of calomel were immediately given, and afterwards one drachm every one or two hours, until some effect was produced. In 17 out of 18 cases in which this plan was tried, the vomiting and purging diminished, and the patient recovered. Several of them took from 20 to 30 drachms without the subsequent ptyalism being at all excessive. In one case (a female, aged 36 years) 30½ drachms were administered within forty-eight hours; moderate ptyalism took place, and recovery. In the unsuccessful cases to which I have alluded, 53 drachms of calomel were administered within forty-two hours, without the least sensible effect.
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> "Dr. Griffin also tells us that, in several cases of cholera, he gave calomel hourly, in 'scruple doses, to the amount of two or three drachms, or upwards, without eventual salivation; and I recollect,' he adds, 'one instance in particular, in which I gave two drachms within an hour and a half, with perfect success, and without affecting the system.'" (p. 853.)

Now, as a commentary upon this practice, which seems absurd enough, it may be mentioned, that the investigations of Chardienko of Russia, Corewinder of Lille, and Dr. Taylor of London, appear to show, that the alvine dejections in cholera contain only a small proportion of the alkaline chlorides; indeed, the last-named author says, that these salts amount to no more than about ·8 per cent. of the entire fluid.

It is found that certain articles of diet will also modify the actions of medicines. We have already remarked, that the use of salt provisions will increase the susceptibility to the action of mercury and of lead. And Mialhe states, that an animal diet has a tendency to increase the amount of acid in the secretions, and that a vegetable diet exerts an opposite influence.

The administration of more or less water will also materially affect the operation of a medicine, especially when its therapeutic action is dependent on the solvent influence of the acids, alkalies, or salts of the alimentary canal. In these cases, according to Mialhe, their state of dilution is of the greatest importance, and it deserves the careful consideration of the practical physician; for he states "that the action of insoluble remedies must be inversely to the quantity of water administered with them." (p. cclxxviii.) In fact, it is easily demonstrated by means of chemical experiment, that the more a solvent is diluted, the less is its action on the substance to be dissolved. If, for example, we take 9 grains of calomel, and the same quantities of common salt and of muriate of ammonia, and digest them with about 70 drops of water for twenty-four hours, they will give rise to the formation of about $\frac{1}{2}$ of a grain of corrosive sublimate; but if the quantity of water be doubled, only $\frac{1}{4}$ of a grain will be formed; and if it be quadrupled, then only $\frac{1}{8}$ of a grain will be produced; and so on. The administration of water, therefore, directly after a dose of calomel, is very likely to diminish its purgative action.

In administering resinous matters, however, as it is our object to make them pass the pylorus as quickly as possible, it is advisable to give a cup of tea or coffee directly after the medicine, and then to direct the patient to abstain from drinks for some hours, during which time the undiluted alkaline secretions from the intestines will exert their solvent action on the remedy.

Again, Millon and Laveran have shown that when any soluble saline purgative is administered with a large quantity of water, the medicine is quickly absorbed, and it rapidly passes off by the kidneys. Administered in this way, therefore, it acts as a diuretic, and not as an aperient. But if the same medicine be given with a smaller quantity of water, so as to form a solution, the density of which shall be greater than that of the blood, then the salt will act as a purgative, and little or none of it will appear in the urine.

Many authors have asserted that the vital force has the power of modifying the effects of remedies, and that it does so by coercing affinity, and by influencing the chemical changes to which remedies are ordinarily subject; thus, to take an example, sulphur and mercury have a strong affinity for each other, and yet when these substances are administered simultaneously, they circulate together in the blood, and seem to exhibit no disposition to unite until they are excreted, that is, until they are removed from the influence of the vital force; for on being thrown off by the skin, they instantly combine, and form the black sulphuret of mercury. It has been noticed, also, that when we exhibit a salt containing an organic acid, the acid is consumed in the system, and the base appears in the urine as a carbonate; but if we administer the acid itself, notwithstanding that it forms an analogous salt directly it enters the circulation, yet under such circumstances the acid is not consumed, but seems to be protected by the influence of the vital force.

Again, Bernard and Barreswill, who are strongly opposed to the ultra-chemical doctrines of Mialhe, state that the gastric juice has a power *sui generis*, and that alimentary substances must be submitted to its action before they can undergo those changes which are necessary to assimilation. They say that when sugar is dissolved in simple water, and injected into the circulation, it invariably appears unchanged in the urine; while if it be mixed with a little gastric juice, and then injected into the blood, it is no longer excreted unchanged, but is digested or rather consumed in the animal system. From such data our authors believe that the reactions which take place in the living organism are often very different from those which occur out of it; indeed M. Bernard, who has been much occupied with this subject, says, that though lactate of iron and prussiate of potash will instantly react upon each other, and form prussian blue, when they are mixed with water, urine, or even gastric juice, yet that they do not effect any such combination when they are injected into the blood of living animals; and from this he concludes that the vital force puts a check to ordinary chemical changes. We think, however, that this hinderance is much more likely to be due to the alkalies which are present in the blood.

Modes of administering medicines. It is pretty well established that before a remedy can exert any general or rather remote action on the animal system, it must be absorbed; and in order to effect this object, physicians have been accustomed to resort to various contrivances. Most commonly the medicine is swallowed, and it is then exposed to the absorbing surface of the gastro-intestinal mucous membrane. Occasionally it is presented to the influence of the lower part of the alimentary canal; as, for example, when we administer enemata or suppositories. Sometimes the remedy is applied to the mucous membrane of the respiratory tract, as when substances are inhaled or used as snuff. At other times the therapeutic agent is rubbed into the skin. More rarely it is placed in contact with wounds or blistered surfaces. And more rarely still it is injected into the blood-vessels.

Now a consideration of the chemical principles which are involved in this part of our inquiry, will show that a medicine can only gain access to the body by being dissolved in the animal fluids with which it is brought into contact. The question, therefore, as to the best mode of administering a medicine resolves itself into a knowledge of the best mode of effecting its solution in the animal fluids; and we can only obtain this knowledge by considering the nature of the reactions which will take place between the remedy to be used and the various humours of the body. But how rarely are these points attended to; and in consequence how empirical, how irrational, and how unsatisfactory are most of our modes of employing remedies. Let us turn to our English authority by way of proving all this:

"The mode of employing medicinal agents according to the iatraleptic method is

> the following: the substance to be applied being reduced to the finest possible state of division, or suspended in some appropriate liquid, is in this state rubbed into the skin. The dose is as much as ten, and in some cases even twenty times the ordinary dose; but no absolute rule on this head can be laid down. The liquid employed to dissolve or suspend the medicine may be water, spirit, or oily or fatty matter. Iatraleptic writers, however, prefer the gastric juice, or saliva, or even bile; but I am not acquainted with any just grounds for this preference. Collard de Martigny concludes from his experiments, that the palms of the hands, soles of the feet, neighbourhood of the joints, the chest, the back, and the inner part of the limbs, are to be preferred for the application of the medicine." (Pereira, p. 127.)

It is clear from this quotation, that our author has not himself bestowed much consideration on any of the chemical points connected with the subject; and he is therefore not in a position to give any specific directions either as to the solvent to be employed, or the dose to be used, or even respecting the place to which the remedy is to be applied. Nor is he enabled to offer any opinion as to the form or manner in which the medicine gains access to the body.

Again, at page 128, our author states that "the advantages of the endermic method are, that the substances are not submitted to the influence of the digestive process, and that the *pure* effects can be better ascertained." And among the disadvantages of this method he mentions that "some substances have no effect when used endermically."

So, here also, it is evident that Dr. Pereira has scarcely considered the nature of the changes which take place between the remedy and the fluids of the body; for inasmuch as the secretion from an abraded surface is alkaline, and is not very different from that which is poured out upon the surface of the intestines, it cannot be that the effects of the remedy are more pure; and as regards the disadvantage to which he refers, a knowledge of the composition and chemical reactions of any medicine will at once indicate whether it be absorbable or not from the abraded skin. But further, in speaking of the disadvantages which are attendant upon the administration of medicines by the mouth, Dr. Pereira states that, "of all parts of the body the gastro-intestinal surface is the most useful for the application of medicine." And wherefore, we may ask, is it so? Assuredly not solely because "of the great susceptibility, the active absorbing power, and the numerous relations which the stomach has with almost every part of the body" (p. 129), but chiefly because the secretions from the alimentary canal are charged with those very substances which are necessary to the solution of every therapeutic agent; for there are acids in the stomach, alkalies in the intestines, and saline matters in every part of the canal.

> "Sometimes, though less frequently than the stomach, the rectum is employed for the application of medicines. It has been asserted that the general susceptibility of the rectum is only one fifth of that of the stomach, and that medicines take five times as long to operate by the former as by the latter; hence it has been said that both the dose, and the interval between the doses, should be five times as great as when applied to the stomach. But this assertion is far from being universally correct, though it may be so occasionally. Orfila asserts that those agents which operate by absorption, as opium and tobacco, are more active by the rectum than by the stomach; and he assigns as a reason, the greater venous absorption of the rectum, and its less digestive power. But this statement is in direct opposition to the experience of almost every practitioner." (p. 130.)

Now, why is it that there is so much uncertainty attendant upon the mode of administering a remedy; and that certain medicines are readily absorbed from the rectum, while others cannot be made to gain access to the circulation through such a channel? Simply because due attention is hardly ever paid to the nature of the chemical reactions which take place between the components of the remedy, and the fluids which are secreted into this part of the alimentary canal.

Is it not clear from all this, that little or no attention has hitherto been directed to the proper principles which should guide the practitioner in making choice of the best mode of administering a remedy? If, indeed, it were necessary to furnish stronger evidence of the truth of this statement, such evidence might easily be obtained by taking a survey of the various suggestions which have been offered for the exhibition of medicines by inhalation; for practitioners have been advised to administer powdered bark, nitrate of silver, lycopodium, sulphuret of mercury, and, indeed, a host of insoluble and irritating substances, in this manner; but, as we need not show, the recommendations are as devoid of principle as they are of therapeutical value. And so also are those suggestions which have been offered for the introduction of medicines by injecting them into the circulation; the advocates of which assert, that such a mode of exhibiting a remedy is valuable, because it enables the practitioner to avoid those chemical reactions to which the medicine is subject during its passage through the alimentary canal. But it is scarcely necessary to remark, that the opinion here expressed is founded upon very erroneous data; for if we except the acid matters of the stomach,—matters which are easily neutralized by means of a little magnesia,—the medicine is sure to be brought into contact with the same reagents as those with which it associates in the blood; and it must, therefore, exhibit the same reactions, and give rise to the formation of the same compounds, as if it had been administered by the mouth. Moreover, as positive objections to such a mode of introducing a remedy, we may advance the danger and difficulty which are attendant on the operation itself, and the chance that the medicine may form insoluble compounds with the blood, and thereby instantly extinguish life by stopping up the capillaries of the lungs. Truly has Dr. Pereira remarked:—

> "That some of the supposed advantages of this operation have no real existence, while several objections to it exist; such as the danger of introducing air into the veins, or of throwing in too large a dose of the remedy (for a slight excess in some cases may prove fatal), or of the occurrence of phlebitis. These, then, are sufficient reasons for not resorting to this practice, except on very urgent occasions; for example, to excite speedy vomiting when the patient is unable to swallow." (p. 132.)

And as a counter-blast to the absurd practice of injecting salt and water into the veins of those who are attacked with cholera, we may say, in addition to the foregoing, that the practice has been very unsuccessful, and "that, however it may be with pigs and herrings, salting a patient is not always the same thing *as curing* him." (Dr. Watson.)

We will now turn our attention to the *form* in which remedies can be most advantageously administered.

Mialhe asserts that all medicines, when they are exhibited internally, must be dissolved in the gastro-intestinal humours before they can exert any general action on the animal economy. This proposition, indeed, constitutes the basis of his treatise on the art of prescribing; and in enunciating it, he says:

> "I am not the first who has thought that the liquid form is more proper for absorption than the solid; nor am I the first physician who has thought that certain insoluble medicinal agents, such as iron and magnesia, do not become active until after they have been submitted to the dissolving action of the gastric juice; but I think that I am the first who has formally proclaimed that no metal, no oxide, no saline compound, &c., can become active until it is rendered soluble, and I think that I am also the first who has traced the laws which preside over this chemical action." (p. xx.)

In all of Mialhe's formulæ, efforts are therefore made to bring the medicine into the condition of a soluble compound, and he frequently associates it with matters which have a tendency to counteract any irritating properties which it may possess: thus, in prescribing mercury, he generally makes choice of the bichloride of this metal, and he associates it with sal-ammoniac, common salt, and certain organic matters which deprive it of its corrosive and irritating action. By adopting a principle of this kind, and by administering medicines in a soluble form, we prevent, says Mialhe, the chance of their accumulating in the alimentary canal, and we can also predict, with the greatest accuracy, the effect which any dose will produce. On considering the importance of these objects, together with the irrational manner in which medicines are commonly prescribed, it is somewhat surprising that our leading writers on pharmacology have not endeavoured to frame a series of rules for the proper association and administration of remedies. It does, in fact, appear to us to be a striking inconsistency, when we find authors labouring to become acquainted with the origin of abstruse therapeutical terms, and striving to coin others which are no less obscure; then busying themselves about the synonymes, history, natural history, commercial value, and all else which is comprehended by that great word *pharmacognosy*, or the commerce of drugs; while they are manifestly ignorant and unconcerned about the best methods of employing their remedies. Let any one take a survey of what Dr. Golding Bird has denominated our therapeutic weapons; let him attempt to enumerate them, and to ascertain the value of their respective claims to the physician's notice: and he will not fail to perceive that their number is too great to be manageable, and that many of our preparations are useless and uncertain; for, to take an instance, where is the necessity for so many preparations of iron, of mercury, of antimony, &c.?

Truly did Sydenham remark, when he was commenting on the first of these compounds, "that busy chemists will make this, as well as other excellent medicines, worse rather than better, by their perverse and over-officious diligence." In fact, a great many of our therapeutic agents might be expunged from the materia medica, without loss to practical medicine; for it must be admitted, that the act of curing disease is not founded upon the knowledge of many drugs, but upon the right use of a few.

We find, on referring to the work of Dr. Pereira, that very little is said with respect to the form in which medicines should be administered; indeed, that author has disposed of the subject in less than one page, and much of what he has said is derived from the writings of Dr. Paris. So, also, with the French writers Trousseau and Pidoux; they do not appear to attach a very great deal of importance to this matter, and we are, therefore, compelled to turn for information to the treatise of Mialhe.

According to the last-named author, metals should never be administered in their uncombined state, for their action is dependent on the influence of a limited and unknown quantity of some special solvent. And for the same reason, all insoluble remedies, as calomel, bismuth, oxide of zinc and of iron, &c., should be given in small and repeated doses, so as to ensure their complete solution and absorption.

In speaking of the mercurials, Mialhe states that, in order to excite the general action of this metal, we should always employ the bichloride of it, for we can thus be quite certain of the amount which is admitted into the circulation. In this opinion he is supported by Dr. Pereira, who says: "The balance of evidence is decidedly favorable to the employment of this medicine as an internal remedy for venereal diseases" (p. 862); but he afterwards remarks, "that few have ventured to employ the perchloride of mercury in acute diseases; however, Schwartz gave it in hepatitis, after the fever and pain had subsided; Sauter employed it in an epidemic scarlet fever; and Berends administered it in asthenic malignant fevers" (p. 862): while "Mr. Lempriere, who practised in Jamaica, finding that calomel was often exhibited in immense quantities, without exciting any apparent action, was induced to employ corrosive sublimate, in doses of the ⅛ part of a grain, with the addition of ten drops of laudanum, and this quantity was repeated every hour until some affection of the mouth was observed, or until the more alarming symptoms had considerably abated" (p. 826).

Mialhe says, that the bichloride should always be administered in very small doses, as from the ⅒ to the ½ of a grain daily; and that it should be given in solution, with a little common salt and muriate of ammonia, as the latter substances keep the bichloride soluble, and prevent it from acting on the tissues of the alimentary canal. He states, moreover, that this action may be still further reduced by mixing the medicine with almond emulsion or white of egg. According to Mialhe, corrosive sublimate should never be given in substance, and we ought to avoid the admixture of it with those saccharine and extractive matters which have the power of decomposing it. The normal mercurial liquor of this author is composed of distilled water, 16 oz.; salt and muriate of ammonia, of each 15½ grains; corrosive sublimate, 5 grains; and the white of one egg. The white of egg and water are first to be mixed and filtered, then the alkaline chlorides added, and lastly, the bichloride. Each fluid ounce of the solution contains rather less than ⅓ of a grain of the bichloride. It is, therefore, a little weaker than our pharmacopœial preparation, which contains about ½ a grain of corrosive sublimate in each ounce.

The bichloride of mercury cannot, according to Mialhe, be substituted for calomel when we wish to produce a purgative action; for, in the first place, it is rapidly absorbed, and will hardly ever reach the intestines; and, secondly, if we were to give it in sufficient quantity to produce purgation, it would become a mortal poison.

The author has introduced an ointment of corrosive sublimate, consisting of bichloride of mercury, 1 part; muriate of ammonia, 2 parts; and lard, 7½ parts. This ointment is to be employed in quantities varying from 15 grains to a drachm. He says that it is a most active preparation, the activity of it being considerably increased by the alkaline chloride; and that it may be employed instead of blue ointment in the abortive treatment of certain local inflammatory affections.

The preparations of iron. Authors still disagree with regard to the comparative value of these preparations. For instance, Trousseau and Pidoux think with Sydenham, that iron is most active when it is given in a metallic form. Desormeaux and Blache state that the black oxide and the proto-carbonate are the best preparations of it. Bouchardat, believing that the protoxide of the metal is the active agent, says that it ought to be given as a proto-carbonate or as a proto-organic salt. Lastly, Merat and Delens, together with the greater number of English physicians, are of opinion that iron may be advantageously employed in any form; but that this is a great error is evidenced by the fact, that many of the preparations of iron are given in very variable doses, without any constancy of result. Thus, iron filings are administered in quantities which vary from 10 to 30 grains; the black oxide in proportions which extend from 5 to 20 grains; and the red oxide in amounts which range from 10 to 240 grains,—the greater part of the mineral being, in each case, carried through the body without exciting any therapeutical action whatever. These facts have induced Mialhe to undertake a new examination of the subject; and, as we have before said, he concludes that the potent agent in all the martial preparations is the peroxide, and that only those compounds of iron can be advantageously employed in medicine, which are soluble in the gastric juice, and which are capable of being decomposed, either directly or indirectly, by the free or carbonated alkalies. To this class belong metallic iron, the oxide, bromide, chloride, iodide, and all the oxy-salts. He states, moreover, that these preparations are not equally valuable, but that the soluble salts of iron are by far the most active, and of these he gives the preference to the potassio-tartrate; for he says that it is a very soluble salt; that it has little or no taste; that it agrees well with the stomach; that it includes the peroxide of the metal; that it is not decomposed either by the acids of the stomach, or by the alkalies of the intestines; that it is capable, therefore, of being absorbed through the whole course of the alimentary canal; and that, on getting into the blood, its organic acid is consumed, and the peroxide is precipitated in a state of combination with albumen, which is best suited for the reproduction of blood-corpuscles.

The preparations of lead, silver, gold, and platinum ought, according to Mialhe, to be given with an alkaline chloride.

Sulphur. This substance being dependent on the alkalinity of the secretions for its solution, it ought to be administered with some alkaline or earthy base. The same remark applies to *the resins*, and to *oleaginous substances.* While, on the other hand, the vegetable alkalies, from their being but slightly soluble in water or the intestinal secretions, ought to be given with acids as super-salts; and this rule, says Mialhe, should be especially attended to when the alkaloids are administered in large doses, for unless they are absorbed from the stomach, they are not likely to gain access to the circulation.

A very slight consideration of the chemical principles which are here set forth, will show that medicines ought not to be mixed together indiscriminately, but that they should be associated in accordance with certain rules. It should be remembered, says Mialhe, that when we administer several remedies at the same time, one of three things will happen; either each of the medicines will act in its own proper manner, as if it had been administered alone, or one of the substances will augment the action of the other, or the associated bodies will diminish or even annihilate each other's action.

As instances of the first, we may mention all those soluble substances which do not interchange elements with each other; as, for example, the alkaline and earthy sulphates; the sulphates of quinia and cinchonia; the various vegetable astringents, &c.

As instances of the second, Mialhe states, that we may increase the purgative action of scammony by combining it with almond oil, and that we may augment the cathartic effects of oils and resins by administering them with a little calcined magnesia. Moreover, the association of such substances as do not require the same solvent will always tend to augment their general effect: thus, to take one example, clinical experience has shown that a mixture of calomel and jalap acts in a much more marked manner than does a considerable dose of either of these bodies given separately. This arises from the fact that the calomel appropriates the soluble chlorides of the alimentary canal, while the jalap effects a union with the alkalies thereof. Again, it has been remarked, that iodide of potassium increases the action of mercurials; and, according to Mialhe, the same thing occurs when prussic acid is administered with a salt of mercury.

Lastly, as instances of one medicine weakening, or even altogether preventing, the effects of another, we may refer to the mixture of those substances which require the same solvent for their action, as chalk and magnesia, magnesia and subnitrate of bismuth, magnesia and quinia, magnesia and iron, oxide of zinc, &c.; all of which substances require the agency of the gastric juice in order to be rendered soluble. As other examples, we may mention the association of medicines which mutually decompose each other, and thereby produce insoluble and inert compounds.

It would here be proper to refer to the rationale of the action of antidotes; but as we have already discussed this matter in a former volume (Vol. II, p. 241), we may be excused from again entering upon it. It may not, however, be superfluous to state, that the criticisms which we there passed on Mr. Taylor's definition of an antidote, are fully supported by the opinions entertained by Dr. Pereira; for, in speaking of the counter-poisons, at p. 159, he uses almost the same words, and adopts the same classification as we did upon the occasion to which we refer. We may likewise mention that Mialhe seems nearly to have reached that "consummation so devoutly to be wished," the discovery of an universal antidote; for he says, that the hydrated protosulphuret of iron is endowed with the power of neutralizing the effects of nearly all the metallic poisons, as the salts of tin, lead, copper, zinc, bismuth, mercury, silver, gold, platinum, antimony, and arsenic: indeed he regards it as the antidote "par excellence" to the last-named mineral; and his conclusions are, in the main, verified by the more recent inquiries of Bouchardat and Sandras.

Another set of counter-poisons to which we might refer, are the prophylactics; but our space will not permit us to discuss them here.

And now, in concluding our review of Mialhe's work, we are bound to state that this investigator has done much to improve our knowledge respecting the action of remedies, and to expose that blind empiricism which has hitherto been mixed up with the science of therapeutics. Thus far, therefore, his efforts deserve our warmest praise; but, in our admiration of these efforts, we must not be blind to the fact, that Mialhe has often gone beyond the boundary of safe speculation, and that he has frequently constructed hypotheses upon assumptions, rather than upon well-established data. We may truly say, therefore, that while a contemplation of the general results of his labours may be made with much profit, it would not be advantageous to scrutinise them individually.

We approach our last question, namely, that which refers to the application of chemistry to the discovery and cure of disease, with much diffidence. For, in the first place, even though our inquiries into the chemical composition of the animal fluids and solids have been limited almost entirely to the blood and urine, two fluids which have received the greatest share of attention from pathological chemists, yet, on passing in review the countless examinations which have been made of these fluids, we find that our knowledge respecting the changes to which they are liable during the existence of diseased action is far from being complete or satisfactory; secondly, we are, comparatively speaking, ignorant of the influence which remedies exert over the composition of the healthy and diseased structures; and lastly, we know but little of the changes to which the remedies themselves are subject during their passage through the animal body. How, therefore, until our information is more complete on these subjects, can we expect to be able to make a successful application of chemistry, either to pathology, or to practical medicine? Nevertheless, we are sanguine enough to believe, that the time is not far distant when the greatest benefit will be derived from such an association of these sciences. In fact, even with our present imperfect knowledge, the chemist has been enabled to give an interpretation of many of the phenomena of disease, and he has been bold enough to construct hypotheses, which, with a little modification, may, perhaps, bear the test of future investigations. Among these hypotheses, there is one to which we have frequently referred in the pages of this journal—an hypothesis which accounts for the phenomena of fever, plague, cholera, smallpox, scarlatina, and, indeed, of the whole of the exanthemata, on the supposition that they are dependent on a species of fermentation, which is due to the introduction of a decomposing organic molecule into the interior of the living body; and with regard to which, it is remarked by Liebig, to whom we owe its revival in its present form, "practical medicine will soon decide whether this view is just, or whether it is to be rejected."

It may, however, be asked, is there, in the first place, any evidence to show that the introduction of putrid matter into the animal system does give rise to effects which are at all comparable with those of fever, &c., or even that a state of transformation or putrefaction is propagated to the constituents of the living body. Now, as Liebig remarks,—

"It is a fact, that subjects in anatomical theatres frequently pass into a state of decomposition, which is communicated to the blood of the living body. The slightest wound with instruments used in dissection excites a state which is often dangerous, or even fatal.

"The fact, observed by Magendie, that putrefying blood, brain, bile, eggs, &c., laid on recent wounds, cause vomiting, lassitude, and death after a longer or shorter interval, has never, as yet, been contradicted.

"It is a fact, that the use of several kinds of food, as flesh, ham, sausages, in certain states of decomposition, is followed in healthy persons by the most dangerous symptoms, and even death." (Animal Chemistry, p. 203.)

So again, the experiments of Magendie, Gaspard, Lauret, Hamont, and many others, show that the injection of putrid matter into the blood of healthy animals will give rise to a set of symptoms which are very analogous to those of typhus.

"If a small portion of putrid matter," says Dr. Armstrong, be "accidentally introduced into the blood during dissection, or if the experiment be made upon the lower animals, it produces a fever, having exactly the characters of typhus, under its continued form. And though, as far as my own observations have gone, malaria or marsh effluvia alone produces typhus fever, under an intermittent, remittent, or continued form, yet I believe that putrid matter introduced into the blood produces an affection so exactly resembling typhus fever, that (putting out the local affection of the wounded part) I believe no individual could confidently pronounce that it differed from typhus fever."

Bernard has also shown that many of the ordinary kinds of fermentation are easily excited in the living blood, and that we can, by injecting yeast and sugar into the circulation, produce a disease which is very analogous to a low typhoid fever; for it is accompanied with great prostration of strength, with bloody fluxes, with ecchymoses on the intestines, and with a black and uncoagulated condition of the blood.

Moreover, Dr. Copland has, in his essay on Dysentery, satisfactorily shown that this disease is often occasioned by the use of waters which contain decomposing organic matters.

"Lastly, it is," says Liebig, quoting from Henle, "a universal observation, that the origin of epidemic diseases is often to be traced to the putrefaction of large quantities of animal and vegetable matters; that miasmatic diseases are endemic in places where the decomposition of organic matter is constantly taking place, as in marshy and moist localities; that they are developed epidemically under the same circumstances after inundations; also in places where a large number of people are crowded together with insufficient ventilation, as in ships, prisons, and besieged places." (p. 205.)

Again, Mr. Walker, in his work on Graveyards, has brought together a body of facts, which prove beyond all doubt that putrid animal exhalations have frequently occasioned diseases which have raged like a pestilence or an epidemic. He cites one instance, which is particularly instructive. In the year 1773, a sexton, while letting down a corpse into the vault of the parish church of St. Saturnine at Saulieu, in Burgundy, accidentally broke a coffin, which contained the body of a fat man that had been buried twenty-three days; a discharge of sanies followed, the odour of which greatly annoyed the assistants; and—

"Of one hundred and twenty young persons of both sexes, who were assembled to receive their first communion, all but six fell dangerously ill, together with the curé, the gravedigger, and sixty other persons. The illness with which they were seized is described as a putrid verminous fever, accompanied with an hemorrhage, eruption, and inflammation." (p. 97.)

Paré also relates that, in the year 1572, a pestilential fever spread nearly ten leagues round Guienne, and that it was occasioned by the putrid exhalations from a pit into which several dead bodies had been thrown two months before.

These facts and observations are, we think, sufficient to prove that the introduction of putrid matters into the animal body will give rise to effects which are very similar to those of fever, &c.

Secondly, it may be asked, what evidence is there that, during an attack of fever, there are any phenomena manifested in the living body, at all analogous to those which result from the fermentation of dead organic matter? To this it may be answered, that the evidence is chiefly derived from a study of the products of the two operations; for, on the one hand, it has been remarked that, during the putrefaction of dead animal substances, ammonia is always evolved, and various secondary compounds, containing phosphorus, sulphur, &c., result; and, on the other, it has been noticed, that analogous compounds are actually thrown out of the body by the kidneys during the crisis of fever. These important facts have not escaped the observation of the illustrious Liebig, who says:

> "If, in a number of contagious diseases, for example, in typhus, an almost never-failing product of putrefaction, namely, ammonia, free or combined, is observed in the air, in the urine, in the fæces (as phosphate of magnesia and ammonia), it appears impossible to entertain any doubt whatever as to the origin and propagation of many contagious diseases." (p. 204.)

Hence, says Liebig, after reviewing the chief points connected with these two questions,—

> "According to all the rules of scientific investigation, the conclusion is fully justified that, in all cases where a process of putrefaction precedes the occurrence of a disease, or where the disease can be propagated by solid, liquid, or aeriform products of disease, and where no nearer cause of the disease can be discovered, the substances in a state of decomposition or transformation must be regarded as being, in consequence of that state, the proximate causes of the disease." (p. 205.)

This hypothesis, now propagated with so much zeal by the cultivators of organic chemistry, is identical with that which was put forth by Hippocrates, and which has also been maintained by some of the more modern fathers of physic. Witness the following definition of the poison of fever:

> " '*Fomitem febriferum asseramus esse. Deleterium quid in spirituum systemate deletescens, quod fermenti ad instar eos adviens atque æstro primùm exagitans, deinde humoribus secundo quasi momento, varias mutationes atque qualitates morbosas nobis sensibiles imperiti.*' If it be inquired, who is this apparent supporter of the views now so popular, and so generally admitted—who is this who talks of a morbid poison communicating its action like a ferment to the constituents of the system, setting up, by a secondary impulse, perceptible metamorphic changes in the blood, tissues, and secretions?—I answer, that the author who thus announces, and in a laborious manner through a large work developes the now popular theory, with the single difference of using the Latin word *fermentum* for the Greek ζύμη, is at least free from plagiarism, seeing that he was consigned to the tomb in this modern Babylon some 150 years before the oldest present in this theatre saw the light." (Dr. Golding Bird, Lecture V, p. 142.)

So popular have these opinions become, that they are now freely admitted and confidently discussed in most of our modern works on practical medicine. Nay, more, they have been made the basis for the support of a great group of maladies which go by the name of zymotic diseases; and in the statistical nosology which was drawn up by Mr. Farr for the guidance of those who have to make returns as to the causes of death, the following maladies are included under the head of zymotic diseases: smallpox, chicken-pox, miliaria, measles, scarlatina, diphtheria, mumps, hooping-cough, croup, thrush, diarrhœa, dysentery, cholera, influenza, scurvy, purpura, ague, remittent fever, yellow fever, typhus, puerperal fever, plague, rheumatic fever, syphilis, gonorrhœa, erysipelas, necrosis, malignant pustule, hospital gangrene, glanders, hydrophobia, porrigo, and scabies. Such a mode of arranging these diseases is perhaps a little hasty, but it is worthy of remark, that many of them exhibit considerable resemblance both in their origin and progress. This is especially the case with the fevers, the exanthemata, &c., and it is said to be so with cholera and intermittents, both of which appear to result from a miasm, and to present an algide and a reactive hot stage.

It must be admitted, however, that chemists have not yet been able to obtain that tangible evidence in support of the doctrine in question, which would be afforded by the isolation of the malarious or miasmatic ferment; and hence many have looked for the cause of epidemics in a disturbed condition of the atmospheric and terrestrial electricities, rather than in the presence of an actual poison. At present, however, this is a pure hypothesis, not having by any means the same amount of evidence in its favour, as that on which the zymotic theory is erected. We cannot at present enter further into the discussion of this question; but must leave it with the expression of the conviction, that future investigations will prove that the miasm of fever and the poison of epidemics are not delusions; and that these investigations will probably point out the *origin* of such morbific agents, and will show that, whether they be organic molecules in a state of change or decomposition, or the germs of simple plants or animalcules, they tend to produce, when admitted into the circulation, a series of abnormal changes among the constituents of the animal fluids.

The admittance of such a doctrine into the field of pathology will enable us to obtain better and more definite ideas concerning the phenomena of disease; it will enable us to take a more comprehensive view of those hitherto dimly discerned subjects,—contagion and infection; it will probably also give us the power of striking at the root of disease, and of protecting the community from the scourge of epidemics: and above all it will, we think, banish from the practice of medicine much of that empiricism with which it has at all times been infested. On this subject, however, we have dwelt too recently to render it necessary for us again to treat upon it at any length. We shall, however, adduce one or two examples that will serve to indicate the character of the inquiries at present in progress, from which good results may be expected.

Diabetes is a disease upon which the labours of the chemist have thrown some little light. They have shown, for example, that it can no longer be regarded as a disease of the urinary organs, but that it is one of malassimilation; for the researches of M'Gregor, Blondlot, Magendie, Bouchardat and Sandras, Bernard and Barreswill, Liebig, Prout, Simon, and many others, have proved that the production of sugar in the alimentary canal is a natural consequence of the digestion of feculent matters; and that the sugar thus formed, on gaining access to the circulation, is, under ordinary circumstances, converted into fat, or else consumed and made subservient to the production of animal heat. Under certain unknown conditions of the human body, however, this sugar is not so appropriated, but circulating with the blood it acts as a foreign substance, produces disease, and is finally excreted by the kidneys. The great problem, therefore, which the chemist has now to solve, is that which relates to the condition of the body whereby the metamorphosis of the sugar is prevented. Many speculations are afloat with regard to this morbid condition. Some chemists, as Liebig and Bence Jones, believe the liver to be at fault, and they base their opinions upon the ground that the liver is concerned in the production of fatty compounds. It would appear, moreover, from the researches of Bernard and Barreswill, Martin-Solon, &c., that this organ is also concerned in the formation of sugar; for they state that the latter substance is always present therein, even when a strictly animal diet has been used, or even while the animal exists in utero. Again, Willis states that the bile is always acid in diabetes, in place of being alkaline; and from the investigation of Mialhe it appears, that alkaline matters are deficient in the blood.

Some authors refer this disease to the stomach and to the duodenum; it has even been said that, under ordinary circumstances, the digestion of saccharine and feculoid matters is effected in the duodenum, and that normally the product of this digestion is absorbed, not into the veins, but into the lacteals, where it undergoes a conversion into some other substance before it passes into the general circulation; in certain diseased states of the body, however, probably in those where the gastric juice is deficient, this digestion is supposed to take place in the stomach, and the saccharine product to be conveyed at once into the veins and so into general circulation, without having been submitted to this transforming influence of the lacteals.

Dr. Aldridge states that in diabetes the sugar is not entirely produced from the food, but that much of it results from the decomposition of the albumen of the blood; for, says this author, if the vegetable is enabled to form albumen from sugar and ammonia, there is no reason why the animal should not possess a counter-power, namely, that of transforming albumen into sugar and ammonia.

Guided by such opinions, many have concluded that diabetes is a disease of the whole animal fabric, and that the protein-elements of the body are resolved into sugar, extractive, and ammonia, and not into their ordinary products, uric acid, urea, &c. By way of supporting this opinion, they say that in diabetes the urea and sugar excreted are always in an inverse ratio to each other; but M'Gregor has shown that this statement is erroneous, and that the urea is often in excess in the renal secretion, even when the sugar is above its mean proportion. The truth is, that we are unacquainted with the true pathology of this disease, and that we are still ignorant of that condition of the body in which the ordinary appropriation of sugar is prevented.

As regards the treatment of diabetes, chemistry has only been enabled to point out the palliative course. It has merely shown that the patient should abstain from those vegetables which give rise to the formation of sugar; and to Bouchardat are we indebted for experimentally proving that a total abstinence from vegetables is not necessary during the treatment of this disease, but that we should merely prohibit the use of those substances which contain much of either starch or sugar.

Gout and Rheumatism are two diseases which have from the earliest time claimed the attention of the pathological chemist; and though his inquiries into the nature of these diseases have been both numerous and long continued, still he has only discovered that they are attended with the circulation of morbid matters, which, in the one case, may be uric acid, and in the other, lactic or acetic acid. He has yet to unfold the source of these morbid principles, and to discover the condition of the system whereby they are produced in such abnormal quantities. The experiments of Bouchardat on ducks, and those of Lehmann and Percy on the human subject, show that uric acid is derived from the protein-elements of our foods and tissues. In fact, Dr. Prout is of opinion that this acid results from the secondary mal-assimilation of the albuminous tissues, while others assert that it may also be derived from the gelatinous elements of the body. Liebig and his followers affirm, that the gouty diathesis results either from a deficiency of oxygen in the system, or from a mal-appropriation of it; and they account for the influence which luxurious, slothful, and intemperate habits have in exciting this disease, by supposing that an indulgence in such habits occasions the formation of effete matters, which, entering the circulation, appropriate the oxygen of the blood, and thereby prevent the normal oxydation of the tissues and their conversion into urea.

As to the origin of the lactic and acetic acids, which are excreted so abundantly in rheumatism, and which appear to be the peccant elements of this disease, Prout thinks that they result from the secondary mal-assimilation of the gelatinous tissues; and it is probable that they are produced under very nearly the same circumstances as the preceding.

The rational modes of treatment, therefore, for these diseases are—

1st. To promote the oxydation of effete matters, by taking exercise, by increasing the number of the respirations, and by exciting the free action of the skin.

2dly. To abstain from all those exciting and oxygen-seeking compounds which are so abundant in the diets of the luxurious.

3dly. To free the alimentary canal from all peccant matters, and to promote the free action of the liver.

4thly. To make use of alkaline beverages; for they are found to have the treble effect of increasing oxydation, of dissolving the acid matters of the economy, and of exciting free diuresis.

> "In connexion with this subject, I would especially draw attention to the undoubted benefit resulting from the treatment even of acute rheumatism by large doses of one of our most certain diuretics,—nitrate of potash, in doses of ʒss or ℥j, dissolved in two or three pints of any diluent, in the twenty-four hours. An enormous amount of urine replaces the scanty excretion generally noticed, and the cure of the patient is considerably expedited. This practice, which has been popular in the Parisian hospitals for the last two or three years, has already attracted notice here." (Dr. Golding Bird, Lecture vi, p. 232.)

In fact, while we are speaking on this subject it may be mentioned that of all the means which are open to us for the elimination of effete matters from the system, that presented by the kidneys is certainly the most efficient. And a consideration of the influence which different diuretics exert over the composition and quantity of the renal secretion has enabled us not only to make some important and novel therapeutic applications, but also to explain the curative action of many of our most popular remedies. Thus, to take an example:

> "A man," says Dr. Golding Bird, "labouring under some chronic ailment, which, perhaps, like old rheumatism, is the direct result of unhealthy constituents of the blood, starts for one of the Brunnens or spas, and with fearful devotion swallows the enormous quantity of ten or fourteen beakers of the warm bubbling water. In a few minutes he begins to secrete abundance of urine; and he is engaged alternately drinking and micturating for the greater part of the morning, active exercise, when possible, being enjoined the whole time. By this exercise, the wear of the tissues is increased, and the copious water-bibbing positively aids the metamorphosis of the tissue, and washes its results from the body.
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> "The same explanation may be given of the success which has attended some cases when submitted to the hydropathic quackery; the patient being actually cleaned out—the old and diseased tissues being literally washed away to make room for new structures, deposited under the hygienic influence of exercise, good air, &c." (Lecture vi, p. 228.)

Finding, moreover, that the kidneys can depurate the blood, not only of matters which are generally regarded as the normal constituents of urine, but also of compounds which are generally excreted by other glands, we are enabled to perceive the advantages which result from the use of diuretics in diseases of other secreting organs. We have already alluded to this fact, and it may be mentioned that Dr. Golding Bird is especially anxious to point out the benefit which results from the employment of renal alteratives in the cure of hepatic diseases.

Again, as the researches of Krahmer and of Golding Bird show that many of the so-called diuretics act merely by increasing the amount of water excreted by the kidneys, it is evident that such substances can only be of use when we wish to expel any excess of this element from the system. On the other hand, as potash and some of its salts have the power of dissolving the animal structures, and thus of causing the elimination of organic matters, it is clear that the employment of this alkali is indicated whenever we wish to rid the system of the products of a low vitality, &c.

> "On theoretical grounds," says Dr. Pereira, "the alkalies have been supposed useful, because their continued use leads to a diminution of the fibrine of the blood, which, in acute inflammations, is augmented in quantity; and also because coagulable lymph, a product of inflammatory action, is soluble in alkaline solutions. The latter observations led Mascagni to propose the use of alkalies in acute inflammatory disease; and in an epidemic pulmonary affection which in 1800 made great ravages in the district of Chiudino, in Tuscany, the practice proved highly successful; as also in dropsy induced by obstruction of the lymphatic vessels and glands, from depositions of coagulable lymph, in consequence of an inflammatory diathesis." (p. 178.)

Dr. Golding Bird, however, speaks with much confidence concerning the use of alkaline remedies in such diseases; and his opinions are not advanced merely upon theoretical grounds, but as we have shown (at p. 316 of our former Article), they are supported upon the basis of actual experiment, and we may also say of actual practice, for—

> "Before the introduction of iodine into medicine such remedies were more frequently given, and we must either admit their value, or declare the recorded

experience on the subject as a tissue of falsehood or error. As one among many illustrations, I would refer to the results of Mr. Brandish's experience with his solution of potash. In chronic visceral ailments, in cases where albuminous deposits have occurred in glands, as in some forms of struma, and particularly in old rheumatic cases (carefully distinguishing them from mere neuralgic actions), where much of the suffering is kept up by the formation of an undue proportion of acid urates in the system, much good promises to be effected by the remedies in question. The acetate of potash at one time enjoyed a high reputation as a remedy in the treatment of strumous glandular deposit." (Lecture vi, p. 232.)

It having been observed, moreover, that the kidney possesses the power of removing many poisonous matters from the system, it has been proposed by Orfila and by Letheby that diuretics should be given when a poisonous substance has gained access to the body. This proposition has also been sanctioned by Drs. Pereira and Golding Bird. And lastly, concerning the more general treatment of blood-diseases, it has been stated by Dr. Copland that,—

"Whenever this fluid becomes more or less changed, it will be requisite to have strict reference to the causes from which the change has arisen. Unwholesome food, vegeto-animal miasms, imperfect secretions and depurations, and deficient nervous and vital power, have been shown to be the chief of these. That the first and second of these should be avoided, need not be stated; and that the secreting and eliminating functions ought to be promoted in order to purify the blood, is equally manifest. The nervous and vital energies must be not only supported, but also promoted and excited, in order that the power of secretion may be afforded to the torpid and weakened viscera, and that the crasis and vital condition of the blood may be thereby restored, and the tonicity of the capillaries and of the tissues generally increased. In addition to these, also, morbid secretions should be frequently evacuated, in order that vital power may not be further reduced by these morbid impressions on the nerves and mucous digestive surfaces, and that the possibility of the absorption of any part of them into the circulation may be thereby avoided." (Dict., vol. i, p. 198.)

In concluding our remarks upon the benefits which have been derived from the application of chemistry to practical medicine, we are compelled to admit that these benefits are not as yet very considerable. In fact, the progress of the chemist into the fields of pathology and therapeutics has been opposed by so many difficulties, that his advances have been slow and erratic; but he has gone far enough to perceive that there is a rich prospect before him, and to know that incalculable good will result from the continuance of his efforts. This may not, perhaps, be evident to the multitude, whose prejudices so often blind them to the light of truth, but—

"The rational physician, who ventures to cast aside the trammels of dogmatism and empiricism, cannot, for an instant, doubt that pathology, therapeutics, and diagnosis are only safely based on chemistry, physiology, and morbid anatomy; he cannot entertain a doubt that the same chemistry with which he scans the changes in crude inorganic matter, will likewise enable him, if not at present, yet surely at some future period, to detect the variations in the composition of the animal fluids and solids, some of which are dependent on physiological, others on pathological causes, and will throw a new light on the normal functions of the organism, as well as on the various processes of disease." (Simon, vol. i, p. xv.)

This was the language of one of the zealous votaries of chemical science, and we are tempted to confirm it with the prophetic words of another:

"The time will come, although the present generation will hardly live to see it, when we shall have obtained a numerical expression, in the shape of chemical formulæ, for all the normal processes or powers of the organism; when we shall measure the variations in the functions of its individual parts by corresponding variations in the composition of the matter of which these parts consist, or of the products which that matter yields; when the effects produced by morbific causes, or by remedies, shall be quantitatively determined; when a better method shall bring us a knowledge of all the conditions of the vital phenomena, and shall introduce clearness and certainty into our explanations. Men will then look upon it as incomprehensible, that there ever was a time when the share which chemistry is destined to take in these conquests of science was contested, or when doubts could be entertained concerning the mode in which her aid was to be given to physiology." (Liebig, Animal Chemistry, p. 258.)

And let us ask, what is the mode by which we may hope to benefit a science of so much importance? It is, says Liebig, by extending our knowledge beyond the *apparatus* which serves for the production of animal motion, by seeking into the composition of the organs themselves, by learning the changes to which the food is subject in the living body, as its transformation into portions of organs, and its reconversion into lifeless compounds, and by inquiring into the share which the atmosphere takes in the processes of vitality. A knowledge of all these is required as a foundation for our future conclusions.

So with reference to our mode of improving the science of pathology and therapeutics, we must extend our present knowledge far beyond its present boundaries; for at this time we have only gained an imperfect acquaintance with the grosser changes which occur in the position and general appearance of diseased organs:—we have hardly attempted to make ourselves acquainted with the alterations which take place in their molecular structure and chemical composition, and still less have we sought to be informed concerning the influence which therapeutical agents exert over the structure and composition of these organs, or even of the changes to which the remedies themselves are subject during their passage through the living animal body; all of which must be determined, before we can hope to place the science of Therapeutics upon a sure and well-defined basis.

ART. VIII.

1. *On Infantile Laryngismus; with Observations on Artificial Feeding, as a frequent Cause of this Complaint, and of other Convulsive Diseases of Infants.* By JAMES REID, M.D., &c.—*London,* 1849. 8vo, pp. 204.
2. *Observations on the Pathology of Croup; with Remarks on its Treatment by Topical Medications.* By HORACE GREEN, M.D., M.A., &c.—*New York,* 1849. 12mo, pp. 116.

AFTER a careful perusal of Dr. Reid's volume, we have failed to discover the author's object in publishing it. A medical writer, who appeals to the profession, should, we apprehend, possess one of two qualities to justify his claims to attention. He should either have it in his power to impart new information, or be gifted with the happy faculty of placing facts and observations that are not new in a novel and striking light before his readers, so as to assist either directly or indirectly in elucidating the subject of which he treats. A mere reprint of facts already pretty generally known is out of place, especially in this age of progress; and, so far as the author is concerned, it is a fruitless expenditure of time and labour.

The gist of Dr. Reid's 204 pages might be contained in a nutshell. It is as follows: Laryngismus stridulus is a *nervous* disease, occurring, for the most part, in excitable or irritable children between the ages of ten and fourteen months, and generally caused by spoon feeding, impure air, dentition, and irritation of the stomach and bowels. The etiology of the complaint indicates the treatment. Our readers will perceive that there is nothing new in this epitome, and that precisely the same information may be obtained in Dr. Joy's excellent article on this subject in the 'Cyclopædia of Practical Medicine,' in Dr. Williams's article on Laryngismus in the 'Library of Medicine,' in Dr. Marshall Hall's 'Lectures and Essays,' and even in the pages of the different medical periodicals. Hence the question *cui bono?* was the first that presented itself for our consideration on laying down this volume, after an attentive examination of its contents. With these remarks we should have dismissed the treatise on Laryngismus; but as the subject is one of great interest, we propose to offer a few remarks upon it, and, at the same time, to place before our readers a summary of Dr. Reid's monograph, that they may judge for themselves of its merits.

The author describes four different forms of the disease; which appear to us to be phases and not forms; for there are in reality but two varieties of laryngismus, the mild and the severe, depending on the amount of constriction of the glottis, or, in other words, on whether that constriction be partial or complete during the paroxysm. Dr. Reid takes exception to the name "crowing" being applied to this complaint, because, in his opinion, the sound "more closely resembles the concluding rising note produced by the clucking of a hen," and consequently, as hens never crow, the term is a misnomer. Now it appears to us, although we admit we have not had much experience in the sounds of the henroost, that the noise made during the paroxysm is uncommonly like that of the crowing of a cock, and that the name "crowing disease" is very happily chosen, for it prevents all risk of mistake as to the nature of the complaint, which would not be the case if such vague appellations as Millar's asthma, thymic asthma, spasmodic croup, cerebral croup, all of which have been used, had been still generally adopted. Although the name does not sound very learned, it is as legitimate and correct as that of hooping-cough applied to pertussis, and moreover has this great advantage,—it prevents the possibility of mistaking a nervous for an inflammatory affection.*

The author having described the premonitory symptoms of the disease, proceeds to the "second form" of his division; and as this embraces the most striking and prominent features of laryngismus, we shall give it in his own words:

> "In this variety the area of the glottis is still more diminished by the nearer approximation of its margins, the closure, however, not being perfect, except for a

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* Dr. Marshall Hall adopts the name "laryngeal asthma" for this complaint, in his work on Diseases of the Nervous System. Dr. Hall observes: "The crowing inspiration in children has been aptly designated by Dr. Burgess, *laryngeal asthma.* There is a pathological truth of much importance involved in this expression. It denotes that the crowing inspiration of infants, taken as a special disease, which it is, is, like ordinary or *bronchial asthma*, a disease of reflex action." For our own part, and for the reasons above stated, we prefer the simple name, crowing disease, or crowing inspiration.

moment. There is now a much longer interruption to the respiratory function, and the symptoms of suffocation appear more imminent; there is a violent struggle to inspire through the contracted apperture of the windpipe in order to fill the lungs, the countenance presents the characteristics of great anxiety, with indications of distress, becoming at first red, but soon changing into the purple hue of strangulation; the face becomes turgid and swollen, and the tongue also in some cases. In other instances an ashy paleness of the face is observed. The arms are thrown out, the eyes appear wide open and staring, or, more rarely, seemed turned up in the sockets, the nostrils dilated, the head thrown back, the limbs become rigid, and the abdominal muscles contracted. There is frequently, also, a convulsive and violent action of other muscles, especially of the flexors, causing a peculiar contraction of the fingers and toes, as well as the wrists and ankles, a state which has been called the carpo-pedal spasm. In such cases the thumb is bent in upon the palm, the fingers closing over it, whilst the toes and instep are bent downwards. Occasionally the body itself is bent suddenly backwards, as if by a violent effort, and it appears to the bystanders as if nature could hold out no longer; when at length the attempt at inspiration is partially successful, being attended with a shrill whooping noise termed 'crowing.'

"This is often an indication that the paroxysm has terminated; but in some cases a succession of incomplete shrill sibilant catches supervene before a sufficient inhalation takes place. When, however, respiration is again properly re-established, the infant expresses its fright and suffering by a fit of crying, and falls asleep . . . . These paroxysms vary much in their frequency, duration, and severity; there is no regularity in the intervals between the attacks, but as the complaint advances they generally become shorter, and in some instances do not continue beyond an hour at a time, or even half that period, the slightest noise or emotion being sufficient to produce a paroxysm . . . . The duration of the paroxysm varies from a few seconds to three or four minutes; but in the latter description of cases it is more a succession of paroxysms, air occasionally gaining admittance into the lungs, during the slight and almost imperceptible intervals between them." (p. 10.)

The "third form" is an aggravation of the preceding symptoms, with the addition of general or cerebral convulsions; and the "fourth" and last division is, "complete infantile spasmodic asphyxia, produced by a sudden and convulsive closure of the glottis, which proves as speedily fatal as if the infant were plunged under water, or strangled by a cord round its neck."

Although spasm of the glottis may occur at any period of life, from accidental or mechanical causes, the crowing disease of children, which is a constitutional lesion, is in the great majority of cases confined to the period of dentition. The age at which the complaint is most frequently met with has been variously represented at different periods between three weeks and three or four years; but our own experience coincides with that of the German writers, who state that it most commonly occurs between the fourth and tenth month. Children of a nervous or excitable temperament, or born of parents similarly constituted, being more prone to the disease than others, and those of medical or literary men particularly so.

Dr. Reid devotes a considerable part of his volume to an inquiry into the various *causes* that have been assigned to this complaint, a great portion of which might have been omitted, without prejudice to the work. For example, it was altogether unnecessary to discuss the merits of the exploded theory of Dr. Hugh Ley, which attributes the disease to pressure from enlarged cervical and bronchial glands on the par vagum, or to the equally erroneous views of those who assert that hydrocephalus and enlarged thymus gland are the true causes. Those opinions have been long since thrown aside as untenable and as indicating false treatment. Indeed, the author himself seems only to have disinterred them for the purpose of knocking them down again.

The following are his own conclusions as to the etiology of laryngismus:

"1. That for the occurrence of this complaint, the cerebro-spinal system is required to be in a peculiarly excitable state, which then acts as a predisposing cause. The period of teething is the most likely one to produce this condition. 2. That during this irritable state of the nervous centres, the two most frequent (and in the majority of instances the combined) causes are the improper description of food which is administered to the infant, and the impure and irritating atmosphere which it breathes."

By the term "improper food" is meant human milk in an unhealthy condition, as well as the usual substitutes for the natural means of nourishment in infancy.

We are, however, inclined to look upon Dr. Reid's view of the causation of the disease as too circumscribed; moreover, he undoubtedly dwells upon *one* point—impure air—with undue force. It by no means follows, as the author seems to think, that because the spasmodic tendency is often greatly diminished, and in some instances entirely subdued, by removal to a purer atmosphere, the exciting cause was the foul air the patient breathed; for we cannot overlook similar good effects produced by "change of air" not only on affections of the respiratory organs, but on disorders of the *stomach*—including all the protean forms of dyspepsia—and particularly on those of the *nervous system*. In short, Dr. Reid's argument upon this point is clearly a *non sequitur*.

No doubt, in many cases, the mere act of changing the patient from one locality to another is attended with immediate and marked benefit. A severe case of laryngismus came under our observation in the vicinity of Bloomsbury square, which for several successive nights momentarily threatened suffocation. Purgatives were administered freely, and as soon as the intensity of the paroxysms had in some degree abated, the patient was removed to Bayswater, and in a day or two afterwards the spasmodic tendency ceased. Although the child returned to town in the course of three or four weeks, no symptoms of the disease appeared again. But, on the other hand, we have met with several cases in which change of locality was not followed by the desired result. Doubtless, pure air is preferable to an atmosphere charged with carbonaceous and other deleterious ingredients, in every condition of the body—during disease as aiding the cure, and in health as a prophylaxis against disease;—but that impure air is a special cause of laryngismus stridulus we altogether deny. Dr. Marshall Hall has well and truly observed, "that the disposition to spasm of the glottis consists in a peculiar susceptibility of the excito-motor property of the nervous system; the immediate cause of the attacks being the action of sources of irritation, or excitement of this property; the most obvious of which are dentition, indigestible food, morbid alvine evacuations, external agents, and mental emotions."

Dr. Condie* adopts the views of Dr. Burgess as published in the

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* A Practical Treatise on the Diseases of Children. By D. Condie, M.D. New York, 1844.

'Lancet' for 1838, and the 'Provincial Medical Journal' for 1842, as to the nature, etiology, and treatment of laryngismus. They are as follows:

1st. That it generally attacks children under the age of twelve months.

2d. That excitable spoon-fed children are more subject to it than others.

3d. That intestinal irritation, cold, and dentition are its chief exciting causes.

4th. That the opinion of Dr. Hugh Ley and others, as to the cause of laryngismus, is not borne out by facts; for the disease occurs in numerous instances where there is no pressure on the nerves; and the nerves are compressed in many instances where the disease never supervenes.

5th. That when it occurs in a child over two years of age, it is almost invariably symptomatic of cerebral or cerebro-spinal affections, and is not a special disease.

6th. That the treatment consists in lancing the gums, active purging, guarding against exposure to cold, supplying a young nurse if the child is weaned, change of air, attention to the general health and regimen.

Dr. Condie adds, the cases combined with cerebral disease are seldom met with until dentition has commenced; if the patient survives the appearance of the first molar teeth, the case generally terminates favorably. He also points out the importance of attending to the invariable forerunner of the disease—the clay-coloured or putty-like alvine evacuations.

Dr. Reid very properly indicates the evil results likely to arise from "bringing up children by hand," as the phrase is; but this aphorism is so trite, that it might with all safety have been discussed in as many lines as the author has devoted pages to it; and we certainly did not require the language of Crabbe or of Chaucer to tell us that about a century ago there was a great mortality amongst the children in the parish of St. Giles, "there being no *wett-nurses* provided," and for the same reason, "at the Temple and at Chelsea, there dyes abov one half of the foundling children." Every member of the profession, from the merest tyro, knows that it is a matter of almost vital importance to be able to feed infants on their mother's milk.

Dr. Reid remarks with practical force upon the folly of parents in indulging the capricious taste of their offspring. He observes:

> "It is astonishing how exceedingly injudicious parents are in relation to the dietary of young infants; for example, a child of a few months old I found, although still at the breast, was allowed frequently to eat meat, potatoes, cabbage, and, above all, was very fond of cheese! In a case related by Dr. Montgomery, the mother was in the habit of giving her infant *wine, punch, and bottled stout, because it cried for them!*"

Dr. Reid is indulgent in his censure of this practice, when he simply calls it "injudicious." If a woman gives her infant the food of ploughmen, we have little doubt our readers will agree with us in characterising it by a somewhat stronger term than "injudicious."

We are in perfect accord with Dr. Reid with regard to the post-mortem appearances of laryngismus, for our united experience is that they prove nothing. We have ourselves examined several cases post mortem, and have failed to find any appreciable lesion in the immediate locality of the fatal spasm; and several practitioners who have directed their attention to this disease, have furnished us with their experience, which fully supports this conclusion. The fact is, laryngismus, like angina pectoris in adults, is one of those spasmodic diseases in which examination after death reveals little or nothing as to the pathology of the complaint.

Treatment. As the author considers "improper food" and "impure air" to be the principal causes of laryngismus, he strongly inculcates the necessity of immediately changing both, as the basis of sound treatment.

During the paroxysm, the author recommends the application of hot water to the lower parts of the body, cold affusion to the head, and sprinkling cold water forcibly in the face, as highly advantageous in reproducing the respiratory function. "Slapping the chest and nates," he adds, "is also a most valuable aid in the physiological action upon the respiratory nerves." The operation of tracheotomy he considers of doubtful value. Lancing the gums when they are full and swollen, but not otherwise, is strongly recommended; but to be effectual the incisions must be free, so as to relieve the tension of the gum, and if possible that of the investing membrane of the tooth also. The indiscriminate use of the scarificator is condemned as injurious. Dr. Reid feels somewhat doubtful as to the utility of abstracting blood. He need have no hesitation on the subject. The abstraction of blood is positively injurious in all cases of spasm of the glottis, even when general convulsions are threatened. We have seen more than one fatal result follow this practice.

The author corroborates the generally received opinion as to the value of purgations in this disease, and prefers those of a mercurial character to others. Our own experience would lead us to give the preference to calomel and scammony combined, in full doses, according to the age of the patient. We have seen the happiest results follow the administration of this remedy; but the just remark of Dr. John Clark should always be borne in mind, that "children require much larger doses of purgative medicines, in proportion to their age, than adults."

We do not agree with the author, when he states that our only aim in administering purgatives is to unload the bowels. The doctrine of Rasori should not be lost sight of; for we feel convinced that the derivative or counter-stimulant effect produced by the drastic purgatives on the mucous membrane of the bowels, has quite as much (if not more) to do with the beneficial result, as the mercurial preparation has by promoting the flow of bile. We have always thought that the value of antispasmodics was greatly overrated in the treatment of spasm of the glottis. The author, however, has strong faith in their efficacy. Our own experience certainly tends to the conclusion that little reliance can be placed on them.

We can, however, recommend a remedy to the notice of our readers which is deserving of their attention. It consists in the application of a solution of the nitrate of silver, by means of a sponge attached to whalebone, to the glottis, epiglottis, and even to the sacculus laryngis. We shall presently point out the method of applying this remedy, when discussing the merits of Dr. Green's book; suffice it to say, just now, that although laryngismus is a spasmodic *constitutional* disease, still the tendency to spasm once established, frequently becomes a habit which the slightest irritation will bring on; and to correct this abnormal irritability, and familiarize the parts to the effects of stimulants, the application of the solution of the nitrate of silver is recommended. In hooping-cough, the employment of this agent is attended by the most marked, we might almost say, specific benefit.

We shall now proceed to give a brief outline of Dr. Horace Green's interesting *brochure*. The author's object is to recommend to the notice of the profession a remedy, which he has found attended by the happiest results in one of the most dangerous and fatal diseases in children, namely, croup. In the pages of one of our predecessors,* we had an opportunity of bringing this remedy before our readers' notice, when reviewing Dr. Green's work on 'Diseases of the Air Passages,' in which he recommended it for bronchitis. We can now repeat most conscientiously, for experience warrants us in so doing, "that the author has not only made a most valuable addition to practical or empirical medicine, but that the results of his method of treatment will lead to important changes in the prophylaxis and cure of pulmonary phthisis." The previous essays of Vance, Trousseau, and Belloc do not at all detract from the merit due to Dr. Green for his persevering and successful attempts to render the treatment of some forms of pulmonary disease more effectual and certain.

Dr. Green gives the following explanation, in his preface, of the grounds on which he was first induced to apply this remedy in croup:

"The first successful attempt to introduce a solution of the nitrate of silver into the larynx of a child affected with croup, was made by the author in November 1842. During a period of nearly four years, previous to this time, he had been constantly employing this remedy, in chronic laryngeal and bronchial diseases of adults. But up to the above period, such were the prejudices against its employment, and such the scepticism of a large proportion of the profession on the subject of topical applications to the larynx, in cases of adults even, that hitherto he had not ventured on its use in the treatment of diseases in young children; although the happy effects which in so many instances had followed the application of a strong solution of the nitrate to the diseased pharyngo-tracheal membrane and its follicles, had long before convinced him that the same remedy must prove highly efficacious in a disease so strictly local, and of a nature so peculiar, as that of croup."

After giving a brief, but essentially practical, *résumé* of all that is known relative to the pathology of croup, the author submits the following, as the results of upwards of twenty years' experience in the treatment of this disease:

"1st. That true croup, pathologically considered, is a special or simple disease; being dependent for its existence, like tubercular phthisis, on a peculiar or specific cause.

"2d. That its distinctive and essential characteristics consist in an inflammation of the secreting surfaces of the fauces, larynx, and trachea, which is always productive of a membranaceous or an albuminous exudation.

"3d. That the membranaceous concretion, which is found coating the inflamed mucous surface of the parts in croup, is an exudation,—not from the membrane itself, but is secreted by the muciparous glands, which so abundantly stud the larynx and the trachea.

"4th. That the exudative inflammation commences invariably in the superior portion of the respiratory passages, and extends from above downwards—never in the opposite direction." (p. 4.)

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* British and Foreign Medical Review, vol. xxiv, p. 482.

Dr. Green maintains that there is but one true croup; or, in other words, that croup is essentially a simple disease, being only modified by difference in constitution, epidemic and other influences; and although he does not support the views of Guersent and Bretonneau, that the perfect formation of a *false membrane* is necessary to constitute the distinctive character of croup, and that the cases where this is not formed are, as they declare them to be, instances merely of *false* croup, he asserts, that inflammation of the respiratory mucous membrane,—whatever may be its grade,—if unattended with an albuminous exudation, can no more constitute croup, than any of the various diseases of the lungs, or of their tissues, which are unaccompanied by a tuberculous deposition, can constitute true pulmonary phthisis. According to the part in which the inflammation is most intense, he admits of a division into laryngeal, tracheal, and bronchial croup, as the treatment and the prognosis may be in some measure influenced by the locality of the disease.

The theory of the treatment proposed by the author for croup is founded upon the views of Rokitansky, who says that the exudative process advances from the epiglottis downwards, extending in some instances to the very minutest branches of the bronchi; and that bronchial croup is a disease of youth and early manhood, which, when occurring in the terminal branches of the bronchi, is always simultaneous with pneumonia, and consequently in such cases cannot be *true* croup. Professor Hasse also states that the exudatory inflammation of croup invariably extends from above downwards, and never in the opposite direction. If this proposition be received as fully established, it will be readily seen what important results may follow the topical employment of appropriate remedial agents, in the early treatment of true inflammatory croup.

> "I have, on a former occasion," says the author, "expressed the opinion that the physiological and pathological relations which exist between the throat and the respiratory tubes are not justly regarded by medical writers, in describing the nature of those diseases which affect these different parts. Affections of the throat are ordinarily arranged by nosologists in connexion with those of the œsophagus; or are treated independently of those diseases which occur in the larynx, trachea, and bronchi. Pathologically considered, the relation which exists between the fauces, tonsils, and pharynx, on the one hand, and the respiratory tubes, on the other, is much more intimate and important than the connexion which exists between the throat and the œsophagus. In almost all the inflammatory affections of the air-passages, whether primary or consecutive, the diseased action has its origin in the fauces and pharynx, and extends by continuity thence to the respiratory tubes. This is especially true with regard to the origin and progress of the exudatory inflammation of croup. Here, the morbid action commences primarily about the fauces and the upper portion of the respiratory passage, and extends, universally, from above downwards." (pp. 16, 17.)

Dr. Green is not satisfied as to the correctness of the view which attributes the peculiarity of the product in the inflammation of croup to an excess of albumen in the blood of young children; nor the supposition that it arises from the inflammation extending to the cellular tissue that enters into the composition of the mucous membrane, from the secernent arteries of which it is supposed the albuminous exudation is secreted. In fact, it is still an unsettled question in the pathology of croup, whether the albuminous product is owing to some peculiarity in the nature of the inflammation, or in the structure in which it is developed.

Treatment. In discussing the treatment of croup, we propose to confine our observations to the particular remedy which is the leading feature of the author's monograph. The other remedies indicated are those usually adopted in similar cases. Dr. Green introduces his method of local treatment in the early stages of croup in the following terms:

> "In the treatment of croup with topical remedial measures, I have always employed a solution of the nitrate of silver, as, in my opinion, there is no known therapeutic agent which, for safety, efficiency, and certainty of action, can be compared with the crystals of the nitrate of silver, in the local treatment of laryngeal, tracheal, and bronchial affections. In preparing the solution, the pure crystals should be employed, and not the fused or solid nitrate, as the latter is much more likely than are the crystals to contain the nitrate of potash, or copper or lead in combination. A solution of the crystals, of the strength of from two to four scruples of the salt to an ounce of distilled water, when applied freely to the mucous membrane, does not act, as has been supposed, by burning, or by a destruction of the textural matter. It forms immediately a union with the albumen and other secretions of the mucous lining; whilst it operates at the same time to produce a most favorable change in the vital action of the parts." (pp. 28-9.)

In the first two cases in which the author applied this remedy, the disease was so far advanced, and the symptoms so intense, that it was only as a forlorn hope that he applied the solution of the nitrate of silver to the region of the glottis. Although both cases ultimately terminated fatally, still the beneficial effects of the application, however temporary, were so evident, that Dr. Green resolved to embrace the first opportunity of employing this remedy in the early stage of the disease; and the result of that *coup d'essai* is fully detailed in the two following cases, which we give in the author's own words:

> "1. Several years ago, a son of Mr. and Mrs. W., of Franklin street, a fine healthful child of four years old, was attacked with membranous croup, and, notwithstanding the most energetic measures were adopted, died of the disease after a few days' illness. Some time after this, another son, only a little younger than the first, was at the time of his death similarly attacked, and died of the disease in about the same period of time. In November 1847, the only remaining child of these parents, an interesting daughter of eighteen months old, was violently seized with the same disease of which her brothers had previously died. Several hours after the occurrence of the croupal symptoms in this case, very late in the evening of Nov. 20th, 1847, I was called to see the child, and found her exhibiting all the symptoms of a severe attack of membranous croup. The heated skin, the frequent, oppressed, and stridulous respiration, the dry and ringing cough, 'tussis clangosa,' and the appearance of the tonsils and pharynx, which were highly inflamed, and coated with an albuminous exudation, left no room to doubt the nature and the stage of the disorder.
>
> "For two or three days before the disease had merged into true croup, the child had been labouring under catarrhal symptoms, which were attended with hoarseness, and a dry, hard cough.
>
> "Immediately on my arriving at the house, and finding the patient in the above condition, I determined to try the effect, in this early stage of the disease, of remedies applied directly to the inflamed mucous surfaces. But in order, at the same

time, to secure the aid of general remedies, I ordered the administration of a few grains of ipecacuanha, and, after waiting several minutes, followed the medicine with the application of the sponge dipped in a solution of nitrate of silver, of the strength of forty grains to the ounce of water. With this the fauces, pharynx, and the laryngeal face of the epiglottis were well cauterized, and at the same time the sponge, wet with the solution, was carried into the larynx.

"As in the preceding cases, this operation was followed by a cough, and an effort at vomiting; by which a large amount of glairy, adhesive mucus was discharged, commingled with which shreds of the false membrane were distinctly visible. Within fifteen minutes the child breathed with more freedom, and there was an equally favorable change in the dry and ringing cough. Expecting to repeat the application, I remained a short time with the patient; but there was such an improvement in all the croupal symptoms, that no further application of the caustic was made that night. Five grains of calomel were directed to be given to the child; an emetic of ipecacuanha and tartarized antimony was ordered to be administered in the course of the night, if at any time there should be an increase of the peculiar symptoms of the disease.

"At an early hour the next morning I visited my patient, and found she had passed a night of comparative comfort. The respiration was not so laborious as when I left on the previous evening, the cough was less croupal, and the heat of the surface was greatly diminished. The calomel had moved the bowels, and as the patient continued to improve during the night, the emetic was not given.

"The same favorable indications continued during the day; but about nine o'clock that evening a message was received from the alarmed parents, requesting my immediate attendance, for their child, as the messenger reported, was suffering from a return of the croup. I found a recurrence of the same symptoms that had been present the preceding evening; but they were much less in degree, and a single application of the nitrate of silver to the larynx, which I made at once, relieved in a short time the embarrassed respiration, and lessened the croupal cough.

"The patient again passed a comfortable night, and the next day, when I called, she appeared quite cheerful, and apparently almost free from disease. From this time no further medication was needed; the child has since passed through a severe winter without the recurrence of any symptoms of laryngeal disease.

"The above case occurred in a family whose children were predisposed to the disease; and in its access and development, the affection presented all the symptoms of true, or membranous croup. With the exception of the small doses of ipecacuanha and calomel, which were given on the evening of the attack, no other means were used but the local employment of a strong solution of the crystals of nitrate of silver to the seat of the disease.

"During the months of February and March of the present year (1848), the occurrence of croup among children, in this city, has been more than ordinarily frequent. This is to be attributed, undoubtedly, to the epidemic influence of the atmosphere, which, for some cause, has been in a condition to favour the development of influenza, and other diseases of the respiratory organs, in adults, and of croup in children.

"The following cases appeared during this period, and also took place in a family where croup had before occurred, and in one or two instances had proved fatal.

"2. At a late hour in the evening of the 23d of February, the Rev. Dr. B——, of this city, called at my office and desired me to accompany him, to see his little daughter, a child three years of age, who that evening had been violently seized with an attack of croup.

"She had been hoarse, and had had a rough, dry cough for several days previous to the full development of the affection. When I saw her a few hours after the occurrence of the disease, the symptoms of croup, which were present, were marked and severe,—indeed, as I entered the hall of the house, the ringing cough and stridulous respiration of the child—sounds which no physician ever desires to hear a second time, were distinctly audible through the closed doors of the chamber above.

"With the aid of Dr. J. Hancock Douglas, who was in my office when the father of the sick child called, and who accompanied me to his house, I succeeded in obtaining a good view of the throat of the little patient. The parts were highly inflamed, and the tonsils were covered with an albuminous exudation. The barking cough, and the embarrassed and tracheal respiration, plainly indicated the stage of the disease, and that the inflammation had extended to the larynx, and about the vocal chords.

"As no time was to be lost, I immediately administered ten grains of ipecacuanha, and after waiting fifteen minutes, prepared to cauterize the diseased parts.

"Assisted by Dr. D———, I applied a solution of the nitrate of silver (forty grains to the ounce) to the fauces and pharynx, and also introduced the sponge saturated with the solution, into the cavity of the larynx. The introduction of the instrument was followed by a free discharge of muco-fibrinous matter, in which, and also on the sponge, were shreds of the membranous deposit.

"The little patient, very soon after the operation, appeared greatly relieved.

"We remained nearly an hour after the application, in order to repeat it if the symptoms should indicate its necessity; but the child continuing to improve, we left for the night, after giving directions to have an emetic of ipecacuanha administered should there be any increase of the embarrassed respiration. Soon after we left the child fell asleep, and although the breathing was laboured, and the cough, which occurred often during the night, was croupal, yet she slept for several hours, and when I called the next morning, I found a great improvement in all the above symptoms. The emetic had not been given. As there still appeared to be some inflammation about the throat, and the cough retained the peculiar sound of the disease, I had fears that there might be a return of all the unfavorable symptoms before night, and therefore concluded not to wait, but to repeat the application of the nitrate of silver to the diseased organ. This was done, and I feel confident the operation was attended with much advantage, for instead of having a recurrence of the croupal symptoms on the second night, as had occurred in a former instance (Case iii), and which is so likely to follow a remission of the disease in most cases, there was a constant improvement of all the croupal symptoms during that day, and the following night was passed with equally favorable indications.

"In short, after this date no further medication was needed, for the child rapidly recovered." (pp. 36-43.)

We wish that our available space admitted of giving the ninth case—a case of membranous croup complicated with spasmodic and bronchial symptoms, which we can recommend to the attention of our readers as containing several points of great interest and practical utility.

The instrument generally used by the author for making direct medicinal applications to the fauces, and into the cavity of the larynx, in the topical treatment of croup, is one composed of whalebone, about ten inches in length, slightly curved at one end, to which curved extremity is securely attached a small round piece of fine sponge. Care should be taken that the sponge be not only fixed to the rod of whalebone, but that it be not of a size too large to pass the aperture of the glottis. Anatomists are aware that there is but a very slight difference in size beween the larynx of a child of two years and of twelve years of age; and that at this period of life, the calibre of the tube is from three eighths to half an inch diameter; consequently, if the sponge be formed so as not to exceed one third or half an inch in diameter, it can be made with slight pressure to pass the aperture of the glottis, and to enter the laryngeal cavity. The instrument being prepared by suitably saturating the sponge with the solution to be applied, the head of the child being firmly held by an assistant, and the base of the tongue depressed with a spoon, the operator carries the wet sponge quickly over the top of the epiglottis, and on to the laryngeal fall of this cartilage; then, pressing it suddenly downwards and forwards, passes it through the opening of the glottis into the laryngeal cavity. If any patches of false membrane are to be observed upon the pillars or tonsils, the sponge should be passed freely over those parts, and also upon the posterior wall of the pharynx.

M. Bretonneau, who was one of the first to recommend and employ the nitrate of silver as a topical remedy in the treatment of membranous croup, made no attempt to pass the instrument *below* the epiglottis. M. Guersent applied the solution in such a manner as to cause some drops of it to penetrate into the larynx. We have ourselves very recently heard some excellent clinical observations upon this subject at the Hôpital Neckar by M. Trousseau, recommending this remedy in croup. The French practitioners, however, employ a much stronger solution than that recommended by Dr. Green. For example, M. Bouchut, in a recent work on 'Diseases of Children,'* advises the application of a solution in the proportion of one of the salt to three of water; and M. Guiet, in his thesis entitled 'Considérations Pratiques sur le Traitement du Croup,' proposes a still more concentrated solution; namely, equal parts of the nitrate of silver and distilled water. The author has found a solution composed of from two scruples to a drachm of the salt, dissolved in one ounce of distilled water, to answer all practical purposes. Dr. Green has also found beneficial results attend the application of the nitrate of silver in cases of diphthérite, or the croup of adults. M. Louis, who gives a graphic description of this disease, under the name of "Croup chez l'adulte," records only one case in which a cure was obtained by medical treatment.

Having now concluded our analyses of the contributions to the pathology of children by Drs. Reid and Green, it only remains to remark upon their respective merits. The diseases which both writers have chosen for their theme are perhaps two of the most formidable, because the most likely to terminate *suddenly* in death, within the whole range of children's pathology; but how differently are they treated? Dr. Green, seeing the inutility of the ordinary or routine system of treating croup, boldly enters into a new field of inquiry, and with success. Dr. Reid is content to recapitulate the practice of others, so far as it is supported by his own experience. We have an invincible repugnance to a *rechauffé* of any kind unredeemed even with an air of novelty, but above all, to one of medical truisms; and it is our deliberate opinion that if an author has nothing new to offer before he rushes into print, he will do well to bear in mind that wholesome maxim of the Koran, which ordains, "If speech be silver, silence is gold!"

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* Manuel Pratique des Maladies des Nouveaux-Nés, et des Enfans à la Mamelle.

ART. IX.

Pathological and Practical Observations on Strictures, and some other Diseases of the Urinary Organs. By FRANCIS RYND, M.A., M.R.I.A., Surgeon to the Meath Hospital, and County of Dublin Infirmary, &c. &c. &c.—*London,* 1849. pp. 196.

THERE is a large class of works in medicine and the collateral sciences, which contain but little of absolutely *new* matter, and which are still, notwithstanding the deficiency, acceptable contributions to the literature of our profession. Some are meritorious, on account of their laying unwonted stress upon facts that have long been known, but insufficiently attended to in practice. Others, again, are valuable because of the style in which they are written, the fidelity with which they are compiled, the felicity with which they are illustrated; and besides these, there yet remain a large and valuable section which in some measure combine a portion of all these excellencies. We allude to works which present a clear *résumé* of the pathology and treatment of certain classes of diseases, that have engaged the particular attention of their authors, and in whose composition acquaintance with the experience and labour of others has been associated with personal observations and a thoughtful habit of mind; of such a character is the work before us.

Mr. Rynd presents us in the present volume with a very clear and intelligent treatise, in which thought is blended with experience, and research with practice; and from the perusal of which, although we do not agree in all the opinions that are advanced, we have derived both profit and gratification. As a specimen of views from which we dissent, it may be stated that Mr. Rynd denies the influence of gonorrhœa in producing stricture, and sneers at what he calls a "post hoc propter hoc fallacy." In this we think he is mistaken. It is not a violent gonorrhœa, which runs an acute course in a reasonable period, or stimulating injections employed in its cure, that we believe predispose to stricture; but it is the residue as it were of the disease, the long-continued and scarcely-noticed gleet, which lays the foundation of the complaint. So long as the mucous membrane of the urethra secretes an unnatural amount of fluid, it is in a state of excitement; it is redder and more vascular than ordinary; it exceeds by so much its healthy function; and as this continues sometimes for months, it is manifest that changes must take place in it. Accordingly it becomes thickened, loses its vital elasticity, and in parts begins to contract. It is no argument to say, as Mr. Rynd does, that the majority of persons affected with gonorrhœa escape stricture; the question is, does stricture follow gonorrhœa sufficiently often to bear out the relation of cause and effect?

Mr. Rynd considers stricture to be occasionally hereditary, and cites the case mentioned by Hunter of a boy, four years old, with stricture and fistula in perineo, as corroborative of this view; but to us the proof is incomplete.

In applying anatomical and physiological considerations to the inquiry into the causes of stricture, our author makes the following observations, which we transcribe, as not only very plausible in themselves, but as offering a good example of his style.

"Let it be recollected that the membranous portion of the urethra forms a curve under the firm and resisting ligament of the pubis, which curve will have a tendency to become an angle, according as the bladder becomes distended and rises upwards in front of the abdomen; and the angle so formed will, by flattening and compressing the canal in its transverse direction, render the passage of the urine proportionately difficult through it. Any one who has in his own person experienced the delay and trouble of emptying an over-distended bladder, that has seen the water first come by drops, then in a small thread-like stream, and not assume its wonted freedom and size until the discharge is nearly completed, will readily comprehend the effect of this change in the direction of the canal upon it. This may occur in perfect health, even to the extent of producing retention of urine, and perhaps some of the cases attributed to loss of tone in the muscular fibres of the bladder might be as truly explained thus; but in the next place, let it be supposed that from any cause Wilson's muscles should be seized with spasm in any of these cases already affected by the over-distension, and it must so shut up and close the urethra at that angle as to occasion absolute retention. The patient, then, in his efforts to relieve himself, will direct all the force of an excited bladder, aided by that of the abdominal muscles, on this very spot, already probably in a state of irritation, and disposed to become inflamed, and thickened and resisting; and let it be further imagined, that this shall happen again and again, at every repetition the mischief will be increased, until a permanent alteration of structure is finally induced." (p. 11.)

Mr. Rynd considers it doubtful whether rupture of the bladder ever occurs as a consequence of mere mechanical over-distension of urine, but states that the accident is always preceded by pathological changes, which soften its texture; and adds, that the rupture, in his experience, happens in the portion of bladder that is covered with peritoneum, the effusion being poured into the proper abdominal cavity. We subjoin a highly interesting case from Mr. Rynd's book, of recovery from rupture of the bladder.

"A boy, about ten years of age, standing on a dray or float on which there was a cask of sugar very imperfectly secured, unfortunately occupied the space between the front of the cart and its lading, when the horse accidentally fell, and the cask rolling forward jammed him against the cart, and crushed him frightfully. He was brought to the hospital with his pelvis extensively shattered, his left thigh broken just below the trochanter, and, as very shortly appeared, his bladder ruptured also. He suffered intense pain, especially in the abdomen, which he could scarcely allow to be touched, but as the obvious injuries were so extensive, the pain was chiefly attributed to them; and he was placed in bed, and had some warm drink and other restoratives administered previous to being dressed. He very soon expressed a desire to pass his urine, but could not void a drop, and a catheter was then introduced, which drew off a very small quantity, deeply tinged with blood, but afforded no relief. These symptoms continued with great severity for several hours, except that sometimes urine flowed through the catheter, as if on these occasions its beak had been pushed through the rent into the dépôt beyond it; but generally it did not, and in either case there was little or no alleviation of the distress. After the first twelve hours, the instrument always came out stained and blackened by sulphuretted hydrogen, and my colleague, Mr. Porter, under whose care he was, remarked this as being a fatal symptom, having never seen recovery after it,—a prognostic in which he was most agreeably disappointed. At the end of about forty-eight hours a small abscess pointed at the left side of the navel, which being opened gave an exit to an immense quantity of fetid pus and urine; this aperture became fistulous afterwards, and discharged clear and healthy urine whilst he remained in the hospital, which was more than three months. This boy recovered, and was seen and examined by several surgeons more than a year afterwards; the wound in the abdomen healed, and the water passed by the natural canal; but of the pathological process by which so fortunate a termination was obtained no one could form a conclusive opinion. It may be surmised that the extravasated urine had been more or less circumscribed, that it had induced inflammation, which poured out coagulable lymph, and thus formed a cyst, within which the fluid was completely insulated; but how it came to be regularly expelled afterwards, or what contractile force was exerted on it—in a word, how this newly-formed sac came to perform the functions of a bladder, as it seemed to do, is a problem, the solution of which surpasses my ingenuity." (p. 48.)

Our author makes some capital observations on urinary fever, and its liability to be confounded with intermittent fever and ague—a mistake which he has known to occur. He is an advocate for puncturing the bladder above the pubis, when all means fail of removing the urinary distension; and he states that this operation is preferred in Dublin to any other under these distressing circumstances.

In the course of the next chapter, which relates to the pathological symptoms of stricture, the author takes occasion to observe how sympathetic affections, seated in a different and somewhat distant part, occasionally mask the real disease and withdraw the attention from it. He has known a patient treated for piles whilst the real disease was stricture. The urine flowed away occasionally from the distended bladder and thus deceived the practitioner; whilst the piles, which were dependent upon the straining in making water, were alone subject to treatment. In respect to the treatment of stricture by caustics, the nitrate of silver of Sir Everard Home, or the caustic potash of Mr. Whately, the practice is condemned as uncertain and hazardous; an opinion for which reasons satisfactory to our mind are advanced by Mr. Rynd. He gives the highest praise to opium, employed freely during the dilatation of strictures by the bougie, and this accords with the experience of English surgeons.

The subject of fistula in perineo and its complications are treated of in this book in a remarkably clear and judicious manner. The remarks are too long to be transferred to our pages, and an abridgment of them would incur the danger of obscurity, but they may be read with profit by all reflecting surgeons. It may, however, be observed that their principal end is to inculcate the paramount importance of paying great attention to the condition of the urethra previous to attempting the cure of the fistula.

A fistula Mr. Rynd defines thus:

"A complete fistula may be regarded as a long abscess with two small apertures, neither of which is sufficiently large to give free exit to the matter that is constantly in process of formation, some of which must be as constantly seeking to escape from it, and thus keeping the orifices open and unhealed; an incomplete is exactly a similar abscess, only that it has but one aperture instead of two." (p. 131.)

Acting on the supposition contained in this definition, deep and extensive incisions are wholly unnecessary for the cure of fistula; all that is required is a large external opening in a depending position, so as to prevent the lodgement, and facilitate the discharge of all extraneous matters, including the secretion of the fistula itself. This proceeding, which is a much less serious one than that ordinarily adopted of laying open the sinus throughout its entire extent, has been attended with perfect success in the hands of Mr. Rynd.

The concluding chapter of the work is occupied by the consideration of dysuria produced by affection of the prostate gland. We quote the following passage relating to enlargement of the third lobe of the prostate gland:

> "When the middle lobe begins to enlarge, it cannot swell in a posterior direction, on account of the firm and unyielding nature of the vasa deferentia, which pass exactly behind and beneath it; it therefore pushes upwards into the cavity of the bladder, where it appears as a nipple-shaped tumour, varying in size from that of a small filbert to a walnut, and even more; and if at the same time the lateral lobes remain unaltered and of their usual size, the swelling draws with it a fold of the mucous membrane, which lies like a festoon on either side of it. The urethra, in this case, is compressed from before backwards, as well as elongated, and its vesical orifice resembles a *transverse* slit, with its posterior lip larger and more pouting than the other. Retention of urine now happens, by the stream of urine pushing forward the tumour against the orifice of the urethra, which it covers like a valve, the occlusion being in some instances rendered more perfect and complete by the festoon-shaped folds of membrane just described." (p. 165.)

In the treatment of this disease, Mr. Rynd proposes a silver catheter, where there is no obvious and decided objection to its use. The concluding pages treat of abscess in the prostate gland, which are divided into acute and chronic, the latter form being illustrated by the detail of two very instructive cases.

Here we must bring our notice of Mr. Rynd's treatise to a conclusion, not because we have exhausted its interest, but because the space at our command is limited, and we have already said, enough to give a fair sample of the work. It is, without doubt, an able and thoughtful production, and has our cordial recommendation as one of the best of the modern contributions to this branch of surgery.

Art. X.

1. *On the Nature of Limbs. A Discourse, delivered at an Evening Meeting of the Royal Institution of Great Britain.* By Richard Owen, F.R.S.—*London*, 1849. 8vo, pp. 120. With Four Plates.
2. *The Homologies of the Human Skeleton.* By Holmes Coote, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital.—*London*, 1849. 8vo, pp. 100.

Professor Owen's "Discourse" may be regarded as in reality a continuation of his work on the 'Homologies of the Vertebrate Skeleton,' noticed by us in our Second Volume, (p. 107); the purpose of it being to present in a more expanded form those views of the "General and Serial Homologies of the Locomotive Extremities," which were only briefly hinted at in his former work. Following the plan we pursued with regard to it, we shall endeavour to make our readers acquainted with the general *idea* which Professor Owen has in view; referring them to the work itself for the details of its development.

Every comparative anatomist is prepared to admit that the "arm" of man, the "fore-leg" of the beast, the "wing" of the bat or bird, and the "pectoral fin" of the fish are answerable or *homologous* parts; and in like manner he recognises the conformity of the lower extremities of man with the posterior limbs of beasts and birds, and with the ventral fins of fishes,—notwithstanding that these last may be situated in immediate contiguity with the pectorals, or even anteriorly to them. But there has been considerable difference of opinion as to the points involved in their more detailed comparison; and this difference has been yet greater in the determination of the nature of the *repetition*, or *serial homology*, which all have acknowledged to be discernible as existing between the anterior and posterior extremities. Few, save the transcendental anatomists of Germany, have attempted to determine the essential relation of these parts to the vertebral column, or, in other words, to ascertain their *general homology*. These are the questions to which Professor Owen has addressed himself, and of which he has given what is in our apprehension a perfectly satisfactory solution.

After such a comparison between the anterior and posterior extremities of various animals, as serves to show the essential conformity that exists between them, notwithstanding their teleological modifications for particular purposes, Professor Owen proceeds to compare those of man, in whose limbs these teleological modifications have considerably obscured the fundamental conformity of type. No one has any difficulty in recognising the similarity of the arm and the thigh, the fore-arm and the leg, the hand and the foot; and it is not until we begin to compare the individual bones, that our difficulties begin,—the chief of these difficulties being to determine the answerable or homologous bones of the fore-arm and leg. Vicq-d'Azyr, who was the first anatomist who attempted to work out these serial correspondences in detail, at least in the human frame, correctly interpreted the homology of the scapula and ilium, the humerus and femur, the bones of the hand and those of the foot; but being led astray by the different proportions of the ulna and fibula in the human skeleton, he put forth the strange notion that the anterior extremity is not paralleled or repeated by the posterior one of its own side, but by that of the opposite side, the right arm by the left leg, and *vice versâ;* the ulna being thus made the representative of the tibia, and the radius of the fibula; and the patella being supposed to be paralleled by the olecranon. This notion was adopted by Cuvier, whose extended knowledge of comparative osteology might be supposed to have furnished a corrective of it. A still more extraordinary hypothesis has been propounded by Cruveilhier, which shows the absurdity of framing such comparisons upon the human skeleton alone. According to him, the upper end of the tibia is represented by the upper half of the ulna, and the lower half of the tibia by the radius; whilst the fibula is represented by the upper half of the radius and the lower half of the ulna. "Nature, however (as Professor Owen justly remarks), when rightly interrogated and propitiated by due observant service, extricates us from these complex involutions and alternations of serial homology, and makes the simple truth plain." The late Dr. Barclay of Edinburgh seems to have been the first to discern the truth, to which he was guided by his extensive knowledge of comparative anatomy. In his 'Explanations of Mitchel's Plates of the Bones (1824), he pointed out the conformity between the ulna and the fibula, and showed how they exhibit the same variety and unsteadiness of character, being sometimes large, sometimes small, and sometimes merely processes of the more constant bone of their respective segments. Several years later (1838), M. Flourens took up the inquiry, probably in ignorance of Dr. Barclay's views; and arrived at the same results.

The great development of the olecranon, or upper end of the ulna, as compared with the upper end of the fibula, is the chief stumbling-block in the way of the recognition of their conformity; but this is at once removed by reference to those cases in which the fibula manifests a similar development. The marsupial and monotrematous animals, many of which have a rotatory motion of the hind-foot analogous to the pronation and supination of the hand, and in some of which the great toe is an opposable thumb,—the whole foot being thus metamorphosed into a hand, whilst the anterior extremity is more like an ordinary foot,—are fertile in examples of excessive development of the fibula; its parallelism with the ulna becoming apparent, wherever the endowments which the limb is to possess necessitate a similar modification of its form. In the wombat, the fibula not only shows the broad, flat, and high process from its proximal end, which obviously represents the olecranon; but also has a superadded sesamoid, that parallels the sesamoid at the back of the elbow-joint of the bat, which has been considered a detached olecranon, representing the patella. The true homotype of the patella in the bat is found by Professor Owen in a sesamoid contained in the tendon of the biceps.

In the comparison between the carpal and tarsal bones, Vicq-d'Azyr was more successful. We place his determinations in a tabular form, that they may be more readily apprehended.

PROXIMAL ROW.		DISTAL ROW.	
Carpus	*Tarsus*	*Carpus*	*Tarsus*
Scaphoides	Scaphoides	Trapezium	1st Cuneiform
Lunare	Astragalus	Trapezoides	2d Cuneiform
Cuneiforme	Calcaneum	Os magnum	3d Cuneiform
Pisiforme		Unciforme	Cuboides

The mere human anatomist may feel a difficulty in admitting these homologies, in consequence of the difference between the position of the scaphoid in the wrist and in the ankle, and of the development of the calcaneum from a single osseous centre, which seems opposed to the idea of its representing two carpal bones. This difficulty entirely vanishes, however, when the comparison is carried on through other parts of the vertebrate series; and it then appears that the normal or typical number of bones in the carpus or tarsus is *ten*, each metacarpal or metatarsal bone being supported by its own ossicle in the distal series, and having a corresponding bone in the proximal series. This is especially obvious in the Chelonian reptiles, which have the unciform bone and the scaphoid each divided into two; and this division of the scaphoid is presented by many of the higher classes, being even retained by the orang-utan, whose carpus presents a very interesting transition, in the form and arrangement of its bones, between that of man and that of the lower animals, and very strongly resembles its tarsus. Thus while in the carpus of man, the scaphoides and lunare are on the same parallel transverse series, and equally participate in the articulation with the radius, in that of the orang the divided scaphoid is much less connected with the radius, and is in great part interposed between the lunare of the proximal row and the trapezium and trapezoides of the distal row, so as to occupy almost exactly the same position as the scaphoid of the tarsus. In the turtle, the relative positions of the two are precisely analogous; the connexions of the divided carpal scaphoid being altogether the same with those of the undivided tarsal scaphoid. Hence it is obvious that the difference in position of the scaphoid in the carpus and tarsus of man is merely *adaptive*, and does not involve any real departure from their conformity of type. And it is also obvious that a bone may be compound,—that is, may be a representative of two others, as in the case of the undivided scaphoid in the carpus of man,—without having a double ossific centre; so that the difficulty respecting the compound character assigned to the calcaneum altogether vanishes. There are many similar examples of connation or blending of these bones, which Cuvier did not fail to recognise; thus, in many ferine, rodent, and marsupial quadrupeds, the carpal scaphoid unites with the lunare; in the carpus of the sloths, the trapezium blends with the scaphoid; and in the tarsus of the ruminants, the cuboid is anchylosed to the scaphoid. Upon precisely similar evidence, Professor Owen is led to recognise the compound nature of the unciform and scaphoid bones in the human carpus, and of the cuboid, scaphoid, and calcaneum in the tarsus; thus educing the constancy and regularity of the typical plan of these organs, out of the apparent irregularity which arises from their modification as instrumental structures, adapted for special functions.

Another important and instructive result of such comparisons, is the recognition of the constancy of the relations of the distal series of carpal and tarsal bones, whether simple or compound, with the five digits with which they essentially correspond in number.

> "By this constancy of connexion we are able to determine the precise digits that are lost and retained when their number falls below the typical 5; to point out, for example, the finger in the hand of man that answers to the fore-foot of the horse, and the toe that corresponds to its hind-foot; nay, the very nail in the hand or foot, which becomes by excess of development the great hoof of the horse. Were anything wanting to impress the thinking mind with the conviction of the unity of type which pervades animal structures, it might be such a fact as this." (p. 30.)

In order to express the results of such comparisons briefly but plainly, we have placed in a tabular form those with which Professor Owen furnishes us in the present Memoir; the figures 1, 2, 3, 4, 5, used for man, indicating the presence of their respective digits in a condition of moderate development; the smallest figures expressing the rudimentary condition of the digits they represent; whilst the largest indicate an excess of development.

Man . . .	1	2	3	4	5
Spider-monkey . .	$\scriptstyle 1$	2	3	4	5
Bandicoot . . .		2	3	4	$\scriptstyle 5$
Rhinoceros . .		2	3	4	
Ox		$\scriptstyle 2$	$\Large 3$	$\Large 4$	$\scriptstyle 5$

The foot of the Horse is usually regarded as formed upon the type of that of the ruminants; the confluence which in them is exhibited in the "cannon-bone," which is formed by the union of the two metacarpals, having extended to the phalanges. Its formula would thus be $^{2}\ \overbrace{\underbrace{3\ 4}}\ ^{5}$. It is affirmed, however, by Professor Owen, that this is not the real plan of the structure of the foot of the Solidungula, and that its resemblance is greater to that of the tridactyle rhinoceros, than to that of the tetradactyle ruminants;—the single toe being really formed by the excessive development of the third digit alone, instead of being formed by the confluence of the third and fourth; and the two rudimentary digits being the second and fourth, instead of the second and fifth. Its formula will thus be $^{2}\,3\,^{4}$. The extinct Palæotherium affords a link of connexion between the foot of the rhinoceros and that of the horse.—We believe that it might be objected to such a determination, that monstrosities occasionally occur, in which the usually monodactyle foot of the horse presents the division of that of the Ruminants, apparently from the want of the usual confluence; but as we cannot positively affirm this to be the case, we can only throw out the suggestion, to be inquired into by those who may feel an interest in the subject.

A perfect and beautiful parallelism reigns between the order in which the toes successively disappear in the hind-foot, and that which has been shown to exist in the fore-foot; the first or innermost digit being almost invariably the first to disappear, the fifth or outermost being the next, and the second and fourth being more or less arrested in their development, whilst the third or central digit remains predominant to the last.

The following remarks form an apposite conclusion to this part of the Lecture:

> "If we pause to take a retrospect of the ground over which we have been travelling, and consider the numerous and beautiful evidences of unity of plan which the structures of the locomotive members have disclosed,—evidences so little to be expected, *à priori*, seeing the different shapes and sizes of instruments adapted to such diversity of functions;—when we also find that, besides the general conformity of structure in the limbs of different species, a more special parallelism could be traced between the fore- and hind-limbs of the same species, no matter to what diversity of office they might be severally adapted—a parallelism or 'serial homology,' demonstrable even to each little carpal and tarsal bone, from man down to the monodactyle horse,—the thinking mind cannot but be forcibly struck by such facts, and be impelled with the desire to penetrate further, and ascend if possible to the higher law or generalization from which these harmonies flow.
>
> "I think it will be obvious that the principle of final adaptation fails to satisfy all the conditions of the problem. That every segment and almost every bone which is present in the human arm and hand should exist in the fin of the whale, solely because it is assumed that they were required in such number and collocation for the support and movements of that undivided and inflexible paddle, squares as little with our idea of the simplest mode of effecting the purpose, as the reason which might be assigned for the great number of bones in the cranium of the chick, viz. to allow of the safe compression of the brain-case during the act of exclusion, squares with the requirements of that act. Such a final purpose is indeed readily perceived and admitted in regard to the multiplied points of ossification of the skull of the human fœtus and their relation to safe parturition. But when we find that the same ossific centres are established, and in similar order, in the skull of the embryo kangaroo, which is born when an inch in length, and in that of the callow bird that breaks the brittle egg, we feel the truth of Bacon's comparison of 'final causes' to the vestal virgins, and perceive that they would be barren and unproductive of the fruits we are labouring to attain, and would yield us no clue to the comprehension of that law of conformity, of which we are in quest. And so, again, with regard to the structural correspondences manifested in the locomotive members; if the principle of special adaptation fails to explain them, and we reject the idea that these correspondences are manifestations of some archetypal exemplar, on which it has pleased the Creator to frame certain of his living creatures, there

remains only the alternative that the organic atoms have fortuitously concurred to produce such harmony.

"But from this Epicurean slough of despond every healthy mind naturally recoils; and reverting therefore to the hypothesis of the dependence of special and serial homologies upon some wider principle of conformity, we have next to inquire, what is the archetype or essential nature of the limbs?" (p. 40.)

The answer to this question is contained in the latter half of the lecture; which gives a general survey of that beautiful train of reasoning, built upon the comprehensive basis of the facts supplied by comparative anatomy, by which Professor Owen has been led to establish the following positions:

1. That the scapulo-coracoid arch is a repetition of the costo-sternal arches, modified in its development for the purpose which it is to serve. This idea had previously occurred to Oken.
2. That, as the costo-sternal are the "hæmal" arches of the dorsal vertebræ, so the scapulo-coracoid is the hæmal arch of the occipital vertebra of the skull.
3. That the arms are to be regarded as "diverging appendages" of this hæmal arch, and as analogous to the processes which project backwards on either side from each rib of the bird, overlapping the next rib, and which enter into the general idea of a "vertebra."

We cannot do more than give a mere outline of the facts and arguments adduced in support of these propositions.—1. The proof of the first is drawn from the fact, that, in proportion as the anterior limb is simplified in structure, the scapulo-coracoid arch exhibits an approximation to the costo-sternal, until, in its most degraded forms, it becomes evident that the scapula answers to the pleurapophysis or vertebral rib, and the coracoid to the hæmapophysis or sternal rib, the clavicle being a superadded bone, confined to the higher classes.—2. The second is established by the fact, that the scapulo-coracoid arch in Fishes is always in more or less evident relation with the occipital vertebra; completing that segment, which would show a deficiency of the hæmal arch if this be not allowed to possess that character. The dislocation of this hæmal arch, even through several segments, cannot be regarded as a difficulty by the really philosophical anatomist, who considers the numerous other instances of such dislocation, of greater or less extent, which the structure of almost every vertebrated animal presents. Thus, in Man we find the greater number of the costal arches displaced to the extent of half a segment; each being connected with the vertebra below that to which it belongs, as well as with the one of which it is properly an appendage. In Birds and Dinosaurian reptiles, the neural arches of the sacrum are in like manner displaced from the bodies of their respective vertebræ; and a similar displacement is apparent in the dorsal vertebræ of the Chelonia. If we admit the pelvic arch and ventral fins of Fishes into the argument, the case is still stronger; since, as every anatomist and zoologist is aware, these may be shifted forwards along the vertebral column, so as even to take a position in advance of the pectorals. The very same kind of inquiry which leads the human anatomist to restore the displaced costal arches to their respective segments, unequivocally tends to establish the fact above asserted in regard to the relation between the scapulo-coracoid arch and the occipital vertebra of the skull.—3. The third proposition rests securely upon a similar broad foundation. If we follow the limb downwards from its most complex to its most simple forms, we shall find it reduced in the proteus to a mere trifid ray; in the amphiuma, this ray is bifid; while in the lepidosiren it is unbranched, though consisting of numerous segments. This ray holds precisely the same relation to the rib-like scapulo-coracoid arch of the lepidosiren, that the diverging appendages of the true ribs do to the costal arches in birds; a position which is further evidenced by the fact, that even in the apteryx, the rudimentary wing-bones, which are monodactyle, are reduced nearly to the dimensions of the appendages of the costo-thoracic arches.—Thus, then, concludes Professor Owen,—

> "Until the alleged facts are disproved, demonstrating change of position to be one of the modifications by which parts of a natural and recognisable endoskeletal segment are adapted to special offices, and until the conclusions deduced from those facts are shown to be fallacious, I must retain the conviction that, in their relation to the vertebrate archetype, the human hands and arms are parts of the head,—diverging appendages of the costal or hæmal arch of the occipital segment of the skull." (p. 70.)

It is not difficult to show that the pelvic arch is composed of the same elements with the scapular; indeed the "serial homology" or "repetition" of their essential parts has been recognised by all comparative anatomists. It is, therefore, a valuable confirmation of the doctrine which has been advanced in regard to the anterior extremities, to find that it is fully borne out by the phenomena which are presented to us in the study of the posterior; the pelvic arch in its most degraded form being obviously a repetition of the costal; and the hind-limbs, like the fore, being nothing else than "diverging appendages" of that arch. The requirements of the higher animals, however, frequently necessitate its excessive development, so that it is applied to many segments besides the one to which it properly belongs.—It is remarkable that, among fishes, in which it might be difficult to assign the true place to the pelvis and its limbs, the scapular arch should present its normal position; whilst it is in the higher vertebrata, in which the pelvis is fixed, that we meet with the great deviation in the position of the scapular arch, which masks its true relation to the occipital vertebra. But, as Professor Owen justly remarks,—

> "Wherever either arch with its appendages may be situated, it is in its best possible place in relation to the exigencies and sphere of life of the species. It is only when we consider its relations to the ideal exemplar, that we are compelled to use the terms expressive of rule and deviation." (p. 81.)

Although we have on former occasions pointed out how completely the mere notion of "adaptation" fails to satisfy the wants of the philosophical anatomist, yet we cannot deny ourselves the pleasure of quoting the following from Professor Owen's concluding remarks:

> "Those physiologists who admit no other principle to have governed the construction of living beings, than the exclusive and absolute adaptation of every part to its function, are apt to object to such remarks as have been offered respecting the composition of the skeleton of the whale's fin and of the chick's head, that 'nothing is made in vain:' and they deem that adage a sufficient refutation of the idea that so many apparently superfluous bones and joints should exist in their particular order and collocation in subordination to another principle; conceiving, quite gratuitously, in my opinion, the idea of conformity to type to be opposed to the idea of design.—But let us consider the meaning which, in such discussions, is

commonly attached to the phrase 'made in vain.' Were the teleologist to analyse his belief in the principle governing organization, he would, perhaps, find it to mean, that so far as he could conceive of mechanism directly adapted to a special end, he deems every organic mechanism to have been so conceived and adapted. In a majority of instances he finds the adaptation of the organ to its function square with his notions of the perfection of a machine constructed for such an end; and in the exceptional cases, where the relation of the ascertained structure of an organ is not so to be understood, he is disposed to believe that that structure may be, nevertheless, as directly needed to perform the function, although he perceives that function to be a simple mechanical action, and might conceive a more simple mechanism for performing it. The fallacy perhaps lies in judging of created organs by the analogy of made machines; but it is certain that, in the instances where that analogy fails to explain the structure of an organ, such structure does not exist 'in vain,' if its truer comprehension lead rational and responsible beings to a better conception of their own origin and Creator. Our philosophic poet felt that—

'’Tis the sublime of man,
Our noontide majesty, to know ourselves
Parts and proportions of a wondrous whole.'—*Coleridge.*

Nor could the ignorance of this truth be without its benumbing and bewildering influence on the human mind." (p. 85.)

In concluding our notice of Professor Owen's 'Discourse,' we have only to express our thanks to him for this very acceptable contribution to the science of philosophical anatomy; and for the lucid and masterly demonstration with which he has favoured us, of the propositions which his profound researches had previously led him to put forth in a form less adapted for the average of readers. Those who desire to know something of the nature of the investigations on which he has been engaged, and who yet shrink from the attempt at mastering the intricacies of the "vertebral theory of the skull," cannot do better than apply themselves to the attentive perusal of this Discourse; from which, we venture to say, they cannot rise without being satisfied with the line of argument adopted, and with the conclusions arrived at, unless their minds have been previously closed against conviction.

It is not requisite that we should notice at any length the treatise of Mr. Holmes Coote; since it does not profess to contain anything original, its aim being "to give a brief account of the signification of the different bones composing the human skeleton, and to familiarize the mind of the student in anatomy with the idea that the whole body is formed of a succession of vertebral segments;" and the system of Professor Owen being followed throughout. The design is a most excellent one; and great praise is due to Mr. Coote, for being the first to endeavour to improve the teaching of anatomy, by thus bringing down some of its most important principles to the comprehension of the learner. We are always inclined to deal kindly with a first attempt at authorship, where the aim is good and the capacity of the writer manifest; and we shall therefore satisfy our critical conscience by the remark, that the manner of Mr. Coote's production is not so good as its matter. The selection of materials is on the whole judicious; but their arrangement is by no means so clear as it might be made with a little attention to the subdivision of sections, running titles, &c.; and the entire want of illustrations will, we fear, greatly impair the value of the production to those who have not the means of referring to the actual objects adverted to. We must remark, moreover, that we are by no means convinced that, in the present state of medical education, it is by any means desirable that the student should be expected, or even encouraged, to pursue the study of philosophical anatomy, under the guidance of this or any other special treatise on the subject, if in so doing he be led to occupy any portion of the time which may be more profitably devoted to the study of more directly practical subjects. That which we desire is, to see anatomy taught, whether by lectures or by books, on the basis of these principles, and with continual reference to them; just as chemistry is taught on the basis of the laws of affinity, and physiology upon those of cell-life. We are persuaded that it may thus be rendered far more interesting to the student than it is made at present, and that he may be led to derive a far greater amount of intellectual benefit from the study, without any increase, but rather with a diminution, of the time and attention required from him. We know that this idea has been realized in his oral instructions by one of the most distinguished of the anatomical teachers of this metropolis; and we shall hope, ere long, to possess a treatise on Osteology constructed upon this plan. As a supplement to the treatises already existing, Mr. Coote's work will afford to the present generation of students their most accessible means of gaining the desiderated acquaintance with "the homologies of the human skeleton."

ART. XI.

1. *The Cyclopædia of Anatomy and Physiology.* Parts XXX-I, Article "*Products, Adventitious.*" By W. H. WALSHE, M.D., Prof. Clin. Med. Univ. Coll. Lond.
2. *On Cancerous and Cancroid Growths.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., &c.—*Edinburgh,* 1849. pp. 260.

THESE two works have been written to answer different purposes, and have therefore assumed very different forms. That of Dr. Walshe is intended to include a number of details in an extensive generalization, and to arrange under a common heading an immense mass of facts. That of Dr. Bennett commenced in an endeavour to penetrate into a single point, and from thence expanded into a wide inquiry. In the one case, the general idea preceded the assemblage and colligation of the facts; in the other, the record of the facts gradually widened into the generalization. Both works, however, agree in this, that they are of singular merit, and reflect no little credit on the British school of Pathology.

We deem it but just to call Dr. Walshe's article a Work, if this term is to be given to what is, as far as it can be, complete in itself, and independent of the other sections of the publication in which it appears. In point of length merely, this article would form a volume of no inconsiderable size, as it occupies no less than seventy-seven of the closely printed pages of the Cyclopædia. Even this space has been found too narrow for the subject. The writer has been compelled to compress his details to the utmost, to communicate his facts in the tersest way consistent with clearness, and to exclude as much as possible physiological speculation. This condensation was in fact necessitated, by the design of the Article;

for this is intended to include a description of every substance, which, being a product of, or developed in connexion with, the animal frame, is yet foreign to that frame, or to the part of it in which that substance is found. The extent of the subject will be at once apparent. There is hardly any department of medicine which must not be laid under contribution; and so constantly does adventitiousness, either as to nature, form, or locality, occur in disease, that a sketch of foreign products is almost an outline of pathology itself.

Dr. Walshe divides "adventitious products" into solid, liquid, and gaseous. The most important class, the solid adventitious products, are then divided as follows:—First, a grand distinction is drawn between such as are non-plastic, that is, destitute of structure, or of the power of ever exhibiting it; and such as are plastic, that is, possessed of definite structure, or of the power of assuming it. The first class comprises, of course, not only all mineral substances which may form in, or be separated from, any of the animal fluids; but also those organic substances which, in respect of physiological attributes, take rank with minerals, such as sugar, certain protein compounds, as albumen in particular cases, fat, &c. Under the first division of this class, therefore, are arranged all crystals forming in urine, fæces, in the products of inflammation, in specific fluids, &c.; all masses and agglomerations of mineral substances, such as calculi of all kind, urinary, renal, prostatic, salivary, lachrymal, nasal, preputial, gastro-intestinal, biliary, pancreatic, mammary, &c., and all pseudo-calculi, made up of saline matters, deposited in a pre-existing morbid stromal or non-stromal, or in a natural organic basis, as advanced atheroma, arteroliths, phleboliths, arthritic tophaceous deposits, &c. Under the second division of the class are placed such organic foreign substances as are incapable of further organization: albumen in the secretions, fibrin in the secretions; fat in various parts, such as the liver, the kidney, lungs, &c.; sugar in the secretions, &c. The number of subjects passed in review is really surprising; and the short notices of the most important diseases connected with the foreign bodies, such as diabetes, albuminuria, fatty liver and kidney, urinary derangements, &c., are done in a comprehensive and masterly style.

But this class, extensive as it is, and important as are the items which compose it, is, after all, a trifling undertaking compared to the discussion of the "plastic adventitious products." Under this heading comes every organic foreign product, the blastema of which gives rise to cells; whether these are deficient in vegetative or reproductive power; or, being possessed of this, are deficient in permanency, that is, possess no power of assuming a similarity in structure to the natural tissues, and of playing the same parts as these; or whether the cells are possessed both of vegetative power and of permanency, that is, of a power of taking on the character and the stability of natural tissues. These differences in power of propagation and in permanency form, therefore, three orders. The first order, composed of formations constituted by non-vegetative cells, comprises the products termed by the author "deposits," viz. the typhous, tuberculous, purulent, melanic, and diphtheritic. The account of these deposits is very complete, and is admirably written, although, as usual, compressed to the utmost. The second order, viz. of "growths," formed by vegetative non-permanent cells, is divided into two sub-orders, according as the growths have the power of infiltrating or not the surrounding parts. The non-infiltrating growths, of protein, fat, or gelatine-basis, comprehend hæmatoma, sarcoma, cystoma, angiectoma, lipoma, steatoma, cholesteatoma, fibroma, enchondroma, osteoma, and colloma. The infiltrating growths make up the great class of carcinoma. Into the validity of this distinction between infiltrating and non-infiltrating growths, we shall have presently to inquire. The third order of structures, composed of vegetative permanent cells, comprises the "pseudo-tissues."

Under the heading of plastic formations are also placed the products which are not derived from a blastema but from germs, such as animal and vegetable parasites.

The arrangement by which so many distinct and various products of the body are brought together is, no doubt, artificial. The fact of these products being "adventitious," does not at first sight appear to constitute any valid link between them, or to imply any connexion so obvious as to warrant their being treated of together. What should induce us to treat of urinary calculi, lipoma, colloid cancer, and mycodermatous vegetations in the same article? In answer to this it may be said, that there is a real and great advantage gained by bringing into a point of view which may admit of comparison all these various products, which in systematic works are so widely separated. By this comparison we are often enabled to perceive unexpected alliances or antagonisms, which may equally throw light on the nature of the compared substances. And also this character of adventitiousness, though it may be an artificial bond of union between the great divisions, is yet a natural one between the different items of the sub-orders. The several deposits occurring in the course of acute or chronic diseases, as the typhous, the tubercular, and the diphtheritic, have a natural relation to each other, which demands that their mutual alliances and connexions shall be elicited. So also the several growths which are formed of vegetative cells have certain characters in common, which claim comparison; and yet in works on the practice of medicine these growths are so widely separated, that comparison is almost impossible.

It must, however, be conceded that this link, although convenient, is in some respects artificial, and that consequently it is not easy to give an exact definition of what constitutes an adventitious product. In addition to the idea of unlikeness to the body or to its parts, does adventitiousness include the idea of excess? In fatty liver, the fat is foreign to the interlobular passages; it is only in great excess in the hepatic cells. Is this excess of fat sufficient ground for terming the fat in the cells "adventitious," as it is not here in quantity in the healthy state?

Again, are we to include in the description of adventitious products those subtle agents of contagious diseases, which, composed possibly of evanescent cells, are in some cases formed by the living body? They are developed in connexion with the body, and are adventitious; but would it be right to include the diseases resulting from them with diabetes, Bright's disease, or cancer, because in all, there is a product formed which is abnormal to the living system?

These few remarks may serve to illustrate the magnitude of the task which has been executed by Dr. Walshe. We can safely say that we know few men who would have treated it so ably. Among the vast range of subjects which he has touched, there is hardly one on which his remarks may not be read with profit, even by those most conversant with the matter discussed; and for an accurate yet concise recapitulation of the most important facts of the morbid anatomy and chemistry of the adventitious products, we do not know in any language an article which is superior to it.

We may pass very lightly over the class of non-plastic products. The urinary deposits and the various calculi are accurately described, and are illustrated by some good engravings. In speaking of oxalate of lime deposits, we are informed that this salt so frequently occurs temporarily at the commencement of convalescence after acute diseases, that Dr. Walshe regards its sudden appearance as a sign of that change.

With reference to the pathology of diabetes, the author seems inclined to adopt the opinion of Graham, that sugar accumulates in the system, or passes off through certain excretions, in consequence of a deficient oxidation in the respiration-process. The cause of this arrest of sugar-combustion remains, however, completely unexplained.

In the second class, the description of tubercle is both comprehensive and concise. Dr. Walshe believes that the gray granulation is rightly termed tubercle, and that it may be considered as the first stage of the disease in some cases, while in others, yellow tubercle is primarily deposited, and the first or gray stage is, as it were, passed over. Another special kind of gray granulation was that described by Bayle; the exact nature and relation of which to tubercle is still undecided. With regard to the cure of tubercle, while it is stated that unsoftened tubercle frequently undergoes cure either by degradation, i. e. by passing into a substance still lower in the scale than itself, and incapable of further change, such as cretaceous matter, or by infiltration with contractile exudation-matter, it is contended that, when tubercle has softened and formed cavities, such cavities rarely, if ever, become obliterated. This last opinion is supported by a critical examination of the affirmative evidence of Laennec, who, as Dr. Walshe believes with Fournet, mistook simple superficial puckering, either from intra- or extra-pulmonary changes, such as deposit of lymph, &c., for contracted and healed cavities. On this point, however, we consider that doubts may still exist. Certainly, superficial puckerings have been often described as obliterated cavities, and we are disposed to think Dr. Walshe to be entirely correct in believing such obliteration to be very rare. But every now and then cases are met with in the dead-house, in which the manner in which a cyst, or rather a layer of pulmonary substance rendered dense and lined by exudation-infiltration, is contracted partially or entirely round a central cretaceous mass of variable size, seems strongly to indicate an actual shrinking and approximation of the walls of the cavity. Dr. Walshe, however, explains very ingeniously the series of changes which may simulate such an appearance. The point must be left for future inquiry. We observe that Dr. Bennett, in the 32d Observation, speaks of healing tubercular cavities in the lung; and in the same author's work on Cod-liver Oil, there are, if we remember right, several alleged examples of such changes.

Passing over the next section, on pus, we find a lucid description of melanic deposit, and of the black discoloration simulating it, which was so ably described by Carswell under the term of spurious melanosis.

The melanic or pigment-cells are usually rounded, but may be caudate or elongated, or may assume almost any shape; they contain a number of blackish bistre-coloured or yellowish granules, which are also scattered between the cells, and sometimes constitute the whole of the black matter, being uninclosed in cells. The cells are seldom nucleated; their mode of increase is not well defined, and they never exhibit any tendency to adhere. They do not form the basis of a stroma, although we believe they may be found within the meshes of a fibrous stroma of new formation, which then constitutes the tuberiform melanosis of Carswell. This is quite distinct from the "melanic cancer" of some writers, the "carcinoma melanodes" of Müller, in which melanic matter is merely infiltrated into, and forms an accidental constituent of, an encephaloid or scirrhous mass. The true tuberiform melanosis is often seen in the horse, and it is a curious fact noticed by Carswell, that it is much more common in the white and gray horse than in the dark-coloured horse. Indeed, Carswell states that he never found tuberiform melanosis in the black horse. Dr. Walshe mentions that the stromata of melanotic tumours are in some cases composed of enlarged vessels, in others, are cystomatous, in others fibrous. With regard to the origin of melanic matter, he seems to consider it probable that the view which connects it in some way with hematosine is the correct one.

The second order, comprehends the "growths," viz. the structures made up of evanescent vegetating cells. All the growths present one common and unfailing anatomical character; they are constituted by a stroma, and by interstitial matter occupying its meshes. The relative amount of these two constituents, their arrangement and their definition, vary in the several species. Growths possess blood-vessels, which, like the stromal and inter-stromal elements, are of new formation; and they display occasionally nerves and lymphatics, which are in all probability not new formations. The ultimate elements of growths are granules, molecules, cells, free nuclei, and fibrils. With these elements are *accidentally* associated precipitates, deposits, exudation-products, and certain of the simpler pseudo-tissues, such as epithelium, cartilage, cellular, serous, fibrous, elastic, true osseous, cutaneous, and dental tissues, the last three being limited to cystoma.

Our space forbids us to enlarge on these topics, although there is much highly interesting matter which we should wish to extract. Dr. Walshe assigns three modes of origin to cells and fibres. In respect of cells, the nucleus may appear primarily or secondarily to, or contemporaneously with, a cell-wall. In this last case, the perfect cell seems at once to be formed. Fibres also may be formed in three ways: by cell elongation or division; by nuclear elongation and juxtaposition; by immediate fibrillar precipitation from a fluid blastema.

The "conditions of localization" of growths are thus laid down by Dr. Walshe:

"(*a*) The tendency to become the seat of growths, as a class, varies greatly in the different tissues and organs. Thus, while cellular tissue is their peculiarly favorite site, fibrous texture but rarely affords them a nidus. Again, the mamma, the ovary, the uterus are frequent, the lungs and brain much less common, sufferers.

"(*b*) The tendency to become the seat of growths, as a class, varies in the different parts of organs. Thus, the pyloric end of the stomach suffers more frequently than the rest of the organ; the epididymis than the body of the testis.

"(*c*) Certain organs, and certain parts of organs, have an excess of tendency to the formation of certain special growths. Thus, the uterus, the mamma, the stomach, the liver, are peculiarly prone to cancerous, as distinguished from other forms of growth; the bones are the chosen seat of enchondroma. And, again, cancer does not form indifferently in all parts of the uterus, but tends especially to invade its neck; while fibrous tumours affect a preference for the body of the organ. The large intestine is a tolerably common seat of cancer; the small is very rarely implicated.

"(*d*) Growths of different kinds exhibit different degrees of compatibility, as coexistences in the same body. Some growths, as cystoma and carcinoma, are sufficiently prone to appear in the same individual; others, as fibroma and carcinoma, are rare coexistences; none are actually incompatible." (p. 123.)

With regard to the formation of secondary growths in distant organs, as in cancer, and more rarely in the case of fibrous tumours, Dr. Walshe seems inclined to refer it to contamination by absorption; absorption, however, being occasionally "unproductive," that is, failing to contaminate, on account of modifying circumstances.

We have room only for one extract respecting the simple kinds of growth; it has reference to the possibility of fibrous tumours becoming cancerous. Dr. Walshe says:

"The difficulty in deciding this question has arisen from the total want of definite meaning in the minds of authors as to what constitutes 'becoming cancerous.' If the phrase be applied in the manner which seems the only rational and sound one, that is, to parts, whether adventitious or not, in which the development of one or the other of the three species of cancerous formations occurs, the perplexity of the question vanishes at once. The growth of cancerous substance in fibrous tumours is, in truth, at the least, materially more rare than in any natural vascularized tissue."

There seems, in fact, no doubt that when cancer really arises in connexion with a tumour originally purely fibrous, it is merely that the cancerous blastema has been poured into that fibrous matrix, as it might have been poured into the interspaces of any normal part of the body.

The section on the infiltrating growths is very much shortened, as the writer, instead of entering into much detail respecting cancer, refers to his work* on that subject, the doctrines and statements of which require no or but little alteration. Such alterations as have been made since this work was published, will be alluded to in our analysis of Dr. Bennett's book.

The second work on our list is from the well-known pen of Dr. Hughes Bennett, and we do not hesitate in pronouncing it worthy of the high reputation of its author. It has been composed in the following way:—For a series of years Dr. Bennett, in his capacity of pathologist to the Edinburgh Infirmary, has examined every tumour which has been removed by operation from patients admitted into that institution; the greater number of these tumours were supposed at the time of removal to be malignant, or were dangerous on account of their bulk or position. Dr. Bennett has also examined various tumours which have been sent to him by operators, who were aware of his interest in the subject of "growths." In this way he has collected observations on the physical and microscopic appearances of fifty-six tumours; the detail of these observations constitutes the first part of the work; the microscopic descriptions are illustrated by woodcuts, which are in many instances beautifully executed. In the second part of the work Dr. Bennett enters on a general discussion on cancer and cancerous growths, which is founded chiefly on the previous observations. These observations are, then, the basis of the whole work; and whatever value may be attached to Dr. Bennett's opinions, will mainly depend on the accuracy with which they were made. We will enter then, first of all, into as strict an analysis of them as our limits will permit.

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* Nature and Treatment of Cancer. London, 1846.

The following is the method in which each observation is recorded. A short detail of the case is first given, and this is followed by an account of the physical appearance of the tumour, and of its microscopic structure; a brief commentary, which gives Dr. Bennett's view of the nature of the case, concludes the observation. The microscopic examination is made in the following way:—a section being made, the tumour is squeezed; if any fluid escapes, a drop of this is placed under the microscope, and examined in the usual way with water, acetic acid, and other reagents; if none escapes, the surface is scraped; sections are afterwards made of the solid part of the tumour with Valentin's knife, by which means the stroma of the tumour, and the relations of the intrastromal contents to each other and to the walls, can be more or less clearly made out. We shall extract the microscopic examinations of the 1st and 7th Observations, as illustrations of Dr. Bennett's method. In the first case, the tumour examined was true scirrhus of the mamma, and was cut out by Mr. Syme. On section it grated under the knife, and on pressure yielded a milky fluid.

> "A thin section was removed from the centre of the tumour by means of Valentin's knife, and examined with a power of 250 diameters linear. It consisted of a meshwork of fibrous tissue, forming waved bands, arranged here and there in circles, varying in size. Some of these were very large (one fifth of a millimeter in diameter), inclosing other circles, each of which was surrounded by several filaments of the fibrous tissue. Some of these circles contained numerous nucleated corpuscles crowded together, mixed with granules; others contained only a few, or nothing but granules. Here and there were to be seen several compound granular corpuscles. On adding acetic acid to the whole structure, it became more transparent. Many of the fibrous filaments became invisible, and such as remained were studded here and there with elongated nuclei. The walls of the corpuscles were partially dissolved and rendered very transparent, whilst their nuclei were unaffected. The cream-like fluid, which could be squeezed from the surface of the tumour, contained—1st, nucleated cells; 2d, compound granular corpuscles; 3d, numerous granules. The nucleated cells were of a round or oval form, varying in size from the 1-100th to the 1-50th, or even 1-40th of a millimeter in diameter. Some contained one nucleus; others two of an oval form, varying in the longest diameter from 1-100th to 1-75th of a millimeter. Some of these nuclei contained one nucleolus, and others two. On the addition of acetic acid, the cell-wall was rendered more transparent; the nuclei were unaffected, and appeared in consequence more distinct." (p. 3.)

This examination presents, according to Dr. Bennett, a good example of the true cancerous structure; he selects this case, indeed, as the standard of comparison; and the cells described in the extract we have just given, he designates as, *par excellence*, "cancer-cells."

The other microscopic examination is of a different species of tumour of the breast. In this case the tumour was of great size, very hard and painful, the nipple unretracted, and the axillary glands unaffected. The physical appearance of the tumour was that of an amber-coloured gelatinous mass, passing into an opaque, white, firmer fibrous mass.

> "The amber-coloured gelatinous portion was composed principally of a structureless blastema, containing here and there fibres of extreme delicacy, which, with careful management of the light, might be observed to assume the form of fusiform corpuscles. In the midst of the jelly-like blastema, at different depths, were a number of compound granular masses. Some of these, when brought into focus, presented a number of granules, varying in size from the 1-500th to the 1-200th of a millimeter in diameter, which highly refracted light, and were aggregated together without being inclosed in a cell-wall. When out of focus, these masses presented a brownish dark shadow. Where the amber-coloured jelly was passing into the white substance, the fusiform cells became more numerous, and were mingled with a number of oval nucleated cells of great delicacy, varying in size, and especially in length, some being caudate, others spindle-shaped. A thin section of the densest part of the white portion presented a fibrous structure, wholly composed of fusiform cells, which, on the addition of acetic acid, became more transparent, whilst the nuclei were rendered very distinct." (p. 13.)

This tumour seemed to consist of a simple blastema, in which fibrous tissue was forming. It contained no cancer-cells, but apparently only the common elements of exudation.

The fifty-six observations which compose the first part are made up as follows: thirty-three were cancerous tumours, and of these there were six cases of cancer of the breast; four of the stomach; three of the liver and other parts; four of the uterus and other parts; three of the peritoneum and omentum: and the remainder are made up by cancers of the cheek, neck, testicle, diaphragm, lungs, rectum, and other parts. These cancerous tumours were of the nature of scirrhus, encephaloid, or colloid, as Dr. Bennett adopts the generally received division of the genus cancer into these three species. His description of them is not materially different from those given by Müller and Walshe. The relative amount of the fibrous stroma, of the cells contained in its meshes, and of the viscous fluid which surrounds them, is said to determine the nature of the resulting species. "If the fibrous element be in excess, it constitutes scirrhus or hard cancer; if the cells be numerous, encephaloma or soft cancer; and if the fluid abound, and be collected into loculi or little cysts, it is called colloid cancer." (p. 172.) We suspect, however, that the differences between these species are placed much deeper than this paragraph would lead us to infer; it is not merely from an accidental difference in the quantity of the several constituents, but probably from an absolute difference in their mode of formation, and in the diathesis which originates them, that the several kinds of cancer originate.

The remaining twenty-three observations were made on tumours which were not cancerous, although some of them had been mistaken for cancer, and others were distinguished with difficulty even by Dr. Bennett. These tumours were of various nature, fibrous, fibro-plastic, epidermic and epithelial, cystic and enchondromatous. One tumour (Obs. 32) attached to the tentorium cerebelli, although supposed to be cancerous by those who first saw it, was found to be built up of tubercle-corpuscles.

All the tumours of this class Dr. Bennett proposes to call "cancroid," that is, not cancerous, but resembling cancer. "By a cancroid growth," he says, "I understand a structure, which, to the naked eye, the feel, and often in the progress of the case, so closely resembles a cancerous one, that it is commonly mistaken for it, and yet which presents, on examination, structural differences of a very marked character." (p. 175.) Now although we understand the reason which led Dr. Bennett to employ this term, and although we admit that in some points it is a convenient one, we are by no means satisfied of the propriety of using it to designate such growths as fibrous, enchondromatous, neuromatous, fatty, sarcomatous, or even tuberculous formations. These growths have no close relation among themselves, but are brought together by the negative character, that they are not cancerous. But the majority of them cannot even be confounded with cancer, or are so only temporarily, and by those who are not possessed of the necessary diagnostic powers. The tubercular tumour of the tentorium was called cancerous by those who saw only its appearance with the naked eye; examined microscopically, it was found to be a formation whose proper position in nosology is as far removed from cancer as it can well be. Why, then, should this be called cancroid? Because, before examination, it was called cancerous? This is surely a confusion of terms, and likely to lead to a confusion of things. Again, an enchondromatous tumour is by an ignorant surgeon termed cancer; to Dr. Bennett's microscopic eye, it at once reveals its real nature; it has nothing to do with cancer. Why, then, should the ignorance of the first man be deemed so important that, because he mistook the nature of the growth, it should be approximated to that which he conceived it to be, by this epithet "cancroid," cancer-like? In some cases, indeed, the epithet cancroid may be extremely useful, viz. in cases in which the most accurate observer may be in doubt whether the growth be cancer or not; then by placing it in a provisional and undefined group, we avoid the error of assigning to it a position which must be premature, and may be erroneous. It is in this sense that Lebert* uses this term "cancroid." Leaving the subject of "cancroid" growths for a time, let us examine how far the observations of Dr. Bennett will enable us to determine more satisfactorily than heretofore,—1, how to recognise cancer before and after death; 2, the physiological and pathological history of cancer; 3, its appropriate and most successful treatment.

1. *Recognition of cancerous growths before and after death.*

Dr. Bennett considers that the common subjective and objective symptoms, such as lancinating pains, unequal surface, hardness, elastic feel, ulceration, affection of surrounding parts, constitutional cachexia, and return of the growth after excision, are symptoms which are not peculiar to cancer; being occasionally absent in tumours which are undoubtedly cancerous, and being present sometimes in epidermic, fibrous, or fatty growths of the most innocent nature. Accordingly he considers that an accurate microscopic examination, taken conjointly with such evidence as may be afforded by the symptoms and progress of the case, is absolutely necessary before a correct diagnosis can be given. What, then, are the microscopic characters of cancerous, as distinguished from cancroid growths? This inquiry at once leads to an analysis of a great portion of Dr. Bennett's work; for this is certainly the question which he has most fully debated, although he has alluded to many others of nearly equal importance.

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* Physiologie Pathologique, tome ii, p. 291. "Avouons-le franchement, nous ne les désignons sous le nom de cancroïdes qu'en attendant qu'on ait mieux étudié leur véritable nature, pour les séparer probablement ensuite tout-à-fait du cancer."

Dr. Bennett's opinion may be given in a few words: if a tumour presents, among other elements, certain cells of definite aspect, and is at the same time situated in parts which render it impossible that these cells can be epithelial, epidermic, or cartilage-cells, that tumour must be cancerous.

The cancer-cells have been already described in one of the observations we have quoted. We may shortly condense the account which Dr. Bennett gives of their characters in the second part of the work. The cancer-cells are of variable shape, round, oval, caudate, heart or spindle shaped, &c.; of variable size, from the 1-100th to the 1-10th of a millimeter; nucleated, singly, doubly, trebly, or more; generally nucleolated; colourless, or with melanic deposit; increasing commonly on an endogenous plan, and thus often presenting the phenomena of parent-cells, containing two or more generations of younger cells;[*] occasionally increasing by division of the nucleus, but never by division of the cell-wall, or by a splitting-up into segments of the mother-cell. The cell-walls are dissolved by acetic acid, the nuclei corrugated, and their margins thickened. Cancer-cells never pass into fibres, they become caudate, or throw out pointed prolongations, but the transformation proceeds no further. They are contained in great abundance in the white, milky-looking juice which can be squeezed from most cancerous tumours, and they are surrounded by a greater or less quantity of a viscous fluid. Cancer-cells cannot be distinguished from young, plastic, epithelial cells, embryonal and cartilage-cells.

In this description Dr. Bennett agrees very closely with the account of the same structure given by Lebert, Walshe, and Hannover. The chief differences are—1. That Lebert does not seem disposed to admit that cancer-cells can be confounded with any other species of cell.[†] 2. That Walshe states that acetic acid does not dissolve the cancer-cell wall, but merely renders it transparent, and that it can be restored by using an ioduretted solution of iodide of potassium. 3. That Dr. Bennett seems inclined to doubt the exogenous mode of formation of cancer-cells, although the comparatively few cells of scirrhus are said by Dr. Walshe to increase mainly in this way.

Disregarding, however, these differences, and assuming that the characters of the cancer-cell given above are correctly detailed, can we by means of these microscopic characters distinguish cancerous from other tumours?

1. We have already mentioned that some normal cells, as of young epithelium and cartilage, are so like cancer that Dr. Bennett says they cannot always be separated. Thus, in the eighth observation he reports a case of cancer of the cheek: a milky fluid containing cancer-cells could be pressed from the tumour; cancer-cells were found in the loculi formed by the hypertrophied, curled, elastic filaments of the dermis, and in the

[*] The appearance of "mother cells" seems to be occasionally simulated by an imprisonment of nuclei and cells in a finely granular blastema, broken up into various sized masses. Clefts and divisions may be seen between these masses, and this circumstance appears, according to Dr. Bennett, to have suggested to Küss his theory of fissiparous multiplication of the cancer-cell. (See pp. 42-3.)

[†] Physiologie Pathologique, &c., tome ii, p. 255.

meshes of a fibrous matrix situated beneath the skin. In this case, Dr. Bennett says,—

> "The epidermic scales offered a striking resemblance to some forms of cancer-cell. I shall afterwards point out how epidermic and epithelial cells, after being steeped in water, or a thin serous fluid, may exactly resemble, in every essential particular, cancer-cells."

And again, in the 49th observation, an enchondromatous tumour of the ischium and pubis was considered by all who saw it to be cancerous. It had partially softened; the softened pulpy matter contained cells, round, angular, or caudate, with nuclei and nucleoli. These cells looked like cancer-cells, but differed in the fact of acetic acid rendering the cell-wall more opaque instead of more transparent.

If, however, a tumour is found at a distance from skin, mucous membrane, or cartilage, and yet contains "cancer-cells," then Dr. Bennett seems to consider their presence as absolutely diagnostic. 2. Admitting this, it seems unfortunately certain, that a tumour may be truly cancerous, that is, may be connected with a peculiar constitutional taint, may be intractable, and disposed to run through a certain series of changes, and yet no distinctive cancer-cell can be detected in its structure. Without entering into the numerous discussions on this point, we may extract Dr. Walshe's statement, which is of course of the highest authority. At page 124 of the article on adventitious products, Dr. Walshe writes thus,—

> "We have known growths which had destroyed life with the cachexia of cancerous disease, and clearly exhibited the local progress and naked-eye characteristics of encephaloid; growths which, nevertheless, were composed *of non-nucleated cells, undistinguishable from those of common exudation-matter.*"

And in an after part of the same article (p. 137), the following series of propositions are given:

> "1. Parent-cells, containing within them sub-cells having darker nuclei, and these in turn bright nucleoli, are strongly characteristic of cancer; but such cells are rare in, and may be altogether absent from, scirrhus; encephaloid in some phases of its growth may also be without them. 2. The shapelessly-caudate cell seems significant of cancer, but it may be absent from encephaloid, and is excessively rare in scirrhus or colloid. 3. A tumour may present to the naked eye the characters of encephaloid, be the seat of interstitial hemorrhage, affect the communicating lymphatic glands, *run in all respects the course of cancer*, and nevertheless contain no cells, but such as are undistinguishable in the present state of knowledge from exudation-cells. 4. Nay, more, while a primary malignant tumour contains these cells alone, the lymphatic glands, secondarily affected, may contain compound nucleated cells, spherical and shapelessly-caudate. 5. The true fusiform cell is an adventitious formation when it occurs in cancer, and has no diagnostic signification."

In some of these statements we presume Dr. Bennett would not agree; at least, in his summary of the general anatomy of scirrhus (p. 172) he not only does not state that "cancer-cells" may be absent, but assigns their presence as an absolute diagnostic mark between scirrhous and fibrous tumours. Lebert, as might be expected from the decided opinion he has formed of the ease with which the proper element of each morbid growth can be distinguished, does not admit that the true cancer-cell can be ever absent. "The principal element of cancer," he writes, "its proper cell, is found in all truly cancerous formations." (Op. cit., tom. ii, p. 286.)

Hannover* describes the true cancer-cell as invariably present in scirrhus and encephaloma; but in the few instances in which he has examined colloid cancer, it could not be found. Bennett, on the other hand, found, in three of his four cases of colloid, the cancer-cells abundantly present; in the fourth case there were merely isolated nuclei, scattered through a finely molecular blastema. In what he calls "colloid tissue," the interstitial matter is structureless.

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* British and Foreign Medical Review, Oct. 1847, p. 374.

It must be admitted, however, that, with the evidence on the other side, we must entertain grave doubts whether the question of the invariable presence of the so-called cancer-cells in true cancerous growths can be yet decided.

If the true cancer-cell be not an universal diagnostic mark, none of the other microscopic elements of cancer can for a moment be considered as being peculiar to it. These elements are—1. A fibrous matrix, which in most cases is, like the cancer-cells, a new formation, and not merely an alteration of previously existing areolar or fibrous tissue; it seems to be produced by cell- or nucleus-development, and in colloid especially or solely by filamentous precipitation or coagulation of an albuminous principle, originally dissolved in a fluid blastema (Bennett, p. 124). 2. Fibro-plastic and fusiform cells gradually transforming into fibres, which are not peculiar to cancer, but on the contrary, when occurring by themselves, constitute a particular species of tumour, which Lebert, under the name of fibro-plastic, separates from simple fibrous, as well as from carcinomatous growths. 3. Compound granular cells of the usual kind, produced doubtless both by immediate formation from a blastema, and from degeneration of cancer-cells. 4. Fat, which exists in such quantities in most specimens of cancer, that Walshe considers it "almost essential," and which occasionally gives to a cancerous growth an external resemblance to tubercle. 5. Molecules and granules. 6. Naked nuclei, originating either in a primary formation, or set free by the liquefaction and disintegration of the cell-walls. 7. Peculiar diaphanous bodies, which are seen also in the fluid squeezed from the lungs in catarrh, and which are probably nuclei enlarged by imbibition of fluid. 8. A viscous, probably albuminous, fluid. 9. Pus and pyoid corpuscles. 10. Crystals of cholesterine, margarine, and triple phosphate. 11. Other accidental and contingent elements may occasionally be found in cancerous growths, as melanic matter, blood, osteophytes, epiphyta, and epizoa, &c.

From none of these elements singly, nor from any combination of them or union with other structures, as with blood-vessels, can we hope to derive any unfailing microscopic character of cancer. They all occur in other tumours which are decidedly not of a cancerous nature, and their arrangement in cancerous tumours presents no constant peculiarity.

Although we are thus of opinion that there is no one constant microscopic character which will serve as an infallible mark of cancer *in all cases*, we are disposed entirely to agree with Dr. Bennett in attributing the highest diagnostic value to "cancer-cells" when they are present, and are free from ambiguity. They will be present also in the vast majority of true cancerous growths; still we cannot admit that their absence negatives the idea of cancer. We agree also entirely in the judicious observations made by Dr. Bennett, relative to the use of the microscope as a means of diagnosis during life, before the tumour has been meddled with, and when doubt exists as to its real nature. M. Küss, of Strasburg, has proposed to extract a portion of a doubtful tumour with an exploring needle, and to examine microscopically the portion thus cut off. In the case of open sores, a microscopic investigation of the discharge will often clear up all doubts as to the cancerous nature of the disease, by detecting at once the characteristic cancer-cells.

Admitting that the microscopic examination of a tumour is of great value in the majority of cases, but may yet in some instances be insufficient to decide the question of its nature, are there any other circumstances which, apart from the more ordinary symptoms of cancer, may serve as diagnostic marks?

Dr. Walshe attributes considerable importance to the fact of a cancerous tumour yielding a milky albuminous juice on pressure; he says that in ultimate analysis this is the single character least likely to deceive us. Occasionally, however, no fluid can be expressed; in Dr. Bennett's thirty-fifth observation, a cancerous tumour of the mouth gave no fluid on pressure, although numerous true-cancer-cells could be obtained by scraping. And in some cases, enchondroma gives by pressure a milky juice.

Another character by which it has been attempted to distinguish cancer from all other growths, is its property of infiltrating the tissues among which it lies. It extends its meshes in all directions, and in some cases, as in the liver, produces a transformation of the adjoining tissue into carcinomatous matter. Clinically, this character, as Dr. Walshe remarks, is of no value, as cancer is not necessarily infiltrated, but is often abruptly circumscribed, or even, in very rare cases, encysted; and even if infiltrated, this cannot always be recognised during life: but nosologically, this is a characteristic of great significance. Dr. Bennett follows Dr. Walshe in attributing great importance to this peculiarity. Yet a doubt may still be raised whether this infiltrating property is limited to cancerous growths; whether it may not belong to fibrous tumours. Dr. Walshe, in alluding to the reported cases of infiltrating fibrous tumours, intimates that infiltration with common exudation-matter, has been mistaken for fibrous infiltration. (Cyclop., p. 124.) We have, however, knowledge of one case, in which a tumour, whose fibrous nature was accurately determined, had infiltrated like cancer into the adjoining parts.

It has been asserted, also, that it is a peculiarity of cancerous growths to return after extirpation; and there is little doubt that there is a far greater liability to return, than in the case of growths of simple nature. But Dr. Bennett's observations give increased support to the opinion which has been for some years gaining ground, that fibrous, epithelial, and even fatty and enchondromatous growths, return after excision much more frequently than is generally believed.

Thus in the observation (7) which we have quoted above, the tumour of the breast was of a simple fibrous nature. In the appendix we find, however, that a tumour appeared six months after the operation in another situation, and that another tumour formed soon afterwards in a third locality. The patient ultimately died from this disease. A portion of the second tumour cut off after death did not disclose anything beyond a structureless blastema, consisting of fine molecular matter and compound granular masses. In the 48th observation, an arm was amputated at the shoulder-joint for enchondroma. Six months afterwards, a tumour formed in the site of the wound, and is described in the letter of the surgeon, who reported the recurrence to Dr. Bennett, as "evidently of malignant nature." The patient ultimately died from constitutional irritation. Again, in the 34th observation, a simple fibrous tumour was removed from the thigh, which had formed in the site of an old wound, caused by the extirpation of an apparently similar tumour seven years before. In the 38th observation a sailor had a tumour excised by Sir Astley Cooper. Two years afterwards, a nodule formed in the same situation; this gradually increased for twenty-one years, when it was cut out by Mr. Syme; on examination, its nature was not very certainly made out, but it contained no cancer-cells, and from its general characters Dr. Bennett considers it to have been fibrous. Again, in the 56th observation, we have a description of epithelial cancroid tumour of the tongue; the account is very minute, and Dr. Bennett especially distinguishes this formation, "though long called cancerous by surgeons," from true cancer. We are told that Dr. R——— was the sufferer in this case, and if this is intended to refer to a well-known and most eminent physiologist, it is unfortunately but too certain that the morbid process has recommenced. It is, indeed, most important to be aware of the fact that, however innocent we may theoretically suppose these epithelial growths to be, they do yet contaminate adjoining parts, produce a peculiar cachexia, lead to malignant ulcerations, return after extirpation, and kill like growths of a nature considered to be truly cancerous.

The return of tumours of this kind is exceedingly curious, and well worthy of more attentive study. Dr. Walshe explains these cases very simply, by supposing that the removal of the growth has not been perfectly accomplished, and that the particles remaining behind, freed from pressure by the operation, at once recommence to reform the tumour, by acting as attractive forces for new blastema convertible into cells. "This mode of reproduction, as it is erroneously termed," he says, "occurs with growths of all kinds, cancerous, sarcomatous, fibrous, fatty, enchondromatous, erectile, &c."* If this explanation cannot be admitted in any particular case, then we must look to the general cachectic state of the system, in which a depraved blood deposits the necessary blastema, and to some peculiar state of the vessels of the parts which prompts its return here rather than elsewhere. Dr. Bennett seems to agree with Dr. Walshe in attributing the local return of cancer to an imperfect removal of the tumour.

> "A few cancer-cells may easily infiltrate themselves among textures without being appreciable to naked sight, and there is every reason to suppose that a neglect of this rule has led to its return. The same caution as to perfect eradication is also requisite with some cancroid growths, especially the epithelial and fibrous, the occasional return of which is a circumstance that more than anything else has led to their being confounded with cancer." (p. 246.)

We confess the first explanation does not altogether satisfy us; why, if some portion of a fibrous growth be left behind, should it not commence to enlarge at once, instead of remaining apparently tranquil for generally five or six months, and sometimes for some years? Again, why, in the case of a tumour presumably fibrous, as in Dr. Bennett's 7th observation, should there be a "distant reproduction;" an appearance of tumours in some other parts, in which previously there has been no "germinal centre" from which the adventitious growth could spring? This distant reproduction is frequently seen in the case of cancer; it is occasionally seen in the case of fibrous tumours. Is there any difference between the cause of the return in these two cases? However, if both cancerous and some "cancroid" tumours return in the part from which they have been excised, because some portion of their substance has been left, there can of course be no distinction drawn between them, from the mere fact that one returns more frequently than the other.

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* Op. cit., p. 121.

Another general opinion is, that the contamination of adjoining or connected structures, as for example, of lymphatic glands, is a sequence solely of carcinomatous growths, and that its occurrence is a proof, if such be wanted, of the cancerous nature of any doubtful tumour. There is no doubt, however, that epithelial growths have a tendency to affect the neighbouring glands. For example, in the 55th observation is an account of a dissection of a "chimney-sweep's cancer," that is, of an epidermic growth on the scrotum; in this case, the inguinal glands were enlarged, and some of them were removed. In the 56th observation the submaxillary glands enlarged after the ulceration of an epithelial growth on the side of the tongue; on examination, the glands were found filled with a cheesy matter, composed of masses of epithelial particles, mixed with granules and crystals of cholesterine. Dr. Bennett remarks, "that the result of this examination can leave no doubt that the lymphatic glands are subject to the same epithelial form of morbid growth, as affects the tongue, face, or other similar structures. I think it due, however, to Mr. Paget to state, that he had previously ascertained this fact." (p. 259.) A doubt may perhaps be raised as to whether the term epithelial should be applied to cells occurring in situations where normal epidermis or epithelium is unknown.

It thus appears that there is no microscopic character absolutely indicative of cancer, although the occurrence of diagnostic cell-forms is extremely frequent; that a cancerous tumour cannot either be distinguished in every case by the expression of milky juice, by a tendency to return after excision, to contaminate adjoining parts, or by possessing alone the power of infiltration. There are then, in fact, no physical characters which are absolutely essential.

Are there, then, any chemical characters; any distinctions in ultimate composition between these growths and others which are not cancerous? Dr. Bennett does not add much to our chemical knowledge. He thinks albumen constitutes the basis of the fibrous element found in morbid structures. He seems inclined to adopt, indeed, the hypothesis of Zimmerman, that fibrin is a product of the metamorphosis of tissue, and consequently an excretion, rather than the plastic organizable constituent of the blood, as it has generally been considered to be. We have not time to enter into this question; it appears to us that Zimmerman's arguments are of not much weight, if we admit, what indeed has been generally conceded, that organization may in some cases occur from an albuminous blastema, and that fibrin may be formed from albumen.

Dr. Bennett sums up our present knowledge of the chemistry of the heterologous formations in the statement, that not only are there no means of separating chemically the different forms of cancerous tumour from each other, but that it is also impossible to distinguish these from other morbid products, or even from healthy tissues.

Although it appears thus difficult to define "cancer"—to draw a broad line of demarcation between it and other growths,—it is yet probable that the group of cancerous diseases is a peculiar and characteristic group; and it is certain that, in its developed and most defined form, a cancerous tumour is one possessing properties, which do not permit us for one moment to confound it with any other morbid product. But to illustrate this, let us pass to the second heading of our subject, and inquire what additions Dr. Bennett has made to

2. *The physiological and pathological history of cancer.*

In Dr. Walshe's great work on Cancer,* the origin of cancer is laid down with much precision and clearness. The first step in a cancerous formation, is an exudation of a peculiar blastema, which, in our present knowledge, can be distinguished from common nutritive blastema only by the difference in the resulting organized forms. Such a blastema, as a general rule, exudes completely from the vessels, seldom is arrested in the thickness of their walls, and very rarely indeed, if ever, remains within the vessels themselves. This cancerous blastema, thus derived from the blood, necessarily implies that the blood itself is in an abnormal state, when it can furnish so unhealthy a substitute for the ordinary secretive and nutritive blastema. And this inference is corroborated by almost all the facts we can bring to bear on this point, and which all tend to the conclusion, to use Dr. Walshe's words, "that a cancerous tumour, under all circumstances, is but the local evidence of general vitiation of the system."† Subsequently, the cancerous deposit may remain quiescent, the cancerous cachexia which produced it having passed away; or may advance with greater or less rapidity, according to the intensity of this cancerous diathesis. If it proceed, it is destructive or malignant, partly in proportion to the degree of this preceding constitutional disease, and partly in proportion to the infiltrating power possessed by the cancer-tumour in the particular individual. Finally, the local disease itself reacts on the constitution, and impresses a deeper and more advanced influence on the general system.

These views are identical with those held by Dr. Bennett:

> "The great majority of facts with which we are acquainted," he writes, "lead to the conclusion that the filaments, cells, and fluid, which together make up the tissue I have called cancerous, originate in a coagulated exudation. This is poured out exactly in the same manner as all other forms of exudation, viz. by enlargement of the capillaries, their repletion with blood, and the transudation through their coats of the transparent liquor sanguinis, which coagulating outside the vessel constitutes an exudation, more or less solid." (p. 202.)

The following quotation expresses his views very clearly, and proves their exact identity with those held by previous writers:

> "The exudation thus poured out, which I have called cancerous exudation, must differ from the exuded matter in what is called inflammation or tubercle; but in what that difference consists we are ignorant. The characters given to the blood by Rokitanski and Engel, to distinguish what they have called the cancerous dyscrasia, are so vague and uncertain, as to have no real value. Neither is there any proof that such dyscrasia consists in an excess of albumen (Engel) or of fibrine (Keller) in the blood. The latter takes place after all exudations. But although the present state of pathology does not warrant our stating positively wherein exudations differ, we cannot avoid seeing that their occurrence in various indi-

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* Op. cit., p. 50. † Ibid., p. 188.

viduals produces very different results. Thus, if an exudation be poured out in a healthy individual, it produces a series of phenomena which we call inflammatory; if in a scrofulous individual, another series of changes called scrofulous or tubercular; whilst in a third person the result may be a cancerous growth. Every kind of reasoning must lead us to the conclusion, that these different changes and effects depend not upon the vascular system, which is the mere apparatus for the production of exudation,—not upon the nervous system, which leads to the necessary arrangement of that apparatus for the purpose, and not on the texture, which is the seat of the exudation, as that varies, whilst the cancerous formation is the same,—but in the inherent composition or constitution of the exudation itself. But here pathologists pause; once traced back to the blood they are content, and they have not sufficiently taken into consideration that the blood itself is dependent for its constitution on the results of the primary digestion in the alimentary canal, on the one hand, and the secondary digestion in the tissues on the other." (p. 204.)

Pursuing these views still farther, Dr. Bennett remarks, that a comparison between the characteristics of the cancerous, and what may be termed the simple or inflammatory, and the tubercular exudations, may lead us to some useful hints respecting the significance of each.

Let us suppose that an individual in perfect health has from any cause (injury, irritation leading to inflammation, &c.) an exudation poured out into any part, which is plus the normal exudation required for the repair of the tissues; the further changes which occur in this vary infinitely according to the part, the degree of irritation, the rapidity or slowness with which the blastema is poured forth, &c. But one or other of what may be termed the healthy or normal changes occur; the exudation becomes the medium of adhesions, or is organized into false membrane, or accumulates in the interstices of organs, assuming a greater or less resemblance to them, and impeding more or less their functions, or it breaks down more or less rapidly into evanescent cell-forms, which are discharged at once from the free surfaces, or are eliminated from the body through the medium of further changes and different emunctories. The composition of this blastema no doubt varies infinitely within the healthy limits; and these variations are evidently not dependent, for the most part at any rate, on any local condition of the part in which the blastema is exuded, but are attributable to the general condition of the organism, to the purity and excellence of its blood, which is the indication of the due performance of all the varied metamorphoses of the frame.

But if, instead of this healthy constitution, the individual be tuberculous or cancerous, then in certain organs the exudation emitted from any cause does not pass through this series of normal changes, but assumes one or other of the forms peculiar to the diathesis. Thus, in young subjects, and in persons of peculiar and defective assimilating power, the exudation is below the healthy standard. It attains imperfectly to any cell-forms, and is liable to rapid disintegration and complete loss of vitality. This is the tubercular exudation, and although it is poured out in its characteristic form only on certain organs and positions, yet we see enough to convince ourselves that a blastema poured out in other situations, as in accidental breaches of continuity, although it may not possess the tuberculous character, is yet often evidently different in some way from the reparative blastema, which would be effused in a perfectly healthy condition of the system. In an older person, in whom the assimilating functions are also deranged, although in a different manner, the blastema poured out may be entirely dissimilar to the tuberculous blastema. It may have a tendency to the production of very highly-developed and reproductive cell-forms, which grow with great energy and rapidity, and retrograde and disintegrate slowly and imperfectly. Between this diathesis and the former there is therefore almost an antagonism; they can hardly exist in the same person, and the presence of the one may be said almost necessarily to exclude the other. And here the hypothesis we have given above, for at present it is no more, agrees with the facts of the case; for it is well known that tubercle and cancer occur at different periods of life, and affect different organs, while when recent they are rarely found associated.*

Dr. Bennett surmises that tubercle may be connected with some derangement in the function of the primary, and cancer with some derangement in the function of the secondary digestion; and he points out that cancer commences to occur at that epoch of life, when the excretory functions begin to lose their activity.

We have already said, that though it may not be easy to define the term cancer with perfect accuracy, yet that there is no doubt that it is capable of definition. It must be so, if it be true that there is thus a special constitutional state, of which the local disease is the effect and index. The definition cannot perhaps be immediately given; but we feel certain that the continued labours of such men as Walshe and Bennett will very soon clear away all doubts which may exist respecting the essential physical characteristics of a cancerous tumour. The recognition of the "true cancer-cell," as distinctive when it is present, is a great point gained; and the next step is to inquire into the reason of its absence in certain cases, and into the nature of the structure which then takes its place. We have then to learn what it is which enables epithelial and even enchondromatous growths occasionally to produce cachexia, similar to the cancerous cachexia, without possessing the anatomical characters of cancer.

With regard to the growth of cancer, Dr. Bennett does not communicate anything new. He alludes to the increase of fibrin in the blood, referring it, like Zimmerman, rather to absorption of effete matter, than to an excess of plastic material.

The degeneration of cancer is a point of great interest; there seems little doubt that one of Müller's varieties was merely common cancer in process of decay; and it appears that even Lebert has, in some instances, mistaken this degraded or reticular cancer for tubercle, which it closely resembles, both microscopically and to the naked eye.

Decay of cancer occurs in two chief ways: first, by the formation of fatty granular matter between the nucleus and cell-wall; the nucleus becomes indistinct, and then breaks up; the cell now assumes the characters of a compound granular corpuscle, or it may break up altogether into fatty masses, which are mingled with old withered cells and naked nuclei. At last nothing but granules and corpuscles can be detected, intermingled with the original fibrous element of the tumour. Another kind of decay is by calcareous transformation; crystals of earthy salts are sometimes found mixed up with cancer-cells, and have been mistaken for bone; but true bone, as pointed out by Dr. Walshe, never occurs independently of the presence of natural osseous tissue, from which, however, osteophytes, piercing into cancerous tumours, often spring. In addition to these two forms of decay, Dr. Bennett describes a third, in which the cells break down because they have arrived at the natural term of their existence, and are not supplied with the necessary blastema to enable them to originate another generation. But we do not see that he has brought forward any evidence to show that this would not occur in the usual way, viz. by development of granules between the cell-wall and nucleus, leading to the destruction of both.

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* This fact was first clearly established by Dr. Walshe (On Cancer, p. 185.) He says, tubercle and cancer "possess a sort of repulsion in respect of each other."

While, however, there is no doubt that cancer often undergoes this degeneration, in part or wholly, it is certainly uncommon for a cure to happen in this way; a temporary suspension of symptoms may occur, but generally decay at one point is more than compensated by rapid increase at another. Nevertheless, occasionally a cancerous tumour will undergo unequivocal cure.

Before passing on to Dr. Bennett's chapter on treatment, we must briefly allude to his few remarks on statistics. He denies that, at present, we have a sufficient number of cases to enable us to make a numerical analysis, although "he anticipates the most important results by applying the principle of statistics to cancer." He asserts, however, that at present we have not more than 526 observations on tumours of all kinds, which can be depended upon; and he rejects as quite worthless the 9118 cases collected by Tanchou from the Parisian registers. We are disposed to agree with Dr. Bennett's anxiety to have his items as correct as possible, although we think he has underrated the accuracy of Tanchou's tables. Individual experience is daily confirming the main facts deducible from them.

3. *What is the best treatment of cancer?*

We have no intention of doing more than giving our readers Dr. Bennett's views on this point. They are, like many other parts of his book, original, ingenious, but rather doubtful. He divides our remedial measures into three sections: (*a*) means of retardation and resolution; (*b*) means of extirpation; (*c*) means of prevention.

(*a*) The means of retardation are those which are considered hostile to the growth of organized matter. Cold and pressure seem the two chief agents. Pressure is best applied by Dr. Neil Arnott's ingenious apparatus, which is figured by Dr. Walshe. Dr. Bennett also thinks that if the supply of nourishing blastema could be cut off in any way, the growth would necessarily decay. These agents of course attack only the local disease; the general constitutional state is left untouched by them.

(*b*) The means of extirpation are by operation and chemical erodents.

Dr. Bennett alludes, under the head of excision, to the celebrated discussion before the French Academy in 1844, in which Cruveilhier alleged that fibrous tumours of the mamma were frequently and erroneously cut out for cancer. Dr. Bennett remarks that, at the commencement, no diagnosis can be made between a fibrous and a cancerous tumour, and that, moreover, into a true fibrous tumour cancerous blastema may at any time be poured; he seems inclined then to authorise ablation, whenever the tumour shows unequivocal signs of increase. He believes, indeed, that the results of operation are much more favorable than are generally supposed. On the other hand, Dr. Walshe's inquiries, as is well known, give a very unfavorable picture of the result of ablation.

Dr. Bennett's opinion, to be of any value, must be founded upon his experience of the operation. On examining therefore into his cases, we find the following results: of his 33 cases of cancer, we find that operations were performed in 14. These cases are the following:

1. Observation I. This is headed "cancerous tumour of the breast—excision—cure." In this case we find a patient admitted in February 1847, under Mr. Syme, who cut out a cancer of the mamma which had commenced in the previous December. On the 18th of March the wound had healed, and the patient was dismissed in good health. The case is therefore headed "cure," but of course this is quite gratuitous; almost all operations do well for some time; it is idle to call anything a cure until the patient has passed years without a return. In the appendix accordingly we find that the tumour did return in November 1847, and in November 1848, it was of the size of a large fœtal head, it had ulcerated, and was attended with extreme constitutional cachexia. This case then proves nothing for the operation, or rather speaks against it.

2. Observation II. In May 1847, a tumour, supposed to be fibrous, but really cancerous, was cut out from the neck. When the appendix was written, we presume at the close of 1848, it had not returned. Eighteen months, however, can hardly be considered a sufficient time, as cancerous growths often return two or three or even more years after extirpation.

3. Observation IV. Cancer of breast, cut out August 16th (we presume 1848, but it is not so stated) by Mr. Syme. When the appendix was written, the patient had not been heard of. But here the time is too short, whether the date be 1847 or 1848, even if the patient had been heard of.

4. Observation V. Cancer of breast, removed by Mr. Fergusson on 13th April, 1847. Up to October there had been no return. When the appendix was written, Mr. Fergusson had not seen the patient again. Here, again, the case is worthless, as the time is much too short to enable us to say that the operation has not been followed by return of disease.

5. Observation VI. Cancer of breast—excised—returned—re-excised—re-returned.

This case is no argument for the operation. The second tumour appeared two years after the removal of the first.

6. Observation VIII. Cancer of cheek—excision—cure. Operation May 7th (1847 or 1848?), wound healed May 25. No return in November 1848. The period of probation here is only 6 or 18 months according to the date of the operation, which is not given.

7. Observation X. Cancer of testicle—excision—cure. Operation Sept. 1842. In 1848 no return. This was a case of cancerous growth from local injury.

8. Observation XI. Cancer of testicle—excision Sept. 1847. No return at the end of 1848.

9. Observation XXXV. Cancer of mouth—excision some years ago—return soon afterwards—re-excision June 1848. No return when the appendix was written.

10. Observation XL. Melanic cancer of cheek—excised Dec. 1845. No return in 1848.

11. Observation XLIV. Cancerous tumour on the finger and toe—amputation May 1848. No return at the end of the year.

12. Observation LII. Colloid cancer of mamma—excised March 15th, 1847. No return when the appendix was written.

13. Observation LIII. Colloid cancer of mamma—excised Sept. 1847. No return up to June 1848.

14. Observation LIV. Encysted, sanguineous, colloid tumour of the back. No cancer cells to be found—excised June 1848. Patient not afterwards heard of.

Now, out of the whole of these cases, there is only one in which an unequivocal cure can be said to have occurred, and this is one of the cases of carcinomatous testicle, which was excised in 1842. And even this case may be considered to have been an unusually favorable one for the operation, as the tumour resulted from, or followed, a local injury; this being precisely the form which all systematic writers have considered as most likely to be benefited by operation. We cannot conceive how from these cases Dr. Bennett can draw any valid argument for the operation, much less can we conceive what right he has to assume so confident and encouraging a tone, in opposition to most of the writers who have investigated this subject on a large scale. His cases would not even enable us to affirm that life had been prolonged by the operation, so few are they, and so recently have the majority of them been operated upon.

We do not find in Dr. Bennett's other arguments anything which can compensate for this want of direct evidence in favour of the operation. We are disposed, however, to agree with Dr. Bennett, that repeated extirpation might possibly be of service in warding off the terrible constitutional cachexia, which ulcerated cancer often so rapidly induces. This is another question from that of the curative power of operation, and at present we have absolutely no data to lead to any conclusion, as the plan has not, as far as we know, been carried out to any extent.

With regard to the destruction of a cancerous tumour by chemical agents, Dr. Bennett speaks very doubtfully, on account of the impossibility of applying such remedies to the whole extent of the morbid growth.

The means of prevention recommended by Dr. Bennett are rather hypothetical. After stating that all sorts of alteratives and tonics, mercury, iodine, arsenic, gold, copper, chloride of barium, have been ineffectually used as prophylactic agents, he proceeds to draw a possible indication from the observed antagonism of tubercle and cancer. In tubercle, he says, there is a deficiency of fat; the animal oils are indicated to increase the nutritive power. On the other hand, in cancer, fat-elements abound; spareness of frame is here the thing to be sought for, and "the circumstances which diminish obesity, would seem," he says, "*à priori*, to be opposed to the cancerous tendency." We shall only say in reference to such a suggestion, that it is based on the most speculative and hypothetical ground. Moreover, it has absolutely been fully tried and found wanting. Persons have been half starved, but their cancers have unfortunately grown with as much vigour as ever.

We have occupied so much space with the consideration of that portion of Dr. Bennett's book which refers to cancerous growths, that we must pass very lightly over the "cancroid tumours." Here also is much well worthy of more space than we can accord to it.

Dr. Bennett describes a new growth, the "fibro-nucleated cancroid growth." It consists of filaments infiltrated with oval nucleolated nuclei, but destitute of cells; sometimes the growth, to the naked eye, exactly resembles scirrhus, at other times encephaloma. The peculiarity of this humour is that free nuclei exist before any softening or liquefaction of cells could have occurred to set them free, consequently they must be considered to be an original formation, and what is singular, they do not at any time become surrounded with cell-walls, at least Dr. Bennett's observations go to show that cells are never found in this variety of growth.

The different kinds of epithelial growths are well described; the following gives a good description of a form of disease of which we believe we have ourselves seen a characteristic example, which commenced as an induration at the base of the tongue, was attended by glandular enlargement, and was followed by corroding ulceration before which a great part of the lower jaw disappeared, a cancerous hue of the surface, and the general symptoms of true cancer.

> "Another form of epithelial cancroid is one which frequently commences almost at the first as an ulcer, although sometimes it is preceded by slight induration of, or a small wart on, the part affected. It is common in the under lip, in the tongue, and in the cervix uteri. In the lip there may often be early observed a furrow or groove in the indurated spot, or wart, in which the ulceration commences. This slowly extends, with indurated, thickened, and raised margins, is circular and cup-shaped, its surface sometimes covered with a white cheesy matter, at others with a thick crust or scab. It slowly extends until it involves a greater or less portion of the lip or neighbouring parts, pouring forth a foul ichorous discharge. In the tongue the disease follows a similar course; the base of the sore, however, is generally more fungoid or papillated on the surface, and exceedingly dense, owing to the close impaction and compression together of laminæ of epithelium. When examined microscopically, these forms of cancroid ulcers present on the surface masses of epithelial cells in all stages of their development, some spherical, nucleated, about the 1-50th of a millimeter in diameter, others much larger, both often resembling cancer-cells when viewed alone, but associated with flattened scales, varying in shape and size, sometimes occurring in groups adhering at their edges, at others mingled together in a confused mass; many of the cells and scales often reach an enormous size, and as they become old split up into fibres. These elements are commonly associated with numerous molecules and granules, naked nuclei, fibro-plastic, fusiform, and pus-cells." (p. 181.)

Fibrous tumours are divided into the sarcomatous, dermoid, chondroid, and neuromatous. A good description is given of each of these. In the subsequent pages, Dr. Bennett alludes to fatty cancroid growths, tumours from effused blood (which Dr. Walshe, in the article on Adventitious Products, has also elaborately described under the title of "Hæmatoma"), and various other growths or morbid products which, by possibility, may resemble cancer. Among these he mentions the "typhous" deposit, as furnishing cells approaching in appearance to true cancer-cells.

In bringing this review to a conclusion we may congratulate the British school of Pathology on the fact, that it has furnished such men as Walshe and Bennett, as worthy fellow-labourers in the same field with Müller, Bruch, Lebert, and Hannover. We do not hesitate indeed to claim for our countrymen even the first rank among these distinguished men, as authorities on Cancer. Dr. Walshe's work is the most elaborate and complete; but if we cannot assign to Dr. Bennett the praise of having brought together such extensive materials, or of having so philosophically arranged them, we must accord to him the credit of being an excellent and indefatigable observer, who has added much to our knowledge of cancer, and has done still more to clear the path for the observers who will succeed him.

ART. XII.

General Board of Health : Report on Quarantine. Presented to both Houses of Parliament by Command of Her Majesty.—*London,* 1849. 8vo, pp. 172.

WE consider the subject of this Report of so great national importance, that, although we have received it very late in the quarter, and in order to comment upon it have been compelled to defer much valuable matter to a future Number, we feel it our duty to point out the numerous errors it contains, before the practical conclusions to which these errors would lead can receive the sanction of the Legislature. We are compelled, however, to dispense for the present with any analysis of its most valuable portion,—that relating to the effects of overcrowding, impure air, and diet, as causes of disease in ships at sea or in harbour,—and restrict our observations closely to the subject of Quarantine. But we may state, before passing on to our task, that the remarks upon the sanitary regulations of ships and their condition in various ports; upon the defects in marine registration; upon the relative mortality of seamen and landsmen from zymotic diseases, and the general mortality at sea compared with that in the metropolis; upon the increase of zymotic disease in unhealthy districts; upon the prevalence of scurvy in merchant ships; upon the condition of emigrant vessels and passenger-ships; above all, upon the practical utility of ventilation, and the simple means of ventilating ships, proposed by Dr. Arnott, and so successfully adopted by Dr. Millar, to prevent the excessive fatality sometimes observed in transport and emigrant ships:—all these we consider most admirable. We cannot express too strongly our perfect concurrence with this portion of the Report, or recommend with sufficient earnestness its immediate study by all medical men, especially by those attached to our navy or mercantile marine.

When we pass on, however, to the strict subject of the Report, we find, with extreme regret, that the portion relating directly to Quarantine is as faulty as that to which we have just directed attention is excellent It is our duty, therefore, to consider closely the portions which are likely to mislead those, who, deriving their sole information on this subject from the Report before them, would receive what they find stated there without that previous information from other sources, which might enable them to judge of its correctness, and prepare them to discover its fallacies. This examination is peculiarly necessary in the present instance. The subject is not one likely to be practically known to a large proportion of our readers, very few of whom would take the trouble of examining the documents from which the Board of Health have made extracts, or of considering how far these extracts represented the general conclusions of the documents in question, or how far they constituted purely *ex parte* evidence, contradicted by more convincing testimony on the opposite side of the controversy, from the same pages. That general readers are not the only class who require to be guarded in their reception of the evidence afforded by this Report, is very clearly shown by the almost indiscriminate laudation with which it has been received by the press, medical and non-medical. As far as we have seen, with but two exceptions, it has been praised by writers who evidently knew nothing more of the subject than they had learned from the book they were reviewing, and who received with the most child-like and unsuspecting confidence all they found stated under the imposing guise of an official "blue-book."

Let us commence by examining the opinions of the reporters with regard to the true object of Quarantine. They state that,—

> "The object of quarantine is to prevent the introduction of epidemic diseases from one country into another, and its regulations are based on the assumption of the contagiousness of the diseases with which it deals; it being supposed that such diseases are propagated by contact, direct or indirect, of the unaffected with the affected. In accordance with this view, the preventive means adopted by quarantine consist of the isolation of the sick or suspected, with whom it interdicts all communication, whether by person or by articles deemed capable of transmitting contagion." (p. 4.)
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> "Blindly intent on accomplishing an impossible object, it overlooks the circumstances on which the existence and extension of disease really depend; and after the experience of centuries has shown the inutility of the securities it offers, it still proposes to go on reposing implicit confidence in them and in them only. Quarantine is based, as has been stated, on the assumption that epidemic diseases depend upon a specific contagion; but the question of contagion has no necessary connexion with that of quarantine. The real question is whether quarantine can prevent the extension of epidemic diseases, whatever may be their nature, whether contagious or not." (pp. 16, 17.)
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> "In like manner the discussion whether epidemic diseases arise and spread from contagion, or from common or specific poisons generated in the localities in which these pestilences first break out, has nothing whatever to do with quarantine." (p. 18.)

We shall see presently, that, by the term "epidemic diseases," the Board do not always imply diseases which can only be acquired by breathing the air of some locality in which such diseases are prevalent. In some places they appear to restrict the term to this signification. In others they include, as they do here, what are usually considered to be contagious epidemics. The principal object of Quarantine is to prevent the importation of such diseases as experience has shown may be communicated from one person to another by close approximation of the affected with the unaffected. And the preventive means adopted do not consist simply of *isolation* of the sick from the healthy and their *congregation* together, as stated at page 61, but of isolation, not only from the healthy, but in a very great measure from each other,—a most important distinction to be borne in mind. We shall presently see, when we come to the practical applications of the doctrines of the Report, that the second paragraph is altogether contradicted by the most positive evidence, and that quarantine can prevent, and has prevented, the importation of disease.

We must now make some remarks upon the accounts of the manner in which quarantine regulations are carried into effect. Here is the statement of the Board:

> "If the great practical truth, taught by modern investigation and experience, be,

that the only real security against any kind and degree of epidemic disease is an abundant and constant supply of pure air, the prevention of overcrowding, and the dispersion of the sick; and if, as is generally agreed, confinement in a foul atmosphere can convert common fever into pestilence, and ventilation and dispersion can dissipate any contagion, then quarantine must be not only useless but pernicious, since the invariable effect of quarantine as hitherto practised in all countries has been the congregation and confinement of the sick and of those who, though not actually sick, are suspected to have in them the seeds of disease, requiring only a few days or hours for their development,—the congregation and confinement of such persons in a limited space, often in a filthy ship and an unhealthy locality, and always under circumstances calculated to excite apprehension and alarm—conditions in the highest degree favorable to the generation and spread of disease: it follows that quarantine, instead of guarding against and preventing disease, fosters and concentrates it, and places it under conditions the most favorable that can be devised for its general extension; and therefore must not only fail to accomplish its object, but tend to produce the very calamity which it endeavours to prevent." (p. 61.)

This is a most important paragraph, containing a singular mixture of established truths with the most inconceivable error and ignorance. The security to be obtained by ventilation is strongly stated; the advantages of dispersing the sick are alluded to without mentioning any of the necessary precautions which should accompany such dispersion; and then we have a description of the practice of quarantine "in all countries," which, after having visited the principal European lazarettos in the Mediterranean, we do not hesitate to state is totally and entirely contrary to the truth. In no lazaretto for many years past have patients been congregated or confined in a limited space. On the contrary, all suspicious cases of sickness which occur on board ship are removed on shore, are placed in separate and well-ventilated rooms, and attendants are placed there with them. This is the invariable custom; and therefore the conclusion that quarantine fosters and concentrates disease is unfounded and untrue. We have been on three separate occasions in charge of smallpox patients in a lazaretto, for periods ranging from sixteen to twenty-one days. In every case the patients were removed from the ships as soon as they arrived, they were sent on shore to separate and well-ventilated rooms, and we, with nurses, were kept with them in quarantine until all danger of propagating the disease had passed off. Now, can any reasonable being doubt that in this way smallpox was prevented from spreading among other men on board the ships, or that if these precautions had not been taken, the seamen, and through them the whole neighbourhood, might have suffered severely for a long period. The ill effects of the neglect of these precautions was most strongly evinced in 1830, when the smallpox was distinctly proved to have spread from the Naval Hospital at Malta to the home of the person who washed the patient's linen, and then over the greater part of the island. It extended itself rapidly, and did not cease until the following year, 10,351 persons having been attacked, of whom 1503 died. It was proved to have been imported from Greece in H.M.S. Asia, there being no smallpox in the island before the arrival of this ship; and there can be little doubt that the subsequent immunity of the island has been secured by the quarantine regulations, which were neglected in this unfortunate instanee with the effect of destroying and disfiguring so many unfortunate persons.* And yet we are nauseated by philanthropical homilies upon the *inhumanity* of quarantine, and the reporters coolly tell us that the system places disease under conditions the most favorable that can be devised for its general extension. We repeat emphatically that in all the European lazarettos we have visited, and this includes nearly all those in the Mediterranean, quarantine regulations are carried out in exactly the opposite manner to that stated in the above paragraph. The sick are not confined in ships, but are immediately removed from the ships to the lazarettos; and these are not in unhealthy localities, but on islands or promontories fully exposed to the sea breezes. The sick are not congregated together, but are invariably separated from each other; and attendants are placed with them, who must undergo the same period of quarantine, and who know that their own safety depends on the cleanliness and thorough ventilation of the apartments and clothing of the sick.

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* All the details of these facts may be found in Dr. Davy's work upon the Ionian Islands and Malta.

Let us now analyse the doctrines inculcated in the Report with regard to contagious, infectious, and epidemic diseases. We are informed that,—

> "Epidemic diseases were formerly universally considered to be essentially different in their nature, each being thought to depend on its own specific contagion; and the correctness of this view seemed to be confirmed by the great apparent difference between typhus, scarlatina, influenza, plague, yellow fever, and cholera; but whether each of these diseases depends on a peculiar and specific cause, or whether they all derive their origin from one common agent, essentially the same in nature, but modified by peculiarities of climate and other circumstances, and which under varying conditions gives rise to various forms or types of disease, each having definite characters and running a particular course,—whichever of these views be adopted, it is agreed by the most eminent investigators that there is a general resemblance between these various forms of disease, and that they have the following characters in common:—They are all fevers; they are all dependent on certain atmospheric conditions; they all obey similar laws of diffusion; they all infest the same sort of localities; they all attack chiefly the same classes, and for the most part, persons of the like ages; and their intensity is increased or diminished by the same sanitary and social conditions." (p. 5.)

Now this is evidently a mode of asserting by inference what the writers dare not assert boldly, as to the identity of these different diseases. If it be not a false statement, it is certainly a series of statements suggestive of false ideas. The most eminent investigators are most certainly *not* agreed that all these diseases are fevers; very few of these most eminent investigators agree that they are dependent on atmospheric conditions; on the contrary, while they fully admit that some of these conditions may have a most extensive influence upon the intensity and propagation of these diseases, just as atmospheric conditions influence the germination of seeds, they no more believe that atmospheric conditions alone could generate plague, cholera, typhus, or smallpox, than that with all favorable conditions of soil and climate a field might become filled with wheat without any seed having been sown. They know that all recognised varieties of atmospheric condition may be present without plague or cholera, and that plague or cholera are developed under the most opposite states of the atmosphere and in the most varied climates. The most eminent investigators are *not* agreed that all these diseases obey similar laws of diffusion; on the contrary, they know that their laws of diffusion are very greatly diversified; that while one form of yellow fever may clearly be transmitted from an affected to an unaffected person, another form as clearly cannot be so transmitted, and can only be received by visiting spots where it arises from the soil, or is present in the atmosphere—that the laws of the diffusion of cholera are so totally different from those of plague, that while all evidence is in favour of the latter being distinctly communicable from one person to another, the evidence with regard to cholera proves that even if it be thus communicated, it is only in rare exceptions to the opposite general rule—that the laws of the diffusion of typhus and scarlatina are totally opposed to those of influenza; the latter being a disease which is seldom or never clearly traced to contagion, while the two former are known by common experience to be frequently so to a most dangerous extent, whenever the emanations from patients suffering with those diseases are not well diluted by free ventilation.

Again, they *do not* infest the same sort of localities. Plague infests Egypt, and is confined to Egypt, except under extraordinary circumstances favouring its transmission and spread in other climates; yellow fever infests the western coast of Africa, the West Indian Islands, and the southern states of the American Union, but seldom spreads to other climates unless distinctly transmitted; cholera is a native of the East Indies, and has only extended itself westward within the memory of the present generation, although the hygienic conditions upon which it is presumed by Mr. Chadwick and his school to depend have existed for ages; typhus, scarlatina, and influenza do not infest the localities where plague, cholera, and yellow fever are prevalent, and surely Egypt, the West Indies, and India can scarcely be called the "same sort of locality," differing as they do in climate, soil, cultivation, geographical position, and the constitution and habits of the natives. If it be asserted that by the term, "same sort of locality," the authors only imply unhealthy situations, then this argument would equally prove the identity of all diseases which are aggravated by causes acting injuriously on the general health.

We must proceed, however, and again refute the Report by the fact that the diseases in question do *not* attack chiefly the same classes. While cholera and typhus are almost confined to the poor, half-starved, and debilitated residents in crowded dirty habitations, influenza and scarlatina attack all classes in a much more equal proportion; at least such is the common belief of the profession, although it would be very difficult, if not impossible, to establish the point by statistics. Yellow fever in its epidemic form attacks all those not protected by race, who expose themselves to its influence, indiscriminately. Plague is decidedly more regulated in its ravages by segregation, race, and the protection of a previous attack, than by the mere predisposing causes of poverty, dirt, and deficient ventilation. Then as to age, scarlatina is a disease of the young, influenza of the old, as a general rule.

As to the increase or diminution of intensity by sanitary and social conditions, no one would doubt for an instant that any person placed under superior hygienic conditions would be less liable to receive disease, and more likely to recover from it, than another exposed to the influences of poverty, filth, crowding, and impure air. But this influence of sanitary condition no more proves community of character between typhus, influenza, or cholera, than between pneumonia, syphilis, and smallpox.

If such a mode of reasoning were allowed, we would undertake to prove the identity of hydrophobia, itch, and an ulcer on the leg.

It is asserted in the sixth page that epidemic disease essentially depends upon the presence of an epidemic atmosphere, a primary and essential condition which quarantine cannot affect. This is assuming what, although *probable*, and generally admitted to be probable, is by no means proved. An "epidemic atmosphere" is a pure assumption. No such thing can be demonstrated, however reasonable it may be to conclude that certain atmospheric conditions must be present to favour a development of morbid poison sufficiently rapid to cause a general extension of pestilence, just as certain conditions of heat, moisture, and air are necessary for the process of fermentation. The very next paragraph would render it very doubtful if any such epidemic atmosphere really has existed in England lately.

> "The experience of the present epidemic season affords evidence that the influence of an epidemic atmosphere may exist over thousands of square miles, and yet affect only particular localities." (p. 6.)

And again at page 53:

> "During the last six months, for example, cholera has made attacks upon nearly one hundred different places in England. When these places are carefully examined, the conditions favorable to their invasion are uniformly found; but in the near neighbourhood are places, with conditions apparently just as favorable, that escape."

Yet if this epidemic atmosphere were really in operation, really existed, and really were the essential condition upon which cholera depended, surely it could scarcely act over thousands of square miles, and leave no trace of its action. What is there to prove that any such atmosphere existed in places which have remained perfectly healthy? We repeat that its existence is assumed without anything approaching to proof.

When we endeavour to determine what the Board mean by the terms contagion and infection, we find contradictory definitions in different parts of their Report. At page 47 we are informed that, "strictly, contagion, as the word implies, is capable of being communicated only by actual contact;" at page 19 we learn that, "taking the word contagion in its strict sense, it is the communicability of disease exclusively by contact; direct, that is, with the body or breath of an infected person; or indirect, with something which an infected person has touched." Thus, according to this very correct definition, a disease which may be communicated by the *breath* of an infected person is a *contagious* disease; while throughout nearly the whole of the Report, the fact of communication by inhalation of the concentrated emanations of sick, crowded, or ill-ventilated spaces, is assumed as a character of *infectious* diseases distinct from those which are contagious. Are the Board not aware that syphilis, hydrophobia, the itch, and certain forms of scalp disease are communicated by direct contact, while no proof of their infectious character has been demonstrated? that marsh fevers have never been communicated by direct contact? that smallpox is communicated both by direct contact, and by inhalation of the breath of an infected person? and that, with regard to plague and typhus, the evidence with regard to inoculation not being fully admissible, the share which cutaneous contact has in transmission cannot be determined, because it presupposes so close an approach, that interchange of the breath must take place in an ill-ventilated apartment?

After having admitted that in the strict sense of the word contagion, a contagious disease is one communicated by *contact with the breath* of an infected person, we have a section upon the distinction between zymotic and contagious influences:

> "It is not unreasonable, however, to suppose that a single infected person, and much more a large body of infected persons localizing themselves in the midst of a population already predisposed to disease and actually under an epidemic influence, may act upon that population zymotically, that is, as the leaven that sets in action the fermenting mass. 'It is difficult,' says Dr. Sutherland, 'to obtain satisfactory experimental evidence, but I have an opinion, founded on the result of a great number of observations, that—1. A healthy person, sleeping for a single night in a locality where cholera has localized itself, may return to a healthy locality and take cholera and die, and no one else suffer. 2. If there be a slight epidemic influence present, perhaps one or two persons in the house may suffer. 3. If the influence be still stronger, a few cases may happen in the neighbourhood, and that without any communication with the infected house; and should the epidemic influence increase in intensity, these dropping cases may be fresh centres,* from which the disease may spread with rapidity. I look on the poison which propagates cholera in an epidemic atmosphere as being truly *zymotic*, but not *contagious*. Why may it not be the same with other pestilences?'
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> "It is also important to bear in mind, that during the prevalence of an epidemic atmosphere, that is, at a time when the whole population of a district or a kingdom is habitually breathing an unwholesome medium—predisposing to disease, yet too much diluted to produce it unaided—a slight additional morbific influence is sufficient to cause active disease. . . . . But if the exhalations from persons in health are capable of producing this result, what must be the danger of breathing the air of a close room, in which numbers of persons are crowded together, labouring under such diseases as typhus, plague, and yellow fever." (pp. 54-6.)

What is all this but a full admission of the doctrine of contagion? What is it but a full admission that the "slight additional morbific influence," which a single affected person might contribute by his exhalations, should be carefully guarded against? and yet it is followed by one of the most amusing instances of special pleading we ever remember to have met with. A power assimilating all other diseases to its own nature is assumed for all prevalent epidemics; and then all depressing agencies which might under other circumstances induce disease, are supposed to produce the prevalent malady. All this is assumed and supposed, in order to explain the fact that nurses and attendants on the sick may be attacked by any prevailing disease, without resorting to the supposition of contagion. Now is it not a simple and rational conclusion that as persons exposed as nurses are to typhus, become affected with typhus, to smallpox with smallpox, to plague with plague, therefore the emanations from the sick actually contain the morbific matter which is the cause, the special cause, of either form of disease; and that these emanations act in proportion to their concentration, to the amount of dilution of morbific matter by atmospheric air, and in all cases more powerfully on those predisposed by debility or exhaustion to their influence? On the other hand, is it not a clumsy artifice to assume that these diseases being prevalent destroy all other diseases, assimilate them to the nature of typhus, smallpox, or plague, and that therefore all weak and exhausted persons, who, when an epidemic disease was not prevalent, would merely suffer from weakness and exhaustion, have their weakness and exhaustion during such prevalence "assimilated to the nature" of smallpox or typhus. But it is not only a clumsy artifice, it is an assumption daily contradicted by facts; for it is not necessary for the spread of these diseases that any epidemic should be prevalent, but simply that several patients should be crowded in an ill-ventilated apartment to *ensure* communication to some of the attendants. The returns of the Registrar-General prove that during late epidemics of typhus in Manchester and Glasgow, other diseases, instead of being assimilated to the nature of typhus, existed above the average proportion. It would appear that the authors of the Report consider that medical men are unaware that the more the emanations of febrile patients are concentrated, the less they are diluted with pure air, the more dangerous they are: yet Dr. Marcet, before Mr. Chadwick was born, taught us to scatter typhus patients about the general wards of an hospital, and showed that the disease did not spread. He knew, however, that it did spread when several such patients were collected in one ward, and therefore did not argue that typhus was not contagious, but simply that it required a certain concentration of the poisonous exhalations, in other words, a large dose of the poison, to cause communication of the disease. His views have been so generally received and acted upon by the profession, that the doubtful support of Mr. Chadwick is not needed to ensure their general adoption.

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* "Perhaps in everybody affected with cholera, there has been a gradual and progressive accumulation of cholera poison; such a body may, it is conceivable, again give out the poison it has accumulated, and in this manner actually increase the amount of poisonous matter in its immediate neighbourhood."—REPORT.

Having assumed that the different diseases asserted to be identical depend upon atmospheric conditions, the obvious inference is, that as such conditions are not amenable to quarantine, this system should be abolished; and this is the inference of the Board.

> "The outbreak of influenza in 1847 was preceded and accompanied by extraordinary risings and fallings of the barometer, by violent storms of wind and rain, by great and sudden alterations of temperature, and by such a disengagement of moisture in the shape of fog as to make it dark at mid-day. 'No electricity,' says the Registrar-General, 'stirred in the air during that week; all was still, as if Nature held her breath at the sight of the destroyer come forth to sacrifice her children." (p. 7.)

Therefore, because quarantine could not exert any influence upon the weather, it could not have kept out the influenza. Is this inconsequential reasoning, or is it not? Can the Board prove what share these risings and fallings of the barometer, &c., had in the production of influenza? Can they deny that similar changes occur without being accompanied by influenza? Can they deny that, with regard to cholera also, as such peculiarities have been noticed before its appearance, so, on the other hand, it has commenced without the least observable atmospheric change?

As we pass on to examine the extracts from various parliamentary reports and official records upon the subject of contagion and quarantine quoted in the Report before us, we find that authenticated facts controverting the principles laid down by the authors are systematically passed over, while the most powerful arguments in favour of these principles are carefully selected and published. To most men such omission and selection would be a charge of downright dishonesty, but we do not throw any such charge upon the authors of the Report; we merely believe that their minds have become so biassed by one particular limited view of the question, that they *cannot*, not that they *will not*, see anything whatever which may be opposed to this view. With regard to cholera, we find strong facts in favour of its non-contagious nature selected from many sources, while only two short extracts are given in favour of the opposite view, and these are placed in an appendix. Again, when the children affected with cholera in the Tooting Asylum were dispersed, it is alleged that they communicated the disease to some of the nurses at the places to which they were sent. The Board do not deny that this was the case; but they say that on examination, so large a proportion of apparently exceptional cases have proved to be no exceptions whatsoever, "as to warrant the presumption that none were so." Now, because out of 20 cases 15 are examined and prove not to be exceptional, we cannot consider the presumption warranted that the other 5 were also not exceptional. The mere fact of being exceptional would necessarily presuppose their rarity probably in a much greater proportion than even 1 in 20. Why was not every case examined? If examined, why not reported? If reported, why not published? We should be sorry to find that reports have on this occasion also been omitted because they contain disagreeable truths; yet those who remember the evidence of the Tooting cases can scarcely doubt that it is so. There is an evident desire to explain away some unpublished and troublesome facts in the following paragraph.

> "Cases of cholera, caused by infection, would have been consistent with the observations we have made with relation to typhus. But it turned out on inquiry, that before the arrival of the diseased children, the nurses had been labouring under premonitory symptoms, showing that they had been previously infected. These cases were all cited as evidence of the contagiousness of the disease; whereas, had they been well founded, they would only have been evidence of its infectiousness. But if, after all, cases should occur which, after the best examination that can be given, appear to be truly exceptional, they must be referred to the imperfection of our knowledge, and, however unable we may be to explain them satisfactorily, our opinion and practice must be determined by the preponderance of evidence and not by these isolated occurrences, which as they suggest interpretations opposed to large and general observation, are probably misleading, and the whole conditions of which are confessedly not known." (p. 58.)

We assert that opinion and practice must be guided by isolated occurrences and exceptional cases to some extent, as they show that exceptions to the most general rule may occur, and should be guarded against, and they often afford a species of positive evidence which no mere negative fact can disprove. For example, if a mad dog bite nineteen persons and only one should become affected with hydrophobia (and this has actually occurred), the one case is exceptional, but it is a positive proof of the contagious nature of the disease which the other eighteen negative facts cannot shake. The Board seem to be perfectly incapable of comprehending, that contagion may be one agent in the spread of epidemic diseases, without being the sole or even the principal agent; or of admitting that while non-contagion is the rule in some diseases, contagion may be the occasional exception, the possibility of which must be acknowledged, and ought to be guarded against.

If we pass on, we find the specimens of omission and selection still more remarkable with regard to plague. Here is one instance:

> "Captain Bonavia being asked, 'Of these numbers, have you ever known a case of plague occur when persons have been placed in the lazzaret, of course excepting those removed from vessels where the plague was raging at the time of their removal?' he answers, 'Never.'
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> "Dr. Laidlaw has also stated to us that he has availed himself of every opportunity of making inquiry as to this point among the officers of quarantine stations in all the foreign countries visited by him, and that he has never found even a reputed or suspected case of infection among this class of persons, nor had any one examined by him ever heard of such an instance." (p. 66.)

Now it may scarcely be believed, but it is true, that the reporters have gone to an old pamphlet by Mr. Holroyd for this piece of negative information, which has since been corrected by positive experience to the contrary in the lazaretto at Malta, of which Mr. Bonavia is the superintendent; and that the evidence has been fully reported to our government, and published by order of the House of Commons in a blue book, from which our authors have made many extracts, but have not selected for publication the accounts of cases recorded there in which attendants upon plague patients, who had been brought to the lazaretto of Malta in ships from Alexandria, contracted plague, although these attendants had not been on board the ships. There were no other cases of plague in Malta at the time nor afterwards, careful isolation being practised. In the same way the evidence here published with regard to the effects of quarantine at Alexandria has been completely refuted by subsequent evidence, which has also been printed in parliamentary reports. With regard also to the alleged importation of pestilential virus by articles of commerce, all the evidence in this Report is as usual on one side; and selected from the same documents, which contain some very curious facts proving that, at the very least, room for doubt remains, and that infected articles of clothing or merchandize, which have not been freely exposed to the air, really have communicated plague to countries where it was not previously existing. The reporters, instead of examining these statements, and pointing out exposure to air as a simple and efficient means of safety, coolly pass them over unnoticed. It must be remembered also that all the evidence collected is of the negative class. A certain number of persons are exposed to the danger of contagion, and no harm results, therefore contagion is a fable; although in the very same book from which these quotations are made, the most positive proof of communication from affected to unaffected persons is given. We showed just now, by a reference to hydrophobia, the absurdity of attempting to overturn positive by mere negative evidence. In this Report, it is true, the attempt is not made, the positive evidence being left unnoticed.

Again, in quoting the report of Dr. Aubert Roche to the French Academy, the reporters refer to variations in *sanitary condition*, that difference in the relative mortality among several classes of persons at Alexandria, which Dr. Aubert Roche expressly and very properly refers to *race*. The Negroes, Nubians, and Arabs, not soldiers, during the plague of Alexandria in 1835, were in nearly identical hygienic conditions, yet they suffered in the proportions of 84, 61, and 55 per cent. respectively, while the rate in the remainder of the population varied only from 5 to 14 per cent. This latter variation was owing to a variety of causes, better sanitary condition and segregation among the rest; but the former was clearly a question of race. The Negro enjoys an almost entire immunity from the fevers which are so destructive to the white race on the western coast of Africa, and is peculiarly susceptible to the plague of the north-eastern, from which the white man is comparatively exempt.

Some of the practical deductions of the Board would rather astonish a logician, and equally startle a practical man of business. Because great epidemics are periodical to a certain extent, and there is "a relation between the conditions of the atmosphere and the outbreak and progress of epidemic disease, though we are as yet ignorant of the nature of that relation," quarantine is useless. Because great epidemics are diffused over vast ranges of country and with a certain regularity of course, quarantine is useless. Because great epidemics at times rapidly spread over a whole district, quarantine is useless. Because epidemic diseases are for a certain period latent in the system before their presence is known, and may be present in a community also for some time without recognition, quarantine is useless. Because certain removable causes of disease produce a greater prevalence of any epidemic disease in certain localities, quarantine is useless.

When such conclusions as these are drawn from such premises, it is better not to enter into theoretical discussions; but it is still necessary to examine how far the conclusions stand the test of practical observation; and therefore we will now put aside all points of theory, and pass at once to the question, whether or not quarantine "accomplishes, or is capable of accomplishing, its professed object." We are quite ready to allow, with the authors of the Report, that this is a question of evidence and experience; but we think we can prove that evidence and experience are diametrically opposed to the following sweeping conclusions: "It appears to us that facts and observations have been already stated which place beyond all reasonable doubt the utter inutility of this system." (p. 59.) At p. 6, we also hear of the "signal failure of quarantine;" and at p. 74, "that experience has fully shown the utter inefficiency of quarantine, and the danger that has followed from the substitution by this system of false for real securities;" and in the same page, that, as "ventilation and dispersion can dissipate any contagion, the entire basis of quarantine is gone, and the system must be abandoned as useless and even injurious."

Our answer to all this is very simple. Experience has most fully established the great efficacy and real security to be derived from quarantine regulations when properly practised. Until the last twelve years, plague was so regularly a yearly visitor in the Levant, that it was believed to be indigenous in the whole Ottoman dominions. Russia occupied some of the Turkish provinces, the sanitary condition of which, as far as regards the local origin of disease, has not been much ameliorated by the change of dynasty; but quarantine was established, importation of plague was prevented, and these districts have since been as free from plague as the rest of Europe. Since the new kingdom of Greece has been separated from Turkey, and plague has been kept from its seaports by quarantine, no plague has been known in the kingdom, although those best acquainted with the country know that no other sanitary improvements of importance have been effected. In the Barbary States, since quarantine has been established no plague has existed. Formerly it was almost yearly imported by pilgrims, on their return from Mecca. Again, during the last twelve years, quarantine has been established in Constantinople itself; several cases of plague have been imported and confined in the lazaretto; the disease has spread in the lazaretto, but not beyond the walls; and the city has remained free from a disease, which, before quarantine was practised, was so prevalent, that no one doubted that it was of local origin. Those who know Constantinople, know that, if filth, and crowding, and defective sewerage could produce plague, there it would certainly be in abundance. In Egypt itself, where, in certain districts, plague is endemic, and preserved by the system of burial, its communication to other districts is often prevented by quarantine regulations (Brit. and For. Med. Rev., vol. XXIV, pp. 248-50). In the Report of the Committee of the French Academy, quoted with such approbation in the present Report, when its statements suit the purpose in view, are published the following conclusions from authentic notices of twenty-five ships, which, since 1720, have arrived in France and Italy with the plague on board, and from facts observed at Venice, Leghorn, and Genoa: "That the plague is not only transmitted on board ship among persons leaving the same shores, breathing the same air, having the same exercise, the same habits, the same food, but that the plague patient, deposited in a European lazaretto, becomes the cause which developes in others the affection from which he is suffering" (Report of French Acad., p. 163); and that—"It is incontestable that the plague is transmissible beyond epidemic foci, both in ships at sea and in the lazarettos of Europe." (Op. cit., p. 165.) These conclusions are also fully proved by official returns, recording facts of importation at Malta, published in our own parliamentary correspondence on the quarantine laws.

In the same French Report will be also found accounts by M. Ségur Dupeyron and M. Aubert Roche of every case of plague which has occurred in the lazarettos of Europe since 1720; and of sixty-five cases of maritime importation of plague, it having broken out in fifty of them during the voyage. All this, as well as the conclusions above quoted, are omitted here, and that alone is selected for publication which supports the favorite views of Mr. Chadwick.

The third of the three sentences quoted in the preceding page, is one resembling many others in the Report, suggesting the idea that quarantine and sanitary measures are opposed and irreconcileable systems. We are told at page 84, "the only means of preventing the origin and spread of epidemic disease, is the adoption of sanitary measures;" and at page 5, "that the true safeguards against pestilential diseases are not quarantine regulations, but sanitary measures;" as if quarantine was not itself a sanitary measure, which we assert it to be, and one of the first importance. It fully recognises the importance of other sanitary measures, but with the additional precaution of separating the sick from the healthy, not in crowding the sick together, but in separating them from each other, and from those to whom they might communicate disease. But the reporters state that,—

> "The principal ground on which we have objected to the continuance of quarantine, is that the fundamental principle on which it is based is fallacious, and that the only means of preventing the origin and spread of epidemic disease, is the adoption of sanitary measures. *Substitution* of sanitary measures for quarantine

restrictions would, in our opinion, render the importation of any disease from one country into another in the highest degree improbable, and we therefore accompany the proposal to abolish quarantine with the recommendation to substitute for it a plan of sanitary regulation." (p. 84.)

And at page 3 is also recommended the "*substitution* of preventive measures, founded on principles fundamentally different from those of quarantine." Yet those who have been the most ardent advocates for the reform, not the abolition, of quarantine, have also been the most earnest in their endeavours to render the regulations of quarantine unnecessary, by pointing out how the diseases which render their continuance advisable may be annihilated. They point out not only how to prevent the spread of contagious disease by limiting communication between the sick and healthy, but also how the origin of the disease may be traced and destroyed.* They do not, however, follow the irrational example of Mr. Chadwick and Dr. S. Smith, and argue that, as, in the course of time, a perfect system of sanitary regulation perfectly carried out, may probably destroy certain diseases, therefore, while these regulations are being tried and completed, we are to throw aside all *other* means of guarding against the spread of such diseases. It would be just as absurd to argue that, because vaccination is a preventive to smallpox, and because if perfectly and universally practised smallpox would probably become unknown in the course of time, no means are to be taken to prevent the spread of smallpox by inoculation, while measures are being adopted to complete the universal practice of vaccination.

We have seen numerous specimens in the foregoing pages of the kind of practical inferences the Board deduce even from their own assumptions. Let us now briefly compare their inferences from known and admitted facts, with the inferences common sense would draw from the same facts. Because there are evident inconsistencies in present quarantine regulations, and numerous abuses in their practical application, the Board recommend the abolition of quarantine altogether. What says common sense? Simply, remove the inconsistencies, reform the abuses of an institution salutary in its tendency, in order that we may obtain the greatest possible public good with the least possible individual inconvenience and loss. Because some quarantine stations are in bleak, unhealthy localities, the Board say, destroy them all. Common sense says, No, simply remove them to healthy, airy situations. Because unnecessary laws impede commerce, and cause loss to merchants, the Board would expose merchants and their neighbours to the danger of plague to save their pockets. Common sense says, so alter the laws that no unnecessary loss may be the result, while every guarantee is afforded for public security. Because smallpox and scarlatina are more destructive than plague, the Board would run a risk of giving us plague also. Common sense says, keep away smallpox and scarlatina as well as plague. Because consuls and resident authorities in foreign ports are said to state that it is difficult or impossible to obtain information as to the prevalence of disease in really affected parts, for their guidance in issuing or withholding clean bills of health, we, in the name of common sense, do not say that bills of health are useless, but that consuls who make such statements most decidedly are so, and that they ought to be replaced by men who are sufficiently active and intelligent to obtain reports from the medical men in their districts, which would give authority and correctness to the bills of health. Because quarantine regulations are extensively evaded, we do not say abolish them, but take care that the evasions should not be permitted. Because passengers misrepresent the nature and cause of their sickness to escape quarantine, we do not say, admit them indiscriminately to intercourse with the healthy, but take care that proper medical supervision renders misrepresentation impossible. Because there is not proper provision for attendance on sick persons in quarantine, we do not say, send the sick wandering along the sea-shore, but provide them with proper attendance. Because some quarantine stations are out of the reach of medical officers, we do not say, destroy the station, but appoint a resident medical officer, or remove it. Because the system at Stangate Creek is not perfect, we do not say all systems are bad, but, simply, reform the system at Stangate Creek. Because some persons think that the evils of quarantine are greater than the advantages, we do not say, banish both, but simply, banish the evils and retain the advantages.

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* See Brit. and For. Med. Rev., loc. cit.

To some extent, the practical recommendations of the Board are good, and cannot be considered as carrying out the proposition for the abolition of quarantine. At page 123 they say—

> "Instead of detaining all vessels whatsoever arriving from ports which may happen to be the seats of epidemic disease, we propose to detain only the persons who may be in an actual state of ill health, or labouring under epidemic disease. Instead of keeping the parties infected together on board their own vessel, or in a building of the description of those used as lazarettos, we propose that they shall, as far as practicable, be immediately separated, and removed to places where the air is pure, and where suitable accommodation may be provided for them. Instead of arresting vessels which arrive at a port distant from a quarantine station, and keeping passengers together who may be in a state of disease until they are sent to a distant quarantine station, we propose that medical attention shall be given at once on the spot, and for their own proper relief in the first instance, and not as sacrifices to the false notion of security to persons on shore."

But with some good this paragraph contains grave practical mistakes. We say certainly, as all quarantine officers of the present day would say, separate infected parties, remove them to places where the air is pure, and where suitable accommodation may be provided, but take care, also, that they are not allowed to communicate their disease to a healthy community.

If it be said that it is impossible in practice to reconcile the results of conflicting opinions, to satisfy all reasonable men whose views on the subject of contagion differ, to afford security to the public even from imaginary danger, with but little inconvenience to individuals, slight impediments to commerce, and a concentration of attention of health officers upon real seats of danger, we reply, that nothing is more simple, and that all this might be effected by enforcing the following four rules:

1. To furnish all ships with *clean* bills of health when no plague is present at the port of departure, and with *foul* bills in the opposite state.
2. To receive all ships with clean bills of health in free pratique.
3. To impose eight clear days after arrival, and ventilation of cargo to all ships with a foul bill.
4. In case of any suspected disease on board, the quarantine to be regulated in duration by the local authorities.

If these rules became law, it would only be from Alexandria that vessels would arrive subject to quarantine on account of plague, except in very rare instances (which would yearly become more rare) from some parts of Turkey and the Levant. Even from Alexandria whole years would sometimes pass without a vessel being furnished with a foul bill of health; and surely eight days' quarantine for vessels in these few exceptional cases could scarcely be considered as productive of great individual inconvenience, or serious commercial loss. At the same time scientific investigation has shown that nothing more is requisite to ensure public security. It would probably not be twice in a century that a ship would be placed in quarantine, on account of having yellow fever on board, in an English port.

We have now finished what has been a very unpleasant task. We should have infinitely preferred awarding to the whole Report the unqualified praise which is due, and which we have rendered, to one portion of it. But we have a public duty to perform, which permits no influence of private feelings, or respect for official position, to interfere with its fulfilment. We could not pass over glaring and serious errors, leading to grave practical dangers, without pointing them out before it was too late,—without warning the legislature from adopting suggestions made without sufficient reflection, and evidently with an imperfect knowledge of the subject of the Report.

We do not wish to enter at present into an examination of the constitution of the Board of Health; but the Report we have just criticised clearly proves that it must be remodelled before it can obtain or deserve the confidence of the profession or the public. Instead of one medical man and three laymen, we should say reverse the order, and let us have a lay president, with a medical committee of three or more. This is the constitution which has proved on trial to be the best for the Board of Admiralty. When this Board consisted solely of persons not educated for the navy, ignorance of naval affairs and political jobbing were its notorious characteristics. When it consisted exclusively of naval officers, the exaggerated importance given by these gentlemen to purely professional questions led to wasteful expenditure of public money and unfair promotion of personal friends. To remedy these evils, a lay president and secretary were appointed; and with these sat a committee of six or more naval officers. In this way the necessary evils attendant both upon an exclusively lay and exclusively professional Board have been divided in practice—the ignorance of the one, the waste of the other, being alike corrected. A similar constitution should be framed for the Board of Health. Is it not truly absurd, on the approach of a great pestilence, to find medical discussions upon the relations between diarrhœa and the first stage of cholera—upon the treatment of the collapsed stage, as distinguished from that of the premonitory diarrhœa—sent forth to the public by a non-medical secretary, *by command* of two noble lords, a barrister, and but one physician? Surely this cannot be allowed; and we do trust that before another Medical Report is issued by the Board, its medical members will be increased in number, and will take care that nothing so discreditable as the present production shall again appear.

ART. XIII.

1. *On Healthy and Diseased Structure, and the True Principles of Treatment for the Cure of Disease, especially Consumption and Scrofula; founded on Microscopical Analysis.* By WILLIAM ADDISON, M.D., F.R.S., Licentiate of the Royal College of Physicians.—*London*, 1849. 8vo, pp. 320. With Four Plates.
2. *Thoughts on Pulmonary Consumption; with an Appendix on the Climate of Torquay.* By WILLIAM HERBIES MADDEN, M.D., Physician to the Torbay Infirmary and Dispensary, &c.—*London*, 1849. Post 8vo, pp. 220.
3. *De l'Analogie et des Différences entre les Tubercules et les Scrofules; Mémoire qui au Concours, fondé par Portal, a mérité de l'Académie de Médecine une mention honorable.* Par A. LEGRAND, Docteur en Médecine de la Faculté de Paris, &c. &c.—*Paris*, 1849. 8vo, pp. 402.
4. *The Undercliff of the Isle of Wight; its Climate, History, and Natural Productions.* By GEORGE A. MARTIN, M.D.—*London*, 1849. Post 8vo, pp. 366.

AMONGST all the various forms of disease to which the human organism is subject, there are none whose wide-spread diffusion, insidiousness of origin and attack, and terrible effects upon health, happiness, and life, bespeak, from the cultivators of medical science, a more sedulous and patient investigation than those comprehended under the designations of scrofula and tubercle. It were impossible, indeed, but that a disease,—of which, to adopt the views most commonly entertained (in this country at least) with respect to the relations of these affections, one or the other form has left some sad memorial of its presence in almost every home, deforming and weakening where it may have failed to destroy, and whose fatal results have been estimated to produce from one fifth to one third of the total mortality,—should prompt the most earnest endeavours to seek out its true essential nature, to discover the first events in that series of disordered actions out of which our general notion of the disease is constructed, to trace out the conditions under which these actions arise, and to determine the true order of succession, the efficient connexion between these several facts; and all this, with an ultimate reference rather to prevention, which we take to be the highest end of medicine, than to cure. And accordingly it is to a recognition of the strong claims of these affections upon the attention of the physician, that we owe some of the most able and important researches which the literature of medicine records.

All that has hitherto been done, however, still leaves the ground open for further attempts at reaching a true and complete theory of scrofula and tubercle, which shall determine their relations upon a certain basis, and which, explaining the exact nature of those primary departures from normal function, whence all the subsequent changes characteristic of these forms of disease proceed, shall show—to use Dr. Madden's words—"by what links the separate facts are connected, and demonstrate *why* the results are what our investigations prove them to be." And not only do we feel that such a theory is greatly to be desired for the sake of its momentous importance to treatment, and yet more to prophylaxis; but we entertain a conviction that our acquaintance with the physiology of the blood and the nutritive actions,—with what may be considered the corresponding particulars in the healthy states and actions, to those we are in search of under the pathological condition,—has attained of late years so much of precision and certainty, as to warrant an expectation that a more exact pathology of all diseases of nutrition should be rendered possible; although we cannot but feel a misgiving, that the "ultimate facts" capable of being reached will still leave a residuum veiled in darkness which no analysis will be subtle enough to penetrate.

It was then with no small avidity that we proceeded to the examination of three works, making their appearance simultaneously, the title-pages of which led us to hope for some valuable accession to our knowledge on these points. This examination, however, it must be confessed, has, with exception of Dr. Madden's little volume, resulted in more of disappointment than satisfaction. We have, indeed, seldom risen from the perusal of three works devoted in great part to the same topics, with more diverse feelings as to the merits of each, or with a more varying degree of satisfaction in coming to their several terminations. We must own that we closed Dr. Addison's book, heartily glad to have got to the end; and with a far less satisfactory impression of its contents than our attentive study of them might have been expected to leave. Bearing the impress, like all its author's works, of no little originality of thought and observation, though sometimes having more of the semblance than the substance of this, from the mere draping of old ideas in new speech,—there is so much of confusedness both in expression and arrangement, that the perusal of this book is throughout a painful exercise of patience. The result, too, at least so far as regards those practical bearings of pathological research, with reference to which we so eagerly desire further enlightenment is, whether true or not, eminently unsatisfactory. For what can be more so than to learn that,—

> "It follows from all the circumstances of the case, embryonic development, growth, and nutrition, as unravelled by microscopical observations,—that if we would rid ourselves of the evils of inflammation and scrofula, we must be prepared also to resign the benefits of cure and healing. For it appears that the primary stages of growth originally and the first steps necessary for the healing of a wound, the repair of fractures, and the cure of a burn, are identical with those morphological operations which originate and maintain a scrofulous disease." (p. 316.)

It is to the unfolding and establishment, and illustration of the idea expressed in this last sentence, that the great if not sole aim of Dr. Addison, in the present work, seems to us to have been directed. From the "Introduction," which is devoted to an explanation of the now well-known facts respecting the cell-development of vegetables, and the laws which govern the metamorphosis, regular, irregular, and *retrograde* of plants,—through a succession of chapters, severally headed "Practical Physiology," "Practical Pathology," "Practical Psychology," (!) "Semeiology," "Etiology," "Therapeutics and Cure," to the end of the work; this appears to be the great object sought; and the one governing idea, variously expressed, meets us again and again. How far this point is made out, and whether, if established, we gain anything from it, we shall presently more particularly inquire.

Meanwhile we cannot but contrast with Dr. Addison's the modest, unpretending volume of Dr. Madden, directed, like it, to the development and elucidation of one leading idea respecting the substantive nature of phthisis, but characterised by great precision of thought and language, by a lucid order, and a wide view and philosophical consideration of analogies. This idea is, that tubercular disease has its origin in a morbid poison: and it is sought to be shown how, by this hypothesis, we can give a more natural and satisfactory explanation of the morbid phenomena, than is possible upon any other supposition, while there flow out of it at once an explication of the ascertained beneficial results of remedies, and a guide for the rational treatment of consumption. We do not hesitate to express our conviction, that in giving this definite expression to what no doubt many have practically recognised in the doctrine of *diathesis*, Dr. Madden has enunciated a proposition, containing much probability of truth, and one which, if established, will prove an important advance in our notions respecting tubercular disease. But whether our readers should so far accord with us, or not, we think they will agree with us as to the beneficial tendency of this Essay to promote inquiry in a definite direction; while it certainly furnishes an example of a well-directed attempt,—firstly, by analysing the characters, both structural and chemical, of the morbid product, the condition of the blood, the general state of the nutritive function, and the most characteristic symptoms of phthisis; and secondly, by endeavouring to interpret these several facts by the light of analogies supplied by other diseases, to unravel the conditions which lead to the deposition of tubercle, and to decipher those which are fundamental and primary.

The work which stands third in our list owes its existence to a competition in 1847 for a prize founded by Portal, of which the subject was that forming the title of this memoir; and it was published as having been honoured by the Royal Academy of Medicine, in whose hands the adjudication was left, *not* with the prize, but with the distinction of an "honorable mention." It differs widely in scope and character from either of the other two with which we have associated it; its object being confined to that expressed in the title, and the discussion being mainly rested on the facts supplied by a large number of cases, amounting to 76, of which the particulars are formally detailed with all the amplitude usual with our Gallican brethren, and which thus constitute the staple of the entire book. Had the cases been compressed to an extent commensurate with the bearing of their several characteristic points on the main subject, the result would have been a pamphlet, instead of a volume of some four hundred pages. But the author's endeavour would seem to have been rather to spin out than to condense and abridge; this tendency to prolixity being, however, by no means confined to him, but a common characteristic of the writings of his countrymen. Another sin, not of commission, but of omission, common to the medical writers of France, and illustrated in the work before us, is the remarkable absence of that allusion to the productions of English authors, which anyone well acquainted with the medical literature of both countries would consider inevitable. It will be a sufficient justification of this remark to observe that, in a work specially devoted to a consideration of the relations of scrofula and tubercle, no mention of, or allusion to, the works of Mr. Phillips or Dr. Glover is to be found, while Sir James Clark is only referred to for some subordinate point of statistics. It is true, some two or three of our older writers, whose names and opinions may have become known to the author through the second-hand channel of 'Dictionaries of Medicine,' and the like, are referred to; and one of the mottoes on the title-page is even extracted from an English writer so antiquated as Morton, the author of the 'Phthisiologia.' An amusing indication of the extent of M. Legrand's acquaintance with the medical literature of our country, is contained in his reference to the distinguished author of the 'Dictionary of Practical Medicine, as "le Doct. Copland *qui a contribué à la rédaction* du 'Dictionary of Practic. Med.'"

Commencing our more particular survey with Dr. Addison's treatise, we shall endeavour to lay before our readers a concise and connected abstract of the pathological views enunciated in it.—These views are essentially *morphological;* that is, Dr. Addison considers a change of structure, which he designates as a "retrograde morphology," as not merely concerned in the local manifestations of the scrofulous or tubercular diathesis, but as the essential condition of the "unhealthy constitution" from which these proceed. He points, at the commencement of his volume, to the retrograde metamorphosis of plants, as consisting in the intrusion, among the normal elements of a texture, of those distinctive of a prior and lower type; "as when the green chlorophylle cells of the leaf intrude upon and displace the coloured cells of the petal; or when the structure of the stamens assumes the characteristics of the petals;"—changes which may result either from a tendency derived from the parent plant, or be directly induced through the operation of external agencies, such as in the former case may have produced them in the parent; viz. certain conditions of soil, light, heat, air, and moisture. In the changes characteristic of inflammation and scrofulous disease, the analogues of these facts are visible. The structures of the human body are all evolved from a corpuscular or cellular embryonic condition, corresponding to the incoherent corpuscles (or secreting cells) of which the lowest forms of plants consist: all the organs at first consist of such "soft parenchymatous, or corpuscular granulation textures;" then coherent, cellular, and fibrous textures are developed (membranes), answering to the "ministerial, non-secreting cells" of plants, such as are found in fungi, mosses, and ferns, and in the cuticle of the phanerogamia; lastly, cartilage and bone correspond to the spiral fibres and woody textures, which in the highest plants are superadded to the two previous forms. Such is the law or order of development. Now inflammation and scrofula are but processes of abnormal growth and nutrition, and are therefore analogous to natural growth and nutrition. In natural nutrition, the colourless blood-corpuscles pass into—becoming incorporated with—the parietes of the vessels, and thus getting outside them, become transformed into the various special textures. But in inflammation and scrofulous diseases, there is an "exaggerated nutritive action," "an accumulation of morphological elements, consisting of colourless blood-cells and protoplasma;" this accumulation produces a change in the walls of the vessels from "fibrous to corpuscular elements;" and "granulation-fabrics" arise in serous and fibrous textures, and textures which are normally corpuscular ulcerate, that is, discharge incoherent cells (pus). These effects are evidently a retrogradation of the textures from the normal to an earlier type; and as the pulmonary parenchyma is normally of a fibrous or serous type, its retrograde metamorphosis is to the corpuscular. But the subsequent morphology, as well as the rapidity and extent of the previous changes, differs in inflammation and scrofula. In the former, the newly accumulated elements are only *intermingled* with the natural elements of the part; and when they have run through the stages of their metamorphosis, the exaggerated nutritive activity spontaneously subsides, any excess of matter is absorbed, and the parts return to their natural state. In scrofula, on the other hand, "the new elements and granulation-fabrics *overrun and destroy* the natural texture." Corresponding to the other points of agreement between inflammation and scrofula, is the entire absence in tubercular matter of any essentially distinctive character.

> "I have repeatedly," says Dr. Addison, "examined with the microscope the material deposited in the air-cell of the lung in *pneumonia*, and compared its characters and appearance with that forming a tubercle, without being able to detect any more essential or specific difference between them than exists between purulent matter recently excreted, and that of an old chronic abscess." (p. 52.)
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> "Lebert states that the cells he has seen in tubercle and tuberculous matter, have certain peculiar and distinctive characters, but we have found all the characters he describes in some of the cells of ordinary purulent matter from simple pimples on the skin." (p. 53.)

In strict accordance, too, with these views, "the sore of a burn in its granulation state" is said to be, "strictly speaking, scrofulous—inasmuch as it represents a type anterior to that of the normal, areolar, fibrous texture on which it appears." (p. 72.) And we arrive at length at the explicit announcement that "*scrofula* is only a conventional term expressing forms of nutrition below the natural standard."

Now, in all this there seems to us a portion both of what is new and of what is true: not new, indeed, to those who have been at the trouble of making themselves acquainted with Dr. Addison's previous publications, but only new in the sense of its belonging exclusively to the author. And we are inclined to pursue the apoththegm we have borrowed, and to say that "what is new is not true, and what is true is not new." In the account of the mechanism of the processes above described, our readers will recognise Dr. Addison's peculiar views of the process of nutrition, which have been already noticed in the pages of our predecessors. And if we were to dwell upon this description, we should find as much reason to dissent from the representation given of the changes in the parietes of the vessels, and the mode of development of the new material, as in his descriptions of normal nutrition. Nor can we assent to the assertion, that pus-cells, granulation-cells, epithelia, &c., are all merely altered colourless blood-corpuscles, which have worked their way to the outside of the vessels.

But the more essential features in Dr. Addison's theory may be considered apart from these views, and may be reduced to this:—that the manifestations of scrofula are only the expression of a disposition in the nutritive elements when accumulated in excess, as in inflammation, to assume a retrograde morphology. In the close analogy, amounting indeed, so far as we can understand, to identification up to a certain point, of the inflammatory processes, and of those engaged in the production of scrofulous changes, we find much to object to. We see no reason to suppose that an excess of nutritive elements is a necessary antecedent of these last-mentioned changes; and if we did, we should still dispute the propriety of viewing inflammation and scrofula in the common light of forms of exaggerated nutrition. To do so would, in our apprehension, involve a fundamental error respecting the essential nature of both states. Inflammation, we agree with Dr. Addison, is abnormal nutrition; but simply exaggerated nutrition it is not. And such a view of its nature seems to us based either upon a most surprisingly limited contemplation of its phenomena, or upon a similarly narrow consideration of the normal process of nutrition; as though, in either case, the only essential condition was a supply or an accumulation of white blood-corpuscles and plasma, without any reference to the vital properties and condition of the tissues to which this supply is brought, on the one hand, or to those of the plasma itself on the other. We regard inflammation as an abnormal state of the nutritive function, involving a complete disturbance of the normal relations, as regards the inflamed tissue, between supply and demand of nutritive materials, and also an alteration in the usual characters of these materials, with a diminished vitality of the tissues; but in the quiet substitution of scrofulous material for the natural textures,—as when tubercular matter is, without any evidence of inflammation, either by symptoms at the time, or by appearances after death, silently deposited in the pulmonary parenchyma,—we recognise something widely different from, because wanting all the essential characteristics of, the inflammatory process. Dr. Addison's account of the manner in which scrofulous changes occur, so far as it is applicable at all, is so only to scrofulous inflammation, that is, to inflammation occurring in the scrofulous constitution, and modified by the scrofulous diathesis. In this, whether occurring in the external or internal parts of the body; whether diffused extensively through one or both lungs, and preceding an extensive deposition of miliary tubercles, —or attacking a limited portion of one of these organs, and giving rise to one of those albuminous infiltrations described by Dr. Addison of Guy's Hospital, and which, we believe, exhibit different degrees of departure from the plastic deposits of healthy inflammation, and of approach to the most characteristic form of tubercle,—or whether developed around a mass of hitherto dormant tubercular deposit;—in each and all of these instances of its occurrence, we agree with Dr. Addison in recognising features which stamp the process as one of inflammation, and see, with him, in the "ensuing morphology,"—in the permanent unconformable deposit,—evidence of a peculiar inherent constitutional disposition. But to limit the production of tubercle to this process, is to adopt a theory which we decline to discuss any further, simply because we look upon it as one of those questions which we are entitled to consider as settled, after full examination, by the all but unanimous verdict of the profession.

But the fundamental idea in Dr. Addison's theory—that of a tendency to a retrograde type of structure,—may, we conceive, if duly generalized, be accepted as a true expression, so far as it goes, of the scrofulous diathesis. In the general condition of the formative energy, and the frequency of fatty degeneration, as well as in the characters, both chemical and structural, of the products denominated tuberculous, and of the growths resulting from scrofulous inflammation, there is much to bear out such a view. But in this there is no novelty: neither do we believe that it contains the whole truth, nor that, when we have given the largest possible expression to Dr. Addison's morphological explanation of the scrofulous diathesis, we have arrived at the highest attainable notion of the nature of this condition. The explanation proposed goes, in fact, no further than to refer certain of the morbid manifestations to the operation of laws of development; just as monstrosities and malformations are explained, in a certain sense, by being referred to arrests of the normal course: and there are phenomena of characteristic and striking significance in the history of scrofula, which receive no elucidation at all from this mere morphological tendency. The peculiar impress stamped upon the scrofulous constitution, the defect of muscular and nervous energy, the cachexia, the emaciation, the cognisable alterations in the constitution of the blood, the alternations between external and internal deposits, the artificial or accidental production of the diathesis,—all these seem to point to some special vitiation of the blood, as the essence of the diathesis,—the proximate cause of that state from which all the phenomena of scrofula are derived, including those irregularities and defects of nutrition and "retrograde morphologies," upon which Dr. Addison so concentrates his attention.

It is to the blood that most modern researches have been directed, in seeking an explanation of the condition in question; and two distinct and definite theories have been proposed, each tracing the scrofulous products to a primary change in this fluid. In the one of these, it is rather assumed than proved, that tubercular matter already exists as such in the blood, and is simply deposited from it; in the other, the imperfect elaboration of the nutritive plasma, which its chemical and structural characters evince, is regarded as leading to the deposition of an unorganizable albumen, in place of a plastic fibrine. The former view derived, for a time, considerable support from the weight of Dr. Carswell's name, who unhesitatingly announced his detection of tubercular matter in the blood; while the absence of any other cognisable changes in this fluid, which could be regarded as peculiar to scrofula, contributed to give it currency. There are, probably, few who now look upon this as a correct interpretation of what Dr. Carswell saw.

The theory of an imperfectly-elaborated plasma was that of the late Dr. Todd of Brighton; and is evidently near akin to the morphological theory of Dr. Addison; only, by going a step further back, and by locating the first faulty development in the blood itself, it takes a somewhat more comprehensive form. Still it seems inadequate to explain many of the facts presented in the history of scrofulous diseases; and it is open to the further objection, that an equal imperfection in the character of the blood-plasma, attended with, and probably induced by, a similar or greater deficiency of the red corpuscles, exists in chlorosis,—yet without any necessary manifestation of scrofulous symptoms. We cannot but think that these obvious changes in the blood, while they unquestionably play an important part in many of the symptoms presented by scrofulous subjects, have yet to be themselves explained, in common with the more peculiar and specific characters of the disease, by reference to the operation of some element which has gained admission into, or been developed in, the blood, and which, like lead-poison, has the property of diminishing the red corpuscles, probably also of otherwise modifying them, and, through these changes, of inducing a lowered vitality in the entire fluid. But this brings us to the point, which it is the main object of Dr. Madden's treatise to establish; and to this, as unfolded and elucidated in his pages, we shall devote what space remains to us.

In doing this, it will not be necessary for us to follow Dr. Madden through every step of his argument. He enters, in the first part of his book, into the consideration of the chemistry and histology of tubercle, the ascertained changes in the blood, and the state of the nutritive function in scrofula and phthisis. In the second, he treats of the manifested disease (phthisis), discussing successively its leading forms and varieties, its symptoms, its analogies with diseases arising from morbid poisons, its diagnosis and treatment. It is to the section devoted to the analogies of phthisis that we would chiefly direct the attention of our readers, after briefly dwelling upon a few points contained in the former part of the work.

In estimating the results of the analyses of tubercular matter, of which he presents several, Dr. Madden points out a substantial agreement amidst a certain amount of variation; and indicates, as points of significance in which all analysts coincide, the existence of casein, and the preponderance of water, fat, and extractive matters. The casein is an evident indication of defective elaboration, and the other facts point in the same direction; the fat being evidently the result of a process of degeneration, whilst the extractive matters, Dr. Madden seems to think, are more probably a mere expression of the undue activity of the decomposing forces, than an evidence of the retention in the system of the effete products of normal waste from failure of the excretory processes. We do not see, however, that he adduces any adequate proof of this last position, which may or may not be correct.—Dr. Madden shows himself to be practically acquainted with the microscopical characters of tubercle; and draws from them an argument in favour of the operation of a special cause, comparing, in this point of view, the corpuscles of tubercle with the nucleated cells of cancer. Upon this point, however, we must reserve some observations which we desire to make until our next Number; and we must for the present satisfy ourselves with remarking, that we do not think it the less likely, that the tubercular product may be due to a special vitiation of the blood, even if it should not exhibit—as we believe it does not—a characteristic corpuscular element, like that presented by carcinoma.

In the section on the state of the blood in phthisis, Dr. Madden notices only the leading particulars, with which we may presume our readers to be familiar; and in the section following, upon the state of nutrition, he rationally connects the chilliness and pallor of strumous patients, and their defective muscular and nervous energy, with the diminished amount of red corpuscles, and the imperfection of their structures with the low plasticity of the fibrin; which latter condition eventually leads to the deposit of cacoplastic and aplastic tubercle, in place of healthy tissue. But though these results are thus to a certain extent explained, or rather traced to *some* of their necessarily antecedent conditions, Dr. Madden considers that there must still be something else behind,—some *other* antecedent requisite to give effect to the rest, and without which the appreciable changes in the blood would not necessarily be followed, any more than the same changes in chlorosis necessarily are, by the deposition of tubercular matter. Besides that, as Dr. Madden very justly observes, these changes "must themselves depend upon some prior cause, for blood does not become diseased spontaneously."

We pass over the section devoted to a consideration of the "identity of tubercular and scrofulous diseases," in which some of Mr. Phillips's objections are satisfactorily met; as we intend to discuss this subject more in full in our next Number. And we must content ourselves with simply mentioning that the first two sections of the second part contain a brief but well-drawn sketch of the leading forms and varieties, and of the symptoms, of phthisis; in order that we may dwell the longer on that—the most important in the volume—in which Dr. Madden investigates the analogies of scrofulous diseases with those arising from morbid poisons.

In this he first shows, by a detail of the symptoms characteristic of poisoning by mercury, iodine, and lead—three inorganic substances,—and by spurred rye—a substance from the organic kingdom,—how a variety of specific effects follow the direct introduction of particular matters into the system, in such way as to leave no room for doubt as to the cause. In many *diseases*, the rationale is equally plain; and in these the symptoms of poisoning, though often widely diffused over the system, show a predominating tendency towards certain parts. Thus syphilis, manifesting itself only in those who have been tainted with its peculiar poison, affects especially the eye, bones, joints, mucous membranes, and skin. The exanthemata—glanders—the severe constitutional symptoms following wounds received in dissection—pyohæmia, with its secondary deposits, chiefly in the lungs and liver—puerperal fever—the rheumatic diathesis—continued fever, with its ulceration of the mucous membrane and intestinal glandulæ—and intermittent and mild remittent, with their frequently associated affections of the liver, spleen, lungs, brain, and heart, and of the serous, mucous, and areolar tissues,—are all examples of the effects consequent on the presence of some poisonous matter; and differing, as they do, in their several details, which are ably and concisely sketched by Dr. Madden, yet exhibit "so many features in common, as to show that they spring from the same stock."

> "Fever, we find, is almost universally present, though in varying degrees, and of diversified character: the body is emaciated, the digestive functions are more or less deranged; there are pains in the limbs; the skin is seldom natural in appearance, and its functions are perverted; the pulse is small; the whole system weakened; the secretions and excretions depart more or less widely from their normal characteristics. And then we have organic changes in many organs: the eye is often affected; the larynx inflamed or ulcerated; pustules or abscesses form in the integuments, and the subjacent areolar tissue; the brain is not uncommonly diseased; the bones, and particularly the joints, suffer severely; the mucous membranes are greatly influenced; while the important depurating viscera—the kidneys, the liver, and the lungs—are, in a peculiar manner, liable to destructive alterations." (p. 95.)

And then Dr. Madden proceeds to show the existence of a close analogy with these, in the phenomena of scrofula. He traces the analogous *functional* disturbances,—in the fever, so strikingly resembling that produced by the paludal poison, the emaciation, indigestion, pains in the limbs, state of skin, the pulse, debility, and depraved or defective secretions and excretions, of scrofula and phthisis; while the correspondencies between the *organic* or structural changes, are evidenced by scrofulous ophthalmia, ulcerations of the larynx and trachea, glandular swellings and subcutaneous abscesses, caries and articular disease, affections within the cranium, ulcerations of the small and large intestines, fatty states of the liver, and degeneration of the kidneys, besides the well-known changes in the grand centre of attraction for tuberculous manifestations—the lungs. Some of these lesions are more strikingly significant than others. For instance, the "lardaceous liver" described by Rokitansky in patients much wasted by scrofula, is precisely analogous to, if not identical with, the change in this organ noticed by Dr. Graves in those whose health had been broken down from the combined influence of mercury and syphilis,—in other words, under the operation of a double poison. Fatty liver, again, occurs chiefly in phthisical cases; and for the rest, probably, in those only who are the subjects of some blood-disease. Thus, Dr. Budd found it in a man who died of extensive cancerous ulceration of the groins, there being no carcinomatous deposit in the liver. A general review of these resemblances between the two series of morbid actions, tends powerfully to suggest a common order of causation, and to lead to the conclusion "that scrofula, with its highest and most formidable manifestation—phthisis, is as much a poison-disease as any of those others with which we have been concerned."

The argument thus based on the analogies which we have hastily enumerated, Dr. Madden fortifies by considerations derived from such facts as,—that there are many cases in which fever, emaciation, and exhaustion occur from other causes, showing extensive disorder of the nutritive function, and yet the peculiar product of scrofula is not evolved; just as, of the thousands exposed to the exciting causes of cancer, a few only become affected with that disease;—that the morbid product is everywhere the same;—that tubercle is often *coetaneously* deposited in many parts of the body, as is evidenced by its being found there in precisely the same stage of progress; and by the appearance of a considerable analogy between the principles governing the selection of particular organs for the manifestations of scrofula, and those on which the local changes in poison-diseases depend, and which evidently resolve themselves chiefly, if not wholly, into an attraction between the morbid poison itself and particular organs and tissues, this attraction being, for the most part, expressive of a tendency in the system to free itself from the poison, by the channel which those tissues or organs afford. Having, so lately as in our Third Number, entered into a discussion of the *modus operandi* of poisonous agents introduced into the body *ab extra*, including a consideration of the ways in which the tendency just mentioned is manifested, and shown the application of facts and views connected with artificial poisoning to "poison-diseases," in an article from which Dr. Madden has done us the honour to draw his principal illustrations of this point, we need not again go into detail upon the subject; but we trust enough was then said materially to assist our readers in judging how far the doctrine of a distinct *materies morbi* in scrofula is borne out by those facts in its history, which seem analogous to the efforts at elimination manifested in cases of artificial poisoning, and in diseases arising from the undoubted agency of morbid poisons, whether introduced from without, or generated within the body.

Some of these facts, to which Dr. Madden alludes, afford, as it seems to us, strong corroboration of the theory in question. Witness the prevailing and often extensive affection of the great depurating organs of the body in phthisis, seen in the copious sweats, so like in their periodical occurrence to the sweats of intermittent, and in some of their other characters to the abortive eliminatory efforts by perspiration in rheumatism; seen again in the diarrhœa, with deposit in the intestinal glandulæ, reminding us of an obviously analogous state in fever, which there is much reason to regard as eliminatory in its tendency.

Whether the fatty liver of phthisis can be considered in the same light, may be more doubtful. Dr. Madden seems to think it referable to an effort at the elimination, by the hepatic cells, of fat, which is in excess in the blood, by reason of absorption from the tissues generally, it having been previously deposited in these as the result of the defective elaboration induced by the presence of the specific poison. But we are not aware that there is evidence of such a general fatty transformation, or even of such abundant deposit of fat in the adipose tissue of the subjects of fatty liver, as would be required to substantiate this hypothesis. Is it not probable that, from mere defective assimilation, there may be an unusual disposition in the blood to the separation of this class of hydrocarbonaceous matters, the molecules of which are not in a state to play their appropriate part in the economy, and are therefore deposited at those points where the elimination of such compounds normally takes place, accumulating thus in the cells of the liver? And does not the large proportion of fatty matter in tubercle itself, and perhaps also the copious sebaceous excretion on the surface so frequently seen in phthisical patients, favour this idea, which is further supported by some analyses of the blood (e. g. those of M. Boudet), which show an increased proportion of fatty matter in the blood?

With respect to the deposits in the lungs, it is supposable that their predominant frequency here may be explained by assuming the *materies morbi*, generated "in the acts of assimilation, disintegration, and secretion, which occur in all parts of the body," to be brought in larger quantity, and more directly, by the blood of the venæ cavæ to the lungs, than by any other channel to other organs; and here to exert, in the process of elimination, its specific modifying influence on the nutrition of these organs. But it may be only that there is a special affinity between them and the poison of tubercular disease; an explanation which is favoured by the especial limitation of the disease to the apex, and we would add, by the fact that it is far more commonly found in both lungs than in one only, and that it manifests a still further agreement with poison-diseases, in the striking exactness of the symmetry often witnessed in its development on the two sides.

It may be worth while here to observe, by way of parenthesis, how completely the doctrine of special affinity, between elements circulating in the blood, and not only particular organs, but special parts of organs, so unequivocally attested by the facts of poison-cases, and remarkably illustrated by the experiments of Mr. Blake, described in the number of this Journal before referred to, sets aside the objection urged by Dr. Addison to a localization of the cause of tubercular disease in the blood,—viz. that if this were its seat, inflammation could not go on to cure in one part of the lungs, while consumption was in progress in another. Other objections to the humoral hypothesis brought by the same writer are still more easily disposed of.

There are yet three or four other points of analogy pointed out by Dr. Madden. The first of these is, that a period of latency, which evidently must be granted to tubercular disease under the hypothesis of its cause being in the blood, exists also for all poisons, though with great difference as to its extent; ranging from a duration so short as to be scarcely appreciable, to a period of many months, as in hydrophobia. Perhaps, in this respect, as in some others, gout affords the nearest parallel to scrofula. Another circumstance is, that "the rapidity with which a poison acts, and the violence of the symptoms produced, are greatly influenced by the state of the system at large; and that whatever depresses the vital energies, tends to exalt the efficacy of the poison." And not only this, but "a poison may be present in the body, and remain there without producing any appreciable effects, until favorable circumstances call it into action;" a circumstance well illustrated in many well-known cases of ague. Both of these facts are paralleled by corresponding phenomena in phthisis. Again, the accumulation of poisonous substances, or at least of their effects, around spots to which they have once been determined, and the controlling influence of previous damage, whether from mechanical injury or inflammation, over the choice of locality in which the operation of poisonous principles shall be manifested,—find precisely analogous phenomena in the history of scrofulous and tuberculous affections.

Such are the grounds derived from the analogies traceable between tubercular disease and those known to arise from morbid poisons, which induce Dr. Madden to conclude that both groups should be included within the same category. He replies to the first objection likely to be raised,—that the supposed poison has never been isolated,—by appealing to intermittent fever, typhus, and plague. He meets the difficulty presented by an acknowledged ignorance of its characters, by asserting the same ignorance with respect to the poison of syphilis, gonorrhœa, or smallpox; and if it be objected, that neither phthisis nor scrofula is contagious or infectious—a point about which, we may be permitted to say, there is some doubt—so neither is rheumatism, nor ague, nor lepra, nor cancer.

Dr. Madden has a couple of chapters devoted to diagnosis and treatment, of which, though they are both excellent summaries, we can only notice those points, in the latter, which have reference to the subject immediately before us. Herein, the application of the theory of consumption evolved in the preceding pages is shown to result in giving clearness and precision to therapeutical efforts, by pointing out in what direction we should work, and by giving an intelligent perception of the end at which we should aim. At the same time, the effects of treatment based on these views support in their turn the justness of the theory; just as the effects of remedies in other cases will often decide the question of the true nature of a disease. Considering phthisis as a poison disease, the indications, apart from those modifications demanded by accidental conditions, and the necessity of always meeting local evils as far as may be by local remedies, would be, first, to neutralize the poison, if that were possible; but, there being no known antidote (though the search for one should not be abandoned), the next thing is, to promote the elimination of the poison from the system. Now all experience proclaims the beneficial effects of a treatment in conformity with this *natural* indication. The advantage of full and frequent ablutions and frictions, which, apart from and beyond their direct tonic effect, operate by increasing the depurating action of the skin; the importance of a moderately warm and dry air, such a state of atmosphere as is most conducive to elimination both by the lungs and skin; the use of moderate exercise; the beneficial operation of mild mercurials and gentle laxatives; and lastly, the decided advantage to be gained from the exhibition of diuretics, but especially of alkalies and their carbonates, are all recognised by practical men, and are so many arguments in favour of the poison-theory. The effects of alkalies are more especially dwelt upon by Dr. Madden, apparently under an impression that they are less known and relied upon by practitioners than we believe to be the case; but further, in order to adduce the interesting and important testimony of Dr. Golding Bird, to which we have on more than one occasion already referred. And certainly, when we couple with this observation the ascertained excess of similar forms of organic matter in tubercle, and reflect upon the way in which they probably originate, and how much their presence in the blood may have to do with the development of the *materies morbi* itself, we should both be disposed, on the ground of the proposed theory, to expect benefit from this class of remedies, and, finding them useful, to see in that circumstance corroborative evidence of the truth of the theory. Lastly, it will be an indication more prominently derived from this view of the nature of phthisis than from many others, to support the system at large; for this is necessary in all diseases arising from morbid poisons, and in none of these may vigorous antiphlogistic treatment be employed without great peril. The importance of following out this indication in phthisis, and generally in scrofula, is now pretty generally recognised; and it is in conformity with this, that good diet, and chalybeates, and vegetable bitters, and iodine, and cod-liver oil, though tending to this general end by different modes of action, and through different intermediate effects, are all found beneficial.

From the manner in which we have laid before our readers the arguments of Dr. Madden, it will be sufficiently plain that we are strongly disposed to acquiesce in his conclusions; at the same time that we think he has sustained his argument with great skill and clearness. It is certainly not the first time that a general notion of the dependence of scrofula upon some principle in the blood, such as we now denominate a poison, has been proposed. Mr. Phillips distinctly refers the diathesis to the accumulation of certain morbid materials in the blood; and the affinities of scrofula with some of those diseases which originate in the formation of a poison within the system, especially with gout, as well as its striking analogies with syphilis, have been often dwelt upon. But we have not before met with so definite an expression of this general notion, nor with so able an induction of facts in support of it. It must, we think, be evident, that only by some theory which assigns to the blood the fundamental change, can all the various manifestations of scrofulous disease be explained. And in going back to a humoral pathology of these diseases, and thus obeying the general tendency of modern views, Dr. Madden illustrates also the superiority of modern humoralism to the ancient, by basing his investigation on well-observed facts, and substituting close analogies for mere fancied resemblances, and thus arriving at a precise expression of the *kind of change* which may be presumed to take place in the blood, and to lie at the very bottom of all the outward manifestations of scrofula.

It may be said, doubtless, that we have still to ascertain what the new element is, and how generated. But it will be much to have settled the point, by a large induction of analogies, that we have as good reason to believe in the existence of some poisonous principle as the essential condition of scrofula, as to assume the same for gout or rheumatism, for ague or continued fever. That changes, too subtle even for the refined processes of modern chemistry to detect, do verily pass upon the blood, none can doubt who adopt the humoral hypothesis for diseases which most obviously of all demand it;—and perhaps nothing more strikingly exemplifies the immense effect, upon the functions of the whole body, of changes which nothing whatever may be able to verify except their effects, than that inscrutable alteration in the blood which remains after the poison of variola or scarlatina has wrought all its visible consequences, and which is sufficient to protect that fluid, for an indefinite period, from a repetition of primary changes which blood in its normal state undergoes in presence of those morbid poisons. Nor need we altogether despair of success, as methods of investigation become more and more delicate, in further attempts at discovering, more absolutely, the nature of the presumed noxious principle. Hitherto we have arrived at no certain knowledge with respect to the *materies morbi* in several diseases, the phenomena and analogies which have nevertheless compelled a general acquiescence as to the existence of such an agent. We are not *quite* sure of what it is, either in gout or rheumatism; still less definite are our conceptions of the nature of the lepra-poison, or of that of psoriasis. We allude to these, rather than to zymotic diseases and the syphilitic poison, because, though the phenomena of scrofula exhibit, in many points, a parallel to both groups of poison-diseases—to those in which the morbid element is introduced *ab extra*, as well as to those where it is generated within the system,—it seems to us that its affinities are, on the whole, most evident with the latter. And as we have been already led by a study of the manner in which some of these are developed, and by a comparison of all the observable changes in them, to *probable* conclusions respecting the nature of their essential morbid elements, so may we hope to arrive at something more definite concerning the others.

In suggesting it as more probable that the poison of scrofula is generated within the system, where it may go on to be removed continually, than introduced into it from without, we are principally influenced by the general indefiniteness of the course and duration of the disease, and by the very remarkable correspondence between the diathesis of rheumatism and that of scrofula,—and by the amount of evidence we have, bearing on the production of the latter, and thereafter of the ulterior manifestations of the disease, by external conditions.

Our space will not allow us to pursue this subject any further; though there are points relating both to the general question of the existence of the poison, and to its probable source, on which we should have liked to add a few remarks. For the present, however, we must leave both of these; not without a hope that Dr. Madden may, by some future extension of the investigation on which he has so promisingly broken ground, tempt us to a further discussion of them.

The Appendix to Dr. Madden's work contains some very valuable information on the Climate of Torquay, based upon the tables published by Mr. Vivian, and upon the meteorological journal accurately kept by the same gentleman. In these are shown the mean temperature of Torquay during the different seasons of the year, its mean extreme range, and mean daily range; a comparative view of the temperature and humidity of Torquay, and in England generally; as well as a comparison of the humidity of Torquay with that of other places resorted to by pulmonary invalids; —the general result of which is to show the great equability of the Torquay climate, both as to temperature and humidity, and to correct the prevailing notion, to which the sanction of Sir James Clark's authority has given additional currency, that Torquay is, from the humidity of its atmosphere, an unfavorable place of residence for those patients, of languid constitution, who have a congested bronchial membrane, with copious expectoration.

We have reserved for comparison with Dr. Madden's tables the results of Dr. Martin's observations upon the climate of the "Undercliff." These observations have been taken regularly at 9 a.m., during the space of ten years (from December 1838, to December 1848), with the omission of only one day; and in the mode in which they have been recorded and digested, as well as in the care taken to secure to the instruments such an exposure as should cause them to give the fairest indications, we consider that they may be held up as models to such as are disposed to carry out a corresponding series in other localities. It is to be regretted that no similar information is given us by Dr. Madden respecting the conditions under which Mr. Vivian's observations have been made; and that we are therefore unable to say how far the two series afford the data for a fair comparison.

The *mean annual temperature* of the Undercliff, deduced from ten years of observation, is 51°·72; that of Torquay, from the four years between 1842 and 1845 inclusive, is 52°·1. These years, by referring to Dr. Martin's table, we find to present a fair average. If the cold year 1840, or the hot year 1846, had been included, the mean deduced from the shorter term would have been unduly low or unduly high.

The *mean temperature of the seasons*, at the Undercliff and at Torquay respectively, is as follows:

	Winter.	Spring.	Summer.	Autumn.
Undercliff . . .	41·80	49·82	61·31	53·95
Torquay	44·0	50·0	61 2	53·1

From this it appears, that, in regard to *winter temperature*, there is a slight advantage on the side of Torquay, if the observations were made under parallel conditions. The *range of variation*, however, is a more important element in the character of climate than the actual mean; and as to this there appears to be very little difference between the two localities.

	Annual.	Winter.	Spring.	Summer.	Autumn.
Undercliff	50·6	26·0	42·8	31·3	41·2
Torquay	51	29	42	30	38

For the sake of comparing these data with similar ones, obtained from other sources, we subjoin the results of corresponding observations made in London:

	Annual.	Winter.	Spring.	Summer.	Autumn.
Mean temperature . .	50·3	39·1	48·7	62·3	51·3
Mean extreme range .	64	32	46	48	48

The *mean daily range* of temperature at the Undercliff and at Torquay appears to be nearly the same, so far as we may judge from a comparison of the average of ten years, given by Dr. Martin, with the results of observations during the year 1847 only, which are given by Dr. Madden. Both have a *considerably less* daily range during the winter months than Madeira presents; a point of no inconsiderable importance.

We may pass over the barometric observations given in equal detail by Dr. Martin, but not comprehended in Dr. Madden's Appendix, with the remark that they do not present any peculiarity on which we need stop to dwell; the *average* at the Undercliff being nearly the same with that of London, and the *range* rather less.

With respect to the *hygrometric* observations recorded by our two authors, it is to be regretted that they do not afford data for more exact comparison. Dr. Martin very justly points out, that no sufficient indication as to the humidity of a climate can be drawn from any observation of the dew-point taken only once a day, which is the usual method. He gives the following as the results of observations taken during seven years at 9 a. m. and 6 p. m., the hours which seem to give the nearest approach to the mean, during the winter months more especially; and with these we bring into comparison those averages of observations made at Torquay and at Clifton, which were taken, we believe, only once a day, namely, at 9 a. m.

	Annual.	Winter.	Spring.	Summer.	Autumn.
Undercliff . .	46·28	37·28	43·41	56·07	48·35
Torquay . . .	45·9	39·4	47·4	53·6	45·3
Bristol . . .	48·6	38·7	51·5	57·7	46·5

From these observations it would seem, that the *absolute* dryness (as shown by the depression of the dew-point) is greater at Torquay and the Undercliff, for the year, than it is in Bristol; that in winter there is but little difference between the three places in this respect; that in spring the advantage is great on the side of the Undercliff, but that Torquay is still much drier than Bristol; whilst, in the summer and autumn, Torquay is considerably drier than the Undercliff, which is moister than Bristol in the autumn quarter. These differences manifest themselves more strongly when we bring the mean temperature into account, so as, by taking the differences between the dew-point and the temperature, to show the *sensible* dryness, or *amount of evaporation*, at each period. In the following table, this is given by Dr. Martin from two observations, as before; whilst, in the other cases, we believe that only one is relied on.

	Annual.	Winter.	Spring.	Summer.	Autumn.
Undercliff . . .	5·9	5·4	5·6	6·6	6·1
Torquay . . .	6·8	4·1	8·0	6·3	4·2
Bristol	3·6	2·6	5·3	4·7	1·1

The inferences deducible from this table do not altogether correspond with those which the preceding would seem to warrant; but still they fully bear out the statement, that the atmosphere, both of the Undercliff and of Torquay, so far from being saturated with moisture, has a greater evaporating power than that of Clifton, which is usually regarded as rather a dry locality. And this seems borne out by the comparison of the *amount of rain* which falls at these three places respectively, as shown in the following table:

	Annual.	Winter.	Spring.	Summer.	Autumn.
Undercliff	25·94	6·61	4·48	5·67	9·18
Torquay	28·20	6·82	5·61	6·38	9·39
Bristol	32·56	8·43	5·69	9·44	9·00

So, again, with regard to the *number of days* on which rain falls,—a matter of as much importance as the actual amount of rain,—we find that both these spots present a favorable contrast to Bristol; Torquay, however, having a decided superiority.

	Annual.	Winter.	Spring.	Summer.	Autumn.
Undercliff	148	38	33	33	44
Torquay	132	35	30	32	35
Bristol	169	45	36	41	45

In regard to seclusion from northerly winds, both the Undercliff and Torquay possess this great advantage, that a very efficient shelter is afforded to them, without that excessive protection which becomes so injurious by obstructing the ventilation, and impeding the solar illumination of towns shut up within a narrow semicircle of hills. In both these localities, the invalid can enjoy long walks or rides, and encounter a considerable variety of scenery, without risk of severe exposure, even during the prevalence of northerly or north-easterly winds.

Dr. Martin assures us that the Undercliff is freer from sea-fogs than most places on the southern coast of England; and he believes that it has also an advantage, especially over the south coast of Devon, in regard to its electrical state,—an element in climate of which we as yet know too little, but of which we trust that much will speedily be learned. Dr. Madden has nothing to say upon either of these points.

It is obvious, then, that both these situations present great advantages to those who seek a change of climate on account of such pulmonary affections as can be removed or mitigated by a mild and tolerably dry atmosphere; and the profession is much indebted to Drs. Madden and Martin for thus presenting it with satisfactory materials for its judgment.

PART SECOND.

Bibliographical Notices.

ART. I.—*Elements of Chemistry, Theoretical and Practical; including the most recent Discoveries and Applications of the Science to Medicine and Pharmacy, to Agriculture, and to Manufactures.* By Sir RICHARD KANE, M.D., M.R.I.A., President of the Queen's College, Cork, &c. &c. Second Edition.—*Dublin,* 1849. 8vo, pp. 1070. Illustrated by 230 Woodcuts.

SIR ROBERT KANE's deservedly high reputation as a chemist, his intimate acquaintance with the *industrial* occupations of the Irish, and his unceasing efforts to develope the resources of his country, have tended to bring his 'Elements' into great public favour; and, as appears from the preface to this edition, his work has not only enjoyed considerable patronage in Great Britain and Ireland, but, under the editorial guidance of Professor Draper, of New York, it has been adopted in the Colleges of the United States.

The general plan and character of this treatise are peculiarly suited, in our opinion, to the wants of the chemical student; for while it is constructed so as to avoid most of the questionable and unproven hypotheses of modern chemists, it omits none of their important discoveries; it dwells much more upon general principles and doctrines, than upon the wearisome details with which most treatises are crowded; and it is written, moreover, in such an easy and familiar style, as to be adapted to the capacity of every reader of ordinary intelligence.

In his attempts to avoid the errors of modern hypotheses, however, the author sometimes commits faults of a serious nature. Thus, to take an instance or two, he treats the subject of heat as if the latter were a material agent; and he speaks therefore of quantities of it, of a special heat, of atmospheres of it, and he says that the production of it, whether it be by reason of friction, compression, or chemical action, is due to the condensation of matter, and to the evolution of specific heat. (p. 95.) So, also, he speaks of electricity as a fluid incorporated with matter; and he adopts this hypothesis, not because he believes in the actual existence of such a fluid, but because the supposition is ancient, and has become a part of our common creed. (pp. 138-269.) Magnetism, again, is spoken of (p. 185) as "a form of electricity," without any recognition of the views of those eminent physicists who regard it as not identical with any form of electricity, but as *correlated* to it, as it is also to heat. He evidently inclines to the idea that all these agents are *dynamical,* not in any sense *material;* and we could wish that he had spoken out his views more boldly, and had made the requisite changes in the phraseology he employs.

The radical theory is attacked by our author, as might be expected, with considerable energy; and he will not give it any more countenance than he is obliged to do. He censures it, not only on account of its involving so much speculation, but because, as he declares, the attempts which have been made by some eminent chemists to represent the composition of organized structures by means of formulæ, have tended to throw into confusion the most interesting questions of physiology and animal chemistry. (p. 661.) So that Sir Robert Kane is a follower in the path of Dumas, Laurent, Gerhardt, and Gmelin; and is disposed to adopt the theory of types, rather than the more popular doctrine of organic radicals. We cannot, therefore, but regard his work as a wholesome check upon the too rapid course of modern speculation; and we can recommend it to our readers as one of the best introductions extant to the science of which it treats.

We trust, however, that whenever it may reach another Edition, the Index will be found to have undergone considerable improvement. The author's fluent style and facile manner tend to hurry him right onwards, and thus not unfrequently lead to the postponement of topics of which some notice might have been expected, until some other and perhaps less appropriate opportunity. Hence a copious and well-arranged index is peculiarly requisite; and we are compelled to remark that the one which now terminates the volume is neither one nor the other, the references being meagre and the words frequently misplaced.

ART. II.—*Lectures on Medical Missions, delivered at the instance of the Edinburgh Medical Missionary Society.*—Edinb., 1849. 12mo, pp. 320.

IN our Third Number, for July 1848, when considering the general question of medical ethics, we briefly alluded to the subject of Medical Missions, and took the opportunity of commending them to the favorable notice of our readers. The appearance of the present volume recalls our attention to this truly important matter, and will tend, we trust, to excite a deeper and more active sympathy among the profession at large—more especially in the metropolis and other large cities, where young men are being educated for the future exercise of the healing art—in the great and good object that is sought to be accomplished. It contains a series of lectures which have recently been delivered before the medical students at Edinburgh, and a prefatory essay from the pen of Professor Alison, in which this distinguished physician points out, in a calm and philosophic spirit, the intimate connexion that should ever subsist between the pursuit of scientific knowledge, and the reception of the blessed truths of Christianity. The lectures are six in number, and stand thus: 1. An introductory one, explanatory of the series and recommendatory of the subject, by Mr. Miller, Professor of Surgery in the University. 2. On the importance of Medical Missions, by the Rev. W. Swan, lately missionary in Siberia. 3. On the qualifications of a medical missionary, by W. Browne, Esq., F.R.C.S., and President of the Medical Missionary Society. 4. On the duties of a medical missionary, by the Rev. J. Watson. 5. On the sacredness of medicine as a profession, by Dr. Wilson, F.R.S.E. 6. On the responsibilities attaching to the profession, by Dr. Coldstream, F.R.C.P.E. They will, each and all of them, repay the labour of a thoughtful perusal, and are, we think, exceedingly well fitted to promote the excellent cause which the Society, at whose instance they were delivered, has in view. The one by Professor Miller (it is also published separately, we believe) is especially deserving of praise for its eloquence and fervour; it should be read, and pondered too, by every medical man in the kingdom. May the good example that has thus been set by the Edinburgh school of medicine—to which so many of the present members of the profession look back with pleasure as their *alma mater*—be followed in other seminaries of professional instruction.

There cannot surely be a doubt but that as a body, we have not hitherto lived or acted up to the true and highest end of our vocation or mission. Our thoughts have been so much absorbed in the materialities of its pursuit, or in the ceaseless acquisition of its merely intellectual details, that its loftier and more spiritual bearings have well-nigh been overlooked by most of us. Perhaps, indeed, this charge might fairly be brought against every department of secular knowledge, as studied and carried out in the present day; they are all too much followed as an end, not as a means to a more remote and nobler one. How admirably has Bacon pointed out and rebuked this fault, when speaking of the impediments to the advancement of truth and science in his time:

> "But the greatest error of all the rest is the mistaking or misplacing of the last or furthest end of knowledge; for men have entered into a desire of learning and knowledge, sometimes upon a natural curiosity and inquisitive appetite; sometimes to entertain their minds with variety and delight; sometimes for ornament and reputation; and sometimes to enable them to victory of wit and contradiction; and most times for lucre and profession; and seldom sincerely to give a true account of their gift of reason to the benefit and use of men; as if there were sought in knowledge a couch, whereupon to rest a searching and restless spirit; or a terrace for a wandering and variable mind to walk up and down with a fair prospect; or a tower of state, for a proud mind to raise itself upon; or a fort or commanding ground for strife and contention; or a shop for profit and sale; and not a rich storehouse *for the glory of the Creator, and the relief of man's estate.*"

If such be the true end of all knowledge, how emphatically so must it be of that branch of it which professes to teach us how to soothe the sufferings and heal the diseases of our fellow-creatures! The physician, indeed, who takes the Bible as the oracle of truth and the guide of his life, cannot but feel the sacredness of his calling, and the deep responsibilities, but no less lofty privileges, which it involves. And is there not something very touching in the fact that the Saviour himself appeals to his works of healing as one of the signs and evidence of His Messiahship? "Go and show John again those things which ye do hear and see: the blind receive their sight, and the lame walk, the lepers are cleansed, and the deaf hear, the dead are raised up, and the poor have the gospel preached to them." When, too, he sent forth his disciples to preach the gospel, we read that he commissioned them also to "heal the sick, to cleanse the lepers, to raise the dead, and to cast out devils;" adding the admonitory injunction,—"freely ye have received, freely give." Do not these things imply a hallowed union between the office of relieving human suffering and that of evangelistic teaching? And is there not therefore reason for believing that if the "healing element," so to speak, in missionary enterprise had been more earnestly and steadfastly worked out than it has ever yet been since the apostolic age, there might have resulted a larger amount of success in the diffusion of the gospel in barbarous and heathen lands, than has hitherto been gained? Indeed, the good that has already been done by the few medical missionaries who have gone forth within the last eight or ten years—and to America belongs the praise of having been foremost and most zealous in the work—fully warrants this belief, and affords, at the same time, the most gratifying evidence how much our profession has it in their power to promote the highest welfare of the human race. How can it be otherwise? We all know the amazing power of active disinterested philanthropy in disarming even the most obstinate of their prejudices, and in winning the confidence and esteem of our fellow-creatures. The human heart cannot resist the force of this talisman; do good—single-minded, unselfish, good,—and men are constrained to love you. Now this is the very weapon whereby it is sought to prepare the ground for the reception of the good seed of Christian instruction, in nations that are still lying in heathen ignorance and superstition. The medical missionary goes forth on his labour of mercy among the sick, the suffering, and the wretched, in lands where neither science has bestowed its light nor charity has conferred its blessing. He invites all to come to him for relief, without money, and without price. The poor, unused to sympathy, flock to him from all quarters; they have tried their own doctors, and probably besought their own gods, but without avail; he heals or relieves them; their affection is won by the good he has done them or their friends; they regard him as a benefactor, and look up to him as a friend; he invites them to join with him in praise and prayer to their common Father in heaven, from whom cometh down every good gift, and to whom he ascribes all the glory; their hearts, warmed with gratitude, respond to the call; the first gleams of divine truth dawn upon their souls; they feel the wish to know more; his advice directs and encourages them; they have the gospel preached to them; and thus they are led on from step to step, until at length they are brought to the saving knowledge of their Redeemer, and become missionaries themselves, in their own humble sphere, and to their own families and kindred, of the glad tidings of great joy which He came into the world to proclaim. This is certainly a glorious field for exertion; it rests, in a great measure, with medical men, whether it be rightly and efficiently occupied. Our privilege, as well as our duty, is great; we can all aid the good work in some way or another; to engage in it will dignify and exalt our profession; the object is an ennobling one; the present time is most favorable, the reward is sure; for beneficence like mercy, is ever

> "twice blessed,
> It blesseth him who gives, and him who takes."

Art. III.—*Letters on the Truths contained in Popular Superstitions.* By Herbert Mayo, m.d.—*Frankfort,* 1849. Post 8vo, pp. 152.

These letters, which originally appeared in 'Blackwood's Magazine' for 1847, profess to examine and elucidate the reputed phenomena of "the divining rod, vampyrism, the belief in ghosts and dreams, second-sight, supposed workings of the holy spirit on masses, possession by devils (in the middle ages and subsequently), and witchcraft." And the author claims to have established as truths, "the od force, the law of sensorial illusions, the laws of trance."

The value of any production of this kind entirely depends upon the care taken in the collection of evidence, on the penetration employed in sifting the details of such as is really genuine upon the whole, and on the philosophical character of the inferences founded upon it. Now we cannot accord to Dr. H. Mayo the credit of success in any one of these particulars. He has brought together a number of stories, old and new, many of which bear pretty obvious marks of having been either invented or improved upon by clever dealers in fiction. In discussing those which have the most claim to be considered as honest records of the impressions of competent witnesses, he has shown but little acquaintance with the mode in which such narrations are affected by the preconceived impressions of the parties, nor has he adverted to many other sources of fallacy which the discriminating reader will easily discover. And in drawing deductions from the evidence he has thus collected, he excludes many considerations which must enter into a really comprehensive and philosophical view of them.

The little book, however, will form a very amusing companion for an hour of leisure, and will afford some interesting suggestions to those who take an interest in following out the various questions discussed in it.

Art. IV.—1. *The Hunterian Oration delivered before the Royal College of Surgeons of England, on the 14th of February,* 1849. By Cæsar H. Hawkins, Surgeon to St. George's Hospital.—*London,* 1849. 8vo, pp. 35.

2. *The Retrospective Address on Diseases of the Chest, delivered at the Fifteenth Anniversary Meeting of the Provincial Medical and Surgical Association, held at Derby,* 1847. By Edward James Shearman, m.d. —*Worcester,* 1848. 8vo, pp. 47.. With Four Plates.

In choosing the principal topic for his Hunterian Oration—a selection which to almost every one now called to discharge that duty must be a matter of some perplexity—Mr. Hawkins followed the example of his predecessor, Mr. Grainger, in bringing before his audience some of the more interesting examples of recent progress, rather than again traversing the ground which has already been so frequently trodden; and he specially directs attention to the improvements in physiology, organic chemistry, and histology, which tend to elucidate the history of diseases, and to suggest means for their cure. Among these, he does not omit to notice in a very appropriate manner the revival of the humoral pathology; the mention of which, in the Theatre of the Royal College of Surgeons, by a distinguished Fellow, is a pleasing indication that the days of those *pure* surgeons, who thank their Maker that they know nothing of physic, are passed away. Mr. Hawkins holds up the system of cooperation which exists among the cultivators of the physical sciences, and from which such great results are being obtained in regard to terrestrial magnetism, tidal phenomena, and the like, as a useful example to the medical profession.

"Would not the stimulus of some such concert and controlling power as I have alluded to in others, advance medical science also, more steadily, step by step in the right direction, and at the proper time, through the combined efforts of many now isolated labourers? Would it not check many crude and ill-digested hypotheses, prevent the revival of disproved facts and exploded theories, and give a higher value to original discoveries?" (p. 33.)

There can be but one answer to this question; nor can there be a doubt that any such plan ought to originate with the existing corporate bodies, which would vastly increase their claims to the esteem of the profession, did they bring forward any measures calculated to elevate its character, without consideration of their separate interests. Will Mr. Hawkins endeavour to arrange the details of such a plan, and to bring them before his own College?

Dr. Shearman's 'Retrospective Address' has only just reached us, although it seems to have been some time published; and we can do no more than mention that it gives an admirable digest of the recent additions to our knowledge of the diagnosis, nature, and treatment of diseases of the chest, brought down to the year of its delivery (1847); and that it is illustrated by several beautiful delineations, chiefly from the pencil of Dr. Branson, of Sheffield, of the pulmonary structures in health and disease.

ART. V.—1. *The Pathology of the Human Eye.* By JOHN DALRYMPLE, F.R.C.S. Fasciculus II. Small folio. Four Plates.

2. *Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fasciculus V. Large folio. Four Plates.

3. *Surgical Anatomy.* By JOSEPH MACLISE. Fasciculus III. Large folio. Four Plates.

OF this unrivalled triad, it is now superfluous to speak in words of commendation; and we might content ourselves with simply announcing the publication of a new Fasciculus of each of the works composing it.

We owe it to Mr. Dalrymple, however, to give a somewhat fuller account of the plan and scope of his undertaking, than we were able to do in our former notice. These are scarcely expressed by his title; for he tells us that "it is not within his meaning to publish *a treatise* on diseases of the eye, but the rather to exhibit them as they occur in nature, in a series of drawings, with such explanations as shall identify them with symptoms, and with the general treatment of the case." In fact, Mr. Dalrymple's purpose is, in the main, analogous to that of Mr. Erasmus Wilson, in his 'Portraits of Diseases of the Skin.' It is not intended, however, to exhibit *all* the diseases of the eye: "many offer such inconspicuous appearances (as in the large class of amaurotic affections), that drawings would scarcely convey any useful information, while others are either too familiar to surgeons to require delineation, or too rare to afford useful or practical assistance." The subjects are selected from a series of many hundred drawings, made at the Ophthalmic Hospital by the best artists (chiefly by Mr. W. H. Kearny and Mr. S. Leonard), under the superintendence of the late Mr. John Scott and of the author; and the utmost pains have, in every instance, been taken to verify their correctness by frequent comparison with existing specimens of disease. To each figure are devoted a few lines of explanation, with a very brief history of the disease it illustrates, and a general outline of the treatment appropriate in such cases. Thus, when complete, it will constitute a sort of "museum of collected cases, showing in various groups not only the active forms of acute diseases, but their terminations or results also;" and will thus form a most valuable accompaniment to any systematic treatise on diseases of the eye.

The second Fasciculus now before us is quite equal to its predecessor in artistic beauty, and in lifelike accuracy of delineation and colouring; and having already said of the first, that "it realizes all that we believe it possible for Art to effect in imitation of Nature," we do not see that we have any higher praise in our power to bestow. The subjects illustrated are Warts and Horns on the Eyelids, Syphilitic and Malignant Ulceration of the Eyelids, Diseases of the Lachrymal Apparatus, Rupia prominens, Phthiriasis (the pediculus ciliorum being identical with that of the pubes, not with that of the scalp), Ecchymosis, Œdema of the Eyelids, specimens of the two principal types of Vascularity in Diseases of the Eye, and Scrofulous Ophthalmia. This last, to which a whole plate is devoted, in order to bring in the entire head of the subject, is a most painfully characteristic representation of one of the worst forms of this most intractable disorder.

Of Mr. Wilson's 'Portraits,' the first delineates a case of *Kelis*, a very remarkable and rare affection, consisting of an excrescence that presents very much the appearance of the cicatrix of a burn; the second is denominated *Xeroderma icthyoides*, better known as Icthyosis simplex; the third represents *Lepra vulgaris*, otherwise known as Psoriasis scutulata; and the fourth depicts *Psoriasis vulgaris*, about whose synonymy there is fortunately no trouble. Of the last three, we can affirm that they are most characteristic and admirably-executed portraits; of the first, as we have never seen a case of the kind, we can only remark that it has every appearance of being the same.

Mr. Maclise gives us, in his first plate, three figures illustrating the Surgical Dissection of the Wrist and Hand; in his second, two figures representing the relative position of the Cranial, Nasal, Oral, and Pharyngeal Cavities; in his third, the relative position of the Superficial Organs of the Thorax and Abdomen (which would have been more truly seen if the shoulders had not been dragged up by the tying of the wrists above the head); and in his fourth, the relative position of the deeper Organs of the Thorax and Abdomen. The plates are all excellent; and we are happy to find that our exhortations have been so well taken by Mr. Maclise, that there is not a single line in their accompanying commentary to which we can object as transcendental.

ART. VI.—*Some Thoughts on Natural Theology, suggested by a Work, entitled "Vestiges of the Natural History of Creation."*—*London*, 1849. 8vo, pp. 246.

THE writer of these "Thoughts" seems anxious to shield the author of the "Vestiges," for whom he appears to have an enthusiastic admiration—ranking him with Newton, Laplace, and Cuvier, as men who "show what the reason of man is capable of, and exalt our conceptions of the dignity of human nature"—from the *odium theologicum* which has been so copiously poured upon him. In particular, he desires to show that the doctrine of "development," as expounded in that work, has no irreligious tendency. Upon this point we are quite in accordance with him; our objection to it being, that it is destitute of an adequate scientific basis. We are disposed to think that all parties will ultimately meet in the belief that every natural phenomenon is the direct and immediate result of the Will of the Deity, acting according to a determinate plan; the original perfection of which was such as to need no subsequent interference, the universe having undergone its grand and harmonious evolution, without any departure from the original scheme, and being still as dependent as ever upon the sustaining power of its Creator.

ART. VII.—*Questions and Observations in Hygiene: recommended to the consideration of Naval Medical Men; suggested to the mind of the Author by the approach of the Asiatic Cholera.* By FRED. JAMES BROWN, M.D. Lond., &c., Assistant-Surgeon R.N.—*London*, 1849. Post 8vo, pp. 64.

THESE very useful suggestions are grouped together under the following heads:—I. Condition of the sick-berth, and round-houses or necessaries. II. Cleanliness, washing, and airing of clothes, &c. III. General clothing, ventilation. IV. Articles of food, water. V. Use of rum and tobacco; leave. VI. Use of fruit—Diarrhœa and Cholera—Sick-berth attendant. VII. Discipline. VIII. Religious and moral condition of the men. IX. Connexion of disease generally with the condition of the ship. —We earnestly recommend them to the attentive consideration of every medical officer in the navy. Our government marine ought surely to take the lead in the promotion of hygienic improvement; and to afford a model in this respect to the mercantile service.

ART. VIII.—*Portraits of Diseases of the Scalp, with the Safest and most Efficient Modes of Treatment.* By WALTER COOPER DENDY, Senior Surgeon to the Royal Infirmary for Children, &c. &c. Fasciculus I.—*London*, 1849. 4to. Four Plates.

WE believe Mr. Dendy to have made good use of the extensive opportunities which his position has afforded him for the study of the diseases of the skin which are most frequent in children; and among these, as every practitioner knows, diseases of the scalp hold a conspicuous place. He has intended to present in this treatise (of which the second and concluding part is announced as nearly ready) "the merely practical view of the maladies, avoiding any minute microscopic delineations, and all formal disquisitions on their pathology;" and so far as his text is concerned, he has succeeded well. But as to his plates, alas! we cannot speak in the same terms of commendation. Had they been issued as *caricatures* instead of as *portraits*, we should have strongly recommended them as admirable provocatives of laughter, as we have observed them to be productive of this result in the case of every one who has inspected them. Mr. Dendy would certainly consult his credit by suppressing these delineations, and substituting a set of plates drawn by some professional artist more capable of doing justice to the subject.

Art. IX.—1. *Memoirs on the Ganglia and Nerves of the Uterus.* By Robert Lee, M.D., F.R.S., Lecturer on Midwifery at St. George's Hospital, &c. &c.—*London,* 1849. Small 4to, pp. 36. With Five Engravings.

2. *Practical Observations on the Diseases of the Uterus.* By Robert Lee, M.D., F.R.S., &c. The Coloured Illustrations from Original Drawings by Mr. Perry. Parts I and II.—*London,* 1849. Large 4to, Plates I-VIII; pp. 20.

The first of these publications is not precisely what its title imports, for the 'Memoirs' constitute but a very small part of it, the remainder consisting of a statement, by Dr. Lee, of all the transactions that have occurred in connexion with them between himself and the Royal Society, The medical public has, we suspect, had quite enough of the discussion; and we do not see that any good purpose can be gained by reopening it. We may state our decided opinion, however, that Dr. Lee, like Dr. Marshall Hall, has fair ground of complaint against the Society on account of some of its proceedings in connexion with his Memoirs; and we trust that the Physiological Committee, as now constituted, will take warning by the errors of that of preceding years, and avoid even the appearance of favoritism or jobbery. At the same time it can scarcely be supposed that Dr. Lee's history gives *both* sides of the case, and it would be wrong, therefore, to judge from it alone. After a careful perusal of it, we are confirmed in the justice of the statement which we have made in the early part of the present Number (p. 9), respecting the insufficiency of the microscopic evidence supplied by Dr. Lee; for although it is true that he did, in the year 1839, seek the assistance of Mr. Tomes, in 1840 that of Dr. Hake and Dr. Grant, and in 1841 that of Mr. Dalrymple, yet the first of these gentlemen confessed himself unqualified to give a satisfactory opinion respecting the structures in question; the second merely states that the fibres resemble those of other nerves; the third says that "the evidence from the microscope is very flimsy;" the fourth (whose opinion is entitled to every respect) did not examine nerves prepared by Dr. Lee, but specimens dissected out by himself; and not one of them says a word about the *ganglia.* At that time, moreover, the true characters of the "gelatinous" nerve-fibres were not understood by microscopists; and as we do not find that Dr. Lee shows himself at all aware of the progress of this branch of histological inquiry, we would earnestly counsel him, before he makes any further move in this matter, to seek the assistance of those who can give him an opinion respecting the nervous or non-nervous character of the structures he describes, which shall be satisfactory to the scientific world.

It is refreshing to meet Dr. Lee again in the field of "practical observation," in which he has already laboured to such good purpose; and we have great pleasure in bringing under the notice of our readers the work he has recently commenced, which promises to be of great value as a clinical history of the diseases of which it treats. The first fasciculus, which is devoted to malignant diseases of the uterus, records the principal features of forty-two cases, from which the author draws the following conclusions:

"From these cases it will be seen that the fungoid tumour of the uterus, or cauliflower excrescence, scirrhus, carcinoma, and corroding ulcer, are merely different forms of the same malignant disease; that the morbid changes may commence in the mucous and muscular coats of the fundus and body of the uterus, though they are observed most frequently to begin in the orifice and cervix. It may be inferred, also, from these histories, that inflammation of the uterus does not give rise to cancer in any form, and that the progress of the disease to a fatal termination is never arrested by those remedies which subdue inflammation." (p. 12.)

Of the plates illustrating this fasciculus, the first represents "a fœtus of seven months, with its placenta partly adherent by a band to a nævus occupying the scalp and dura mater," the case being recorded in the twenty-second volume of the 'Med.-Chir. Transactions.' What right this plate, beautiful and interesting as it is in itself, has to a place among the illustrations of malignant diseases of the uterus, we must confess ourselves at a loss to discover. The subjects of the other plates are highly appropriate, and are executed with Mr. Perry's well-known skill and fidelity; but they scarcely comprise all the most important forms of malignant disease.

The second fasciculus contains a similar account of fibrous tumours and polypi of the uterus, of which sixteen cases are recorded. The illustrations are admirable, and are so selected as to bring all the chief varieties of these diseases under the eye of the student.

ART. X.—*Lectures on the Parts concerned in Operations on the Eye, and on the Structure of the Retina, delivered at the Royal London Ophthalmic Hospital, June,* 1847*: to which are added, a Paper on the Vitreous Humour, and also a few Cases of Ophthalmic Disease.* By WILLIAM BOWMAN, F.R.S., F.R.C.S., Professor of Physiology and General and Morbid Anatomy in King's College, London.—*London,* 1849. 8vo, pp. 144. With Thirty-eight Wood-engravings.

HAVING been prevented, by a press of other matters, from giving such an account of these Lectures as their merits call for, we must postpone to a future occasion our notice of some of the most interesting points of which they treat, and must content ourselves with recommending them to our readers as a most valuable contribution to ophthalmological science. The chief novelty will be found in the account of the minute structure of the cornea, the ciliary muscle, the retina, and the vitreous humour; and in the examination which Mr. Bowman has made into the departures from the normal characters presented by various textures in a state of disease. We trust that his connexion with the Moorfields Hospital will afford him opportunities of carrying out this latter part of the investigation with his well-known industry and acumen.

ART. XI.—*A Treatise on the Diseases of the Heart and Great Vessels, and on the Affections which may be mistaken for them: comprising the Author's View of the Physiology of the Heart's Action and Sounds.* By J. HOPE, M.D., F.R.S., &c. &c. Fourth Edition.—*London,* 1849. Post 8vo, pp. 612.

THE merits of Dr. Hope's classical treatise are so well known to the profession, that we have no need to do more than notice the issue of this new edition: the peculiar merit of which consists in its being reduced in size and price, without any omission from the text except in the case of such controversial matter as is now comparatively unimportant; and with the addition of some notes and cases which the author left in Ms., and ordered to be inserted in the next edition. The plates, we are informed, have been most reluctantly omitted: their insertion would have added considerably to the expense of the work; and it has been thought preferable to sacrifice them to the higher object of usefulness, by bringing the book, at a reduced price, within the reach of a larger number of students. We trust that, in its present compact form, the work will gain that attention from the "rising generation" to which it has so well established its claim.

ART. XII.—*On the Cryptogamous Origin of Malarious and Epidemic Fevers.* By J. K. MITCHELL, A.M., M.D., Professor of Practical Medicine in the Jefferson Medical College, Philadelphia.—*Philadelphia,* 1849. Post 8vo, pp. 138.

IN this little work the author sustains the same hypothesis in regard to the Cryptogamic nature of fever poison, as was put forth by Dr. Cowdell in his work on the 'Asiatic Cholera,' reviewed in our Third Number. It is obvious, however, that the author has not been in the least indebted to Dr. Cowdell, either for the idea itself, or for the arguments by which he endeavours to establish it; but that he has for some years been in the habit of propounding the doctrine to his class and to his scientific friends. After a careful study of his production, we feel bound to say that he has made out a very strong case; and that the arguments adduced by Dr. Cowdell and Dr. Mitchell, from the study of independent diseases, mutually tend to confirm each other. We are glad to find from Dr. Mitchell's Preface, that "experiments are in progress, which seem to promise more direct and unquestionable proof of the validity of the hypothesis;" and we trust that they may lead to some satisfactory result. The author advances some very cogent objections to the theory of *ferments*, as the agents in producing what it is now the fashion to call "zymotic" diseases, which we deem worthy of attentive consideration.

ART. XIII.—*The Training Institutions for Nurses, and the Workhouses: an attempt to solve one of the Social Problems of the present day.* By EDWARD SIEVEKING, M.D., Physician to the Northern Dispensary, &c. &c.—*London,* 1849. 8vo, pp. 22.

THERE cannot be two opinions amongst Medical Practitioners, as to the value of well-qualified and faithful attendants for the sick. How often do the discrimination and watchfulness of an intelligent nurse make all the difference between a favorable and a fatal termination of a dangerous malady! How constantly does judicious kindness mitigate unavoidable suffering; and what a source of tranquillity there is to the patient in the very tone and aspect of a gentle and devoted watcher! On the other hand, how frequently is the most grievous injury done by the neglect and presumption of the careless and ignorant; and how much does any manifestation of impatience, carelessness, or irritability, add to the sufferings of the invalid! The wealthy often find it difficult to secure such services as they desire; the poor must be in great measure deprived of them, except when human sympathy supplies that assistance which they have no worldly means of obtaining. It is not sufficient, however, to make a good nurse, that she be specially trained for the purpose. The requisite *knowledge* may be imparted; but who, save the individual herself, can furnish that *disposition* which is a not less essential qualification? Moreover, our experience of the uneducated classes leads to the conclusion that, with a few rare exceptions, such a general education as tends to call forth the intelligence and to cherish the moral feelings, is an indispensable foundation for the character of a superior nurse. All who have had opportunities of watching the "Sœurs de Charité" in their self-devoted ministrations, will, we are confident, agree with us in attaching the highest value to such primary qualifications.

We are glad of any attempt to excite attention to the want under which this country certainly labours to a greater degree than most others; and we trust that the subject will not be allowed to drop. Dr. Sieveking's pamphlet contains some suggestions which are worthy of consideration; but the plan he proposes is only adapted, in our estimation, to meet one part of the difficulty,—that of providing the needful assistance for the poor, by educating for the object a portion of the inmates of workhouses. We hope to be soon again called on, by the promulgation of some more complete scheme, to draw the attention of our readers to this matter.

ART. XIV.—*On the Science of those Proportions by which the Human Head and Countenance, as represented in Works of Ancient Greek Art, are distinguished from those of Ordinary Nature.* By D. R. HAY, F.R.S.E., Author of 'First Principles of Symmetrical Beauty,' &c. &c. With Twenty-five Plates.—*Edinburgh*, 1849. 4to, pp. 80.

THERE are few subjects as to which the public mind in this country has made more progress during the last twenty years, than it has in the love of art and in the appreciation of beauty. And with this improved cultivation of the practical, there has been a corresponding advance towards a more correct estimate of the fundamental principles which constitute the theoretical portion of æsthetics. We no longer find it doubted that the human mind has an innate sense of beauty, as it has of truth and of goodness; and the notion is rapidly gaining ground, that no system of education can be complete, which does not include, with the cultivation of the intellectual faculties and the moral sense, the development of that sense of beauty which the Creator has implanted in our minds for wise and benevolent purposes, and which may not merely be rendered capable of ministering in innumerable ways to the enjoyments of ordinary life, but is connected also with its higher aims, imparting to truth its most attractive brightness, to goodness its greatest loveliness. Nor do we now find it denied that this sense can be improved by cultivation; or that, except in a few rare cases, its scope must be very limited, its indications very imperfect, unless it has been submitted to a regular discipline. On the contrary, in most departments of art it has come to be admitted that there are certain fundamental principles which may not be disregarded in any attempt to gratify the cultivated taste for beauty; so that, although the most rigid observance of rules cannot make an artist, no one can be a real artist who works in systematic violation of them. "By an implicit obedience to the application of these definite rules," says our author, "genius has clothed the creations of fancy in poetic numbers, or given form and expression to the most exquisite combinations of musical sound, thereby charming the ear and entrancing the soul of the most intelligent and critical student of art, through that inherent principle in the human mind that responds in unison to every species of harmony."

Although this has long been conceded, however, with regard to poetry and music, yet it has been too much the fashion to assume that the *formative* arts are not amenable to any well-defined laws, whose bounds shall guide their development, or supply safe canons for the critic's guidance. There would seem no *a priori* reason, however, why such should be the case—why the combinations of colour and proportion, which are pleasing to the visual sense, and through it awaken the sense of beauty in the mind, should not be reducible, to a certain extent at least, to fixed rules, just as the combinations of sound which are pleasing to the ear are found to be accordant with rules deduced from the physical conditions under which those sounds are produced. It is probably known to most of our readers that Mr. Hay has perseveringly devoted himself to the search for these principles, and that his labours have been attended with a large measure of success,—his works on 'Harmony of Colouring' and the 'First Principles of Symmetrical Beauty' having gained for him a high reputation, and having contributed to diffuse a belief in the existence of fundamental principles in the formative arts, even if it be thought that we are as yet only at the entrance of the path which may at last conduct us to them.

In the present treatise he attempts to show that the proportions of those ideal representations of the human head and countenance, in which the ancient Greek sculptors so much excelled that their works have become standards to all future time, are mutually connected with each other, and may be reduced to certain simple numerical and geometric relations. From the works of Plato, and from various historical data, he adduces strong grounds for the belief that some such theory had been constructed by the Greeks themselves, so that from the simple laws of numerical proportion and geometrical construction they formed a standard of perfectly symmetrical beauty, to which all their representations presented a certain conformity, however they might be modified to express the various phases of divinity or humanity. We fully agree with Mr. Hay in the correctness of the distinction between the beauty of *form* and the beauty of *expression*, and believe that no one who is not blinded by devotion to a theory could fail to perceive it. The one is purely æsthetic, and may be attained, to a certain limited extent, by observance of rules for its production; the other appeals to our emotional consciousness, and through it to the intellect. The one is independent of all association, and appeals simply to our sense of abstract harmony of form; whilst the other is intimately connected, through the associative principle, with the highest enjoyments that we can derive from intercourse with our kind. The two may coexist in the highest degree, or may be blended in various proportions; but either may exist without the other. We may have the most perfect symmetrical beauty with an entire absence of expressiveness; a most charming expression without the least approach to symmetry.

We could not attempt, without the aid of diagrams, to convey an idea of Mr. Hay's views with respect to the symmetry of the human head. It must suffice to say that his ideal head is made up by the combination of a sphere and an ellipse, having certain proportions to each other, the former giving the outline of the cranial portion, and of the upper part of the face; whilst the latter gives the outline of the lower and anterior portion. This he illustrates by well-arranged diagrams, which show that such a combination is accordant with reality in each of the three aspects in which the skull may be viewed, namely, the front, or facial, the lateral, and the basal. And he then goes on to demonstrate how, by a variation in the angles of the triangles which constitute the foundation of his construction, an infinite variety of gradations may be produced, which correspond with the various modifications presented to us in the works of the Greeks; whilst by a further departure from the fundamental type, in the inclination of the long axis of the ellipse, the elevated character of the physiognomy is made to give place to one of greater or less degradation, in which, however, the rules for the determination of the place of the features still hold good. The triangles on which the construction of most perfect symmetry is based, are those which Plato regarded as the most perfect in nature, being the right-angled isosceles triangle, which forms the half of a square; and the right-angled scalene triangle, which forms half of an equilateral triangle. The numerical proportions between the angles of these triangles are extremely simple, and correspond exactly with those which regulate the concords in music.

All these coincidences are certainly extremely curious; and the scheme which Mr. Hay has drawn up deserves the credit of great ingenuity, if nothing more. But we are firmly persuaded that there *is* something more, whether it is reserved for this generation to find it out or not; and that in art, as in science, there are fundamental truths, whose sublimity is only equalled by their simplicity, and which will reveal themselves in due time to its true votaries.

ART. XV.—*Essay on the Use of Alcoholic Liquors in Health and Disease.* By JOHN CHADWICK, M.D.—*London*, 1849. 12mo, pp. 124.

THE author of this Essay tells us that the subject of it having been continually before his attention, in the course of his professional practice, during many years, he has come to the conclusion that a vast amount of mental and physical evil has resulted from the use of alcoholic drinks; that by a large proportion of the profession, as well as by the public, the true mode of action of these drinks on the human body is not known, not being correctly explained in any book specially devoted to the subject; and that there is no safe conduct in regard to them, except their total disuse, save in their legitimate office as medicines.—In these views we heartily concur; and as Dr. Chadwick has advocated them with considerable ability and good sense, we have much pleasure in recommending his treatise to our readers.

We may add, as an indication of the public demand for "temperance literature" of a superior class to the stuff commonly circulated, that the "Scottish Temperance League," having reprinted the article entitled "Temperance and Teetotalism," from the last number of Dr. Forbes's Review (by permission of Dr. Forbes), has circulated the whole of an edition of *fifteen thousand*, and has lately issued a new edition of *ten thousand*, with the name of the author (Dr. Carpenter) attached, at the price of *three halfpence.*

PART THIRD.

Original Reports.

AN INQUIRY INTO THE BEARING OF THE EARLIEST CASES OF CHOLERA, WHICH OCCURRED IN LONDON DURING THE PRESENT EPIDEMIC,

ON THE STRICT THEORY OF CONTAGION.

By Edmund A. Parkes, M.D.,
ASSISTANT-PHYSICIAN TO UNIVERSITY COLLEGE HOSPITAL.

[The following Inquiry originated in a request made to Dr. Parkes, by the General Board of Health, that he should examine into the evidence which might be derived for or against the doctrine of Contagion, by an analysis of the early cases of cholera in London. It is here published, with the cases abridged, in the belief that the evidence brought forward in it will be acceptable to the profession, and will be found to have an important bearing on the great question at issue.]

I propose to give in this Report the chief facts which I have been able to collect, regarding the early cases of cholera in London; in order that some decision may be formed as to the mode in which these cases originated, whether from a poison emanating from the bodies of other persons labouring under the same affection, from a poison introduced in any other method, or from a poison actually generated in London itself.*

It is universally and truly considered, that the inquiry into the origin of the first cases of an epidemic disease, in any locality, is a necessary preliminary to all other inquiries respecting the origin of future cases. At that period of the epidemic, the question is reduced into as simple elements as we can ever hope to find it in; and the influence of essential antecedents is less obscured than at a later date, by the presence of accidental and unnecessary circumstances.

In order that the terms which I am about to use may be correctly defined, and that it may be clearly understood in what method I am about to investigate this subject, and in what light I regard the general aspect of the great question of the nature of contagious and epidemic diseases, I shall commence with a condensed recapitulation of what I consider to be the most prominent and correct opinions at present entertained by medical men, respecting the diffusion and mode of propagation of those diseases, which are generally allowed to arise from specific and uninterchangeable poisons, and which are capable, under certain conditions, of becoming so prevalent as to be entitled to the appellation of *epidemic*. This recapitulation will be, in fact, a general definition; and when completed, the bearing of the subsequent argument will be at once apparent.

All the opinions of the day, however widely different they appear, may, I believe, be comprised under two separate creeds: that of the strict, and that of the modified contagious theory.

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* I assume that cholera, like other epidemic and contagious diseases, must result from the action of a specific agent, rather than from any temporary combination of atmospheric influences. I believe that satisfactory reasons may be given in support of this assumption. Into these I do not wish now to enter; but as far as cholera is concerned, I may refer to the second Volume of the 'British and Foreign Medico-Chirurgical Review' (p. 93), in which this question is shortly discussed.

The *strict contagious theory* I take to be that which refers epidemic diseases to the action of specific poisons, which (it alleges) multiply themselves only during their passage through the animal body. All other reputed modes of increase this doctrine considers to be doubtful or untrue; and it looks upon the external circumstances which surround the animal frame as influencing the efficient cause or poison of the epidemic, only so far as they render the body a more or less fit recipient for its action. As it concludes that the body is the only source from which a fresh supply of the specific agent can be evolved, it deems it necessary that the person, to be infected, should come within the influence of the particles of poison (imparted by contact, diffused in the air, or adhering to clothes) which have been emitted from the breath, surface of body, or excretions of an individual already suffering from the disease, or from the corpse of one who has already died of it. Nor can it be said that this view is otherwise than philosophical; that is to say, it is rested on a foundation of undeniable truth, and its inferences are not obviously inconsistent with the premises it lays down. It sprang naturally, indeed, from a recognition of the great truths, that each epidemic disease originates from a cause which is peculiar to itself, and which is not interchangeable with the cause of another epidemic disease; that, however two or more such diseases may be temporarily combined, they are yet fundamentally distinct; that they are governed by separate laws, and display attributes which manifestly prove their non-identity. And it so happened that when this opinion first took solid root in medical literature, the epidemic diseases which were chiefly witnessed in France, England, and even in Italy, did really spring from poisons whose most potent, and apparently whose only source, was in the very bodies, whether of men or of animals, which were suffering from their effects. The poison grew at the expense of that it tainted. At least, this was eminently the case with smallpox, and with measles and scarlet fever, which were sometimes distinguished, sometimes confounded together. It was also generally presumed to be the case with the Levant plague, which then had received very little study, except in the countries in which it was comparatively an infrequent and transient visitor.

And as in some of these cases the poison was actually tangible, could be procured in substance (though not in a state of absolute isolation), could be carried on the point of a lancet, and transferred from one body to another, it was not really a greater assumption than was warranted by the facts then known, to infer that the more volatile or intangible poisons, which were not inoculable, and whose existence was indeed founded only on analogical reasoning, did in reality multiply only in the same manner as did those poisons whose existence was demonstrable otherwise than by their effects on the human system alone.

During the last sixty years, however, the study of several diseases imperfectly known to the older physicians has added so many new facts to our knowledge of the several specific epidemic diseases, that the strict contagious theory has been insensibly undergoing alteration, until in the present day it bids fair to become merged in a higher generalization.

The extension of commerce and the military occupation of colonies have made us better acquainted with the several forms of fever, which in the West Indies, and on certain parts of the African coast, have an endemic and a local origin, but which also possess the power, under certain circumstances, of increasing themselves in the human body. The birthplaces of the Oriental plague, Egypt and Syria, have been traversed by the military and civil surgeons of France and England; and the progress of science and of national intercourse has enabled us to note more fully the returns, and to interpret more correctly the attendant phenomena, of that catarrhal fever, which, under the name of influenza, la grippe, &c., so frequently pursues its evanescent course over the greater portion of the world. The more accurate investigation of the present day has, particularly in the last twenty years, opened up fresh points of view, under which the several forms of fever prevalent in this country might be studied. While the different affections which were formerly described (under the name of continued fever) rather as varieties of a single entity, than as separate and uninterchangeable diseases, are now considered with great probability to be the products, not of a single poison, but of two or more, the influence of these several poisons has been studied, not simply as by the older physicians during their action on isolated patients, but with reference to classes of individuals. Thus it has been discovered that the circumstances under which these classes are placed, exercise an astonishing influence in aiding or counteracting the entrance of the specific agent into the body, and in favouring or nullifying the vigour of the action it there exerts.

The tendency indeed of all these observations has been to show—1st, that the effect of the human body, as a single and uniform element in producing changes in the morbid poisons, and in thereby assisting their diffusion, is by no means so great in these instances as it is in the cases of smallpox and scarlatina; 2d, that it is most probable that these poisons are in the first instance derived from sources foreign and extrinsic to the human frame, and are subsequently more or less frequently propagated and multiplied by means which are also foreign and extrinsic to it; and 3d, that the degree in which they multiply in the human body, and are in this way propagated, varies in the case of each particular poison, and according also to its alliance with other poisons, or to the state of the system through which it passes.

The evidence on which this opinion is founded cannot, of course, be here discussed; but it may be stated that all the later researches on plague, yellow fever, and in a less degree on the fevers of this country, have tended to strengthen this position, both in respect to the amount and to the precision of argument by which it is supported.

To assimilate these observations, and to accommodate their dogma to the new truths which were inconsistent with the rigor of its previous enunciation, the advocates of the strict contagious theory have introduced various changes, and have modified their modes of expression according to the genius and temperament of each individual advocate.

Such advocates advance the following considerations in reply to the statement above given:

In the case of tropical fevers, they seek to break the link which has been made between marsh and contagious yellow fevers, by considering the latter a peculiar disease, and by placing the specific poison which springs from marshes in a distinct category of morbid agents. They deny the validity of the evidence by which the contagious yellow fever (for such undoubtedly exists) is supposed to be traced up from forms arising from endemic sources, and which evidence, if true, would necessarily place the poisons of all these affections in the same class.

While the influence of epidemic constitutions of the atmosphere, and of external local circumstances, over the fevers of this country, as over the Oriental plague, is admitted, these conditions are presumed simply to augment the susceptibility of the subject, but to have no direct influence on the virus itself. They act on the soil which is to receive the plant, but they do not heighten the inherent force which is normal to the seed. To explain certain special cases, which seem strongly opposed to such a view, and which point to the development *de novo* of the specific virus, the advocates of the strict contagious theory have attributed an extraordinary durability to the poisons of fever and of plague, and a power of lurking within the frame unaltered until the accessory causes have prostrated the force which at first successfully resisted them.

Influenza, again, which is a disease manifestly arising from a special virus, and which attacks so many individuals at once, and is propagated with such rapidity that we can hardly suppose this to occur through the influence of the human body, has not yet been fairly considered by the strict contagious theory. If it is not allowed to modify this theory, it must be shut out altogether from this class of morbid poisons, although in many respects it is an apt representative of the class.*

The strict contagious theory was considered to derive its strongest proofs from the fact, that those persons only suffered who came in contact or were in proximity with the sick. Lately, however, a section of this party has questioned the possibility of setting bounds to the diffusion-distance which a poison can traverse before its powers are destroyed. Whilst the older contagionists never assigned a greater distance than a few yards, this new opinion (which still looks on the human body as the *fons et origo mali*) doubts whether the poison may not float for an indefinite distance through the atmosphere, the condition of which may favour or impede its volatility and rapidity of diffusion. Consequently as regards this party, the evidence which is considered decisive against contagion is not decisive against them, or at least they do not consider it so; although it may be demanded how (independent of inoculation) they could prove that a single morbid poison was ever reproduced in its passage through the body, unless by the evidence derived from the fact that those about the sick are more liable to the disease than those who are not?

Thus, at the present time, two great opinions seem to divide the medical world, and to be contending for the mastery. Each opinion has the same foundation—each looks on epidemic diseases as produced by specific and uninterchangeable agents—each admits that some of these agents evidently propagate themselves chiefly, or possibly solely, by means of their action on the body. After this, their paths divide. What they both assume to be true of certain specific agents, the strict contagionist affirms to be true of all of them; the modified contagionist affirms it to be true of certain of them, and this only in a limited and conditional sense.

The latter party, in seeking to determine the mode of non-contagious propagation, considers with the greatest attention the media external to the human body, which surround the particles of any special poison. It regards the humidity and temperature of the air, its purity, its electrical condition, the weight of its column, and the movement of its masses. It questions what may be the exhalations from the soil, from decaying substances, from all the various natural or artificial peculiarities which vary the surface of the ground. In all these circumstances it sees a twofold action—an action upon a virus introduced among them—an action upon a human frame submitted to them. It seeks to determine what relative assemblage of these conditions is most favorable to the spread of a poison; it believes that, under its favouring conditions, some poisons introduced *ab externo* may augment by reproducing themselves. It goes even farther than this; in certain cases, it sees, in an intense concentration of these several circumstances, a development *de novo* of that specific poison, which is proper to that particular assemblage of conditions. On the banks of the Ganges it witnesses the cholera poison spring into existence; in the swamps of Batavia, the malignant intermittent passing into remittent; on the river marshes of Western Africa, the deadly remittent, from which springs the epidemic yellow fever; among the effluvia emitted from our great cities, or among the miserable hovels of a starving nation, the fever-poisons of this country; among the Fellahs of Egypt, with their peculiar rites of sepulture, and with their pernicious customs of social life, the poison of bubo-plague. It attaches, also, much importance to the consideration of the occasional, and even periodical, augmentations in the vigour of the specific poisons, believing that these are partly dependent on influences exerted on these poisons by "epidemic constitutions" of the atmosphere.

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* If influenza arises from any peculiar allotropic condition of the oxygen of the atmosphere produced by electrical vicissitudes, and does not originate from any agent allied to the ordinary contagious agents, it may be dismissed from consideration; but, at present, the production of ozone during influenza epidemics, the capability of ozone to produce the peculiar symptoms, and the other questions which would prove a connexion between the two, are all matters of the merest speculation. On the other hand, the general laws of influenza, the peculiarities of its course, and even its occasional irregularities, render its separation from the poisons of cholera, of some forms of marsh fever, and perhaps of pertussis, almost impossible.

While the followers of this creed admit that some or all of these specific poisons may be truly contagious—that is, may be reproduced by the human system—they are disposed to limit this mode of propagation, and to admit it no farther than it can be proved by actual evidence. They refuse the argument from analogy, that because smallpox is manifestly augmented by the body, any other disease is necessarily subjected to the same influence. In the case of several poisons, they advance evidence to prove that this mode of increase is exceptional, and comparatively unimportant.

Finally, in conformity with their creed, the supporters of this opinion seek to arrest the progress of epidemic diseases by destroying the external local conditions, which, with perhaps some general atmospheric conditions, are assumed to be the necessary pabula for the poison, and by removing the internal states of the system which predispose to the reception of the cause.

The strict contagious theory, on the other hand, considers these opinions as overstrained; it looks primarily to the human body as the focus for the poison, and to the external conditions as simply predisposing and accessory causes. It questions the doctrine of origin *de novo*, and contenting itself with the fact that the poisons exist, doubts whether it is possible to trace them to their origins. It avoids, indeed, this question as beyond its reach. It separates such poisons as have a determined local origin from the true contagions, and denies that they can ever become epidemic. It places its great reliance, not on sanitary preventive measures, though for the reasons above given it does not deny the wisdom of these, but on isolation and quarantine. By preventing proximity and exposure, it deems that the poison must become extinct, if it meets with no animal frame in the structure of which it may increase. Consequently, to push this view to its legitimate conclusion, a perfect system of quarantines would secure to a nation complete immunity from all epidemic diseases, except those which were previously located within it. To be perfectly consistent with itself, this theory must restrict the action of the so-called "epidemic constitution" to a simple effect on the recipient of the poison.

It will be seen at once that these two opinions are in one sense opposed to each other, in another are even compatible. The former includes the latter, or might do so; for it admits the human body as a possible multiplying source, in the case of all the poisons. It only demands that the proportion of cases which increase in this way, shall be proved from evidence similar to that which proves the production in cases from other sources. And according to the peculiar mode of regarding evidence in each individual mind, the supporters of this opinion are divided into various subsections, who all more or less incline, but none of whom actually reach (except perhaps as regards the case of influenza), the extreme of non-contagion. If in some cases (as in influenza) the evidence of the increase of a poison in the body is so defective as to cause doubts whether it ever occurs, still it is admitted that such negative evidence is always to be received with the reservation, that peculiar conditions may yet remain to be discovered or noted, which may really have the effect of temporarily increasing the influence of this particular source of the poison.—The strict contagious theory, on the other hand, is not capable of this expansion and amalgamation. It repels, and indeed necessarily and completely excludes, everything which is not comprised within its definition, or is not capable of being added to that definition without impairing its fundamental tenet. It cannot, in fact, abandon its doctrine that the body is the only multiplying agent of each poison, without entirely surrendering the character which stamps its individuality. Immediately that a partisan of this opinion allows that the impure and fetid atmosphere which hovers over a fever-locality, can generate a single particle of fever-poison, or can cause a particle thrown off from the body to generate another like itself, he has undermined his theory; and the question between the relative propagating powers of the body and the surrounding media, becomes merely a question of degree.

Such are, I believe, when stated with the utmost brevity and simplicity, and divested of all qualification not absolutely essential, the two great opinions of the day respecting the causes of epidemic diseases. There can be no doubt that in a scientific and a commercial point of view, the debate is one of great importance, and it seems probable that it is now gradually approaching a final solution.

There is one disease to which I have avoided making any but the faintest allusion, but the investigation of which has contributed essentially to the reception of the modified contagionist view. Asiatic Cholera belongs manifestly to the most strongly marked order of epidemic diseases. Its symptoms are remarkably precise and recognisable, and their course and mode of evolution are determinate; its poison propagates itself at intervals over countries which it afterwards abandons for a series of years. There is, therefore, in these countries nothing of the endemic or the local about it.* It sweeps, like an atmospheric wave, over vast tracts of the earth, or possibly itself brings or induces the epidemic constitution which permits it to be generated. There is little doubt but that it nosologically belongs to the same order of diseases as smallpox or typhus—viz. that it is produced by a definite epidemic poison entering the person *ab externo.*

As, therefore, it is a disease evidently not indigenous to this country, as it comes to us from without, and merely borrows force from the elements of increase it finds here, whatever these may be, it affords a most convenient opportunity for applying the two theories above described, and for observing which best interprets its phenomena. And as every point which bears on Asiatic Cholera bears also on the general subject of contagion, it is of the greatest importance to render all evidence as correct as it is possible to make it.

In the following Inquiry, I do not intend to enter on the general subject. I am simply desirous of placing on record the evidence respecting the first cases of cholera in London, and to inquire particularly into the possibility of these cases having arisen from contact or proximity with persons already diseased.

The strict contagionist theory has always considered that strong evidence in its favour was to be obtained from a study of the early cases. It claims to be always able to point out the channel of introduction, and to trace the first steps of the malady; afterwards, when the sick have become numerous, it considers that persons may be exposed to emanations from a distance, and may sicken, without being aware that they have been so exposed. At this late period, therefore, the proofs of contact or proximity cannot be obtained; and negative evidence loses its value.

The first twenty-five or thirty cases are then, in this point of view, most important. Did they arise together, or near each other? Were they exposed to sources of contagion, from which the other inhabitants of the district were exempt? Can each successive case be traced to a prior case, until the patients are too numerous to be followed up?

If these questions can be answered in the affirmative, it must be conceded that the strict contagionists have carried their point; if they are not so answered, then the observer has to seek for the cause of the early cases in other directions.

It is necessary in examining evidence on this point to adopt two precautions.

1. Every reputed case of the disease must be known.

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* I assume here, that common English cholera is an entirely different disease from Asiatic cholera. This point is not fully ascertained; but at present the weight of the argument is against the identity. But, even if we admitted that every year sporadic cases of true cholera occurred in London, still the bearing of the argument in the text would be unaffected. The epidemic has certainly on two occasions travelled regularly to us, and has not arisen into activity among us independently of an external and peculiar influence.

2. Every reputed case must be inquired into, and its exact nature determined; the loose accounts of by-standers and non-professional persons not being received as credible evidence.

With regard to the first of these precautions, there is little doubt that the cases I am about to detail comprise all, or nearly all, the first cases in London. Almost all the early cases in London were reported to the General Board of Health, and were immediately investigated by means of their agents; and if any early case was not so reported, and proved fatal (which in the case of cholera it would probably do), it would be found in the Registrar-General's returns.

As to the second precaution, I have to state that the General Board, as already said, having, by means of competent agents, inquired into all the cases, no loose hearsay evidence will have even to be discussed. I have myself carefully examined the records of all these cases; in many instances I saw the patients themselves; and any case which seems doubtful will be left out of the argument, unless its admission would seriously modify a conclusion come to in its absence, in which case I shall discuss the point both with and without it.

It has been stated in some of the medical journals, that several cases, very much resembling Asiatic cholera, occurred in London during the summer and early autumn months of 1848. The only decided case of this kind which I can find, however, is one reported by Mr. Haden, in the 'Medical Gazette' for October 13, 1848, to which journal I must refer for its details.

CASE I. This case, at the time of its occurrence, was considered by Mr. Haden to bear so strong a resemblance to Asiatic cholera, that he felt inclined to believe it to be an advanced case of the approaching epidemic of that disease, and not to be simply a case of the common English or bilious cholera. If it was, however, a severe case of common sporadic cholera, it becomes a question by what symptoms it can be distinguished from the epidemic disease. The assemblage and concatenation of symptoms were altogether similar to those of Indian cholera. The evacuations were like conjee water, and without bile; there was the usual failure in the circulation, with its accompanying symptoms; there were the copious sweats and the suppression of urine; there were the usual periods of the disease, vomiting and purging, soon attended by cramps, a subsequent superadded cold stage, and a period of reaction. It is clear that had this case occurred at a time when epidemic cholera was prevalent in the neighbourhood, it would unhesitatingly have been considered a marked example of the disease.

As, however, I am anxious to avoid all possible fallacies, I shall put aside this case, or rather shall not use it in the present inquiry. Whatever its nature, it cannot be attributed to any poison brought near the patient by an infected person. There had been no case of cholera in the neighbourhood, and no one subsequently suffered.

CASE II. The second case was reported to the Board of Health by Mr. Howell, of Wandsworth. The patient, a man aged 41, a coalwhipper by trade, living at Spencer's court, Waterside, Wandsworth, was attacked at 11 p.m., July 31, with violent purging and vomiting, and severe cramps; these symptoms were speedily succeeded by blueness, lividity, and coldness of the surface and tongue. He remained in the cold stage for forty-eight hours, and then passed through slight consecutive fever.

Although the symptoms above given are well marked, and are detailed by a practitioner, who it is believed was well acquainted with the symptoms in the epidemic of 1832, still, as cases similar to this one are said by some practitioners to be of annual occurrence in London, and in other parts of the country, it will be advisable to omit this also from the present inquiry. It may be mentioned, however, that in this very locality a considerable number of cases of undoubted Asiatic cholera occurred during the last month of 1848, and the first month of the present year.

CASE III. The third reputed case in London, with which I am acquainted, occurred in the practice of Mr. Fairbrother, of Southwark, on the 16th of September. A man, named John Dean, aged 53, resident at No. 5, Lion street, New Kent road, died after a few hours' illness. An inquest was held, as there was a suspicion of poisoning; and Mr. Fairbrother gave it as his opinion that the patient died from Asiatic cholera.

Although I could not undertake to say positively what this case may have been, and although Mr. Fairbrother, who saw the case, is more likely to be correct than any one who had not that advantage, it does appear to me that some doubts may be entertained whether it was really Asiatic cholera. Without, however, deciding on this point, more particularly as I am not aware whether a post-mortem examination was made, it will be advisable not to use this case. It was not supposed to be traceable in any way to contagion, and therefore its omission is no prejudice to the contagionist argument, but rather the reverse.

CASE IV. The fourth case of cholera which occurred in London, was the following.

The Elbe steamer left Hamburgh on the 22d of September, and arrived in the river on the 25th. A seaman, named John Harnold, left the vessel, and went to live at No. 8, New lane, Gainsford street, Horsleydown. On the 28th of September he was seized with symptoms of cholera, and died in a few hours. It is stated in a letter to the General Board of Health, from Mr. Russell, who attended the patient, that all the characteristic symptoms of Asiatic cholera were present. Mr. Bowie, who inquired on behalf of the Board into the particulars of the case, corroborated this statement. This may then be considered as an undoubted case of cholera.

It becomes a question of some interest to determine whether this man became infected at Hamburgh, where cholera was raging at the date of the sailing of the vessel, or in Horsleydown itself. Some support is given to the first opinion by the fact that another probable case of cholera had occurred on board the steamer at sea.

Mr. Bowie's Report gives the following particulars of this case:

"On the voyage from Hamburgh to London, and about forty miles from Lowestoft, the second engineer, who had been long in ill health, died of an attack very much resembling cholera. Immediately after death, the body was placed in a box which was closely nailed down. All the clothes and bedding were thrown into the sea. When the vessel arrived at Gravesend, she was detained about six hours, during which time the quarantine doctor came on board, and granted permission for the body to be conveyed to London to be delivered to the friends, which was done at St. Katharine's Docks by the first engineer."

It may be questioned, however, whether the statement of the captain, "that the disease very much resembled cholera," is of much value, particularly when it is remembered that this man had been for some time in bad health. But if this were a case of cholera, it would be important, as proving satisfactorily that the crew of the Elbe had been exposed at Hamburgh to the influence of the choleraic poison. The *certainty* thus afforded us that John Harnold was exposed n that locality to the action of the cause, would necessarily increase the probaility that he was there infected. It may be said also that the time between the eparture from Hamburgh, and the occurrence of the disease in London (six days), as within the limits of the incubative period; though certainly it is rather a ng time.

On the other hand, there is little doubt, from the facts to be presently adduced, at Harnold, when he entered London, entered a place where cholera was mmencing to prevail; as cases, contemporaneous or nearly so with his own, curred in two or three distant parts of the metropolis. And as it seems to be peculiarity of this singular disease, that journeys from infected places increase e liability of the system to suffer from its cause, it may be doubted whether it not as likely to be the fact, that this man became infected during the three ys that he was on shore, before the disease appeared.

As far as the argument is concerned, however, it is of no importance which view is the most correct. If the disease was imported thus from Hamburgh, it did not spread in Horsleydown. Two days subsequently, indeed, Mr. Russell was sent for to a patient in the same house, who fancied he had cholera; but on examining into particulars, it turned out that the individual in question had been greatly alarmed at the death of the seaman, and was suffering more from the effects of fear than anything else. He was quite well in a few hours. No other person was taken ill in the house or immediate neighbourhood; although if the second case had not been inquired into, a vague story of communicated disease might have arisen in the neighbourhood.

Case V. Within 24 or 36 hours after the death of John Harnold, an undoubted case of cholera occurred in another part of London, in Lambeth, which is distant about two miles from Horsleydown. The patient was a man named John Murphy, aged 22, resident at No. 26, Lower Fore street, South Lambeth. He was a labourer in the adjoining gas-works, but out of employ, and badly fed. On the 30th of September he had been with a cousin to Kensal Green, on the Harrow road. He appeared in the morning to be perfectly well; but was first seriously attacked at 11½ p.m., and died at 8½ on following morning (Oct. 1).

The correctness of the account given to me by the friends of the patient, when, by order of the Board, I inquired into this case, was corroborated by Mr. Thomson, of Lambeth, who attended the patient, and who at once considered the case to be one of Asiatic cholera. After death this man became very stiff. When I inspected the house five days after his death, he was still unburied. I removed the lid of the coffin, and found that, though decomposition was far advanced, and the face was swollen and black, the cadaveric rigidity was yet well marked in the extremities. That this was a genuine case of Asiatic cholera, I have no hesitation in affirming.

At the same time that this case occurred in Lambeth, the first of a series of cases appeared in a small court in Chelsea, situated on the opposite side of the river from Lambeth, and still farther removed from Horsleydown. In the course of a few days, six cases had occurred in this narrow court, which is situated close to the river, and is one of the most wretched and dirty localities in Chelsea. These cases were attended by Mr. Keen, of Chelsea, and were also visited by numerous medical men, who were all satisfied that they were undoubted examples of Asiatic cholera.

Case VI. Richard Cook, aged 10. This case, the first of the series just referred to, was certainly very mild in character; yet the symptoms, and the occurrence of better-marked cases immediately afterwards in the same locality, seem sufficient to establish the fact of its being one of cholera.

Case VII. The next case occurred in a locality widely removed from any of those formerly described, viz. in a court leading out of Fleet street. The subject of it was Jane Langham, aged 27, who resided at No. 3, Harp court, Fleet street. She was visited by Mr. Digby, of Fleet street; who, when first summoned to the patient, considered her to be in the collapsed stage of Asiatic cholera; and his diagnosis was fully borne out by the general symptoms.

Case VIII. The next occurred in a part of the town several miles removed from any of the former localities.

Owen Jones, æt. 60, a convict on board the Hulk Justitia, lying off Woolwich, was admitted into the Convict Hospital-ship Unité, also lying off Woolwich, at 7 p.m., October 2d. It was ascertained that he had eaten his dinner as usual, and that soon afterwards he appeared chilly and indisposed. At tea-time he had spasms in the arms, and his shipmates becoming alarmed at this, and at the cold listless state into which he seemed to be falling, sent him to the hospital. When admitted he was evidently in the collapsed stage of cholera, and died after seven hours.

I was informed by Mr. Dabbs, R.N, on duty on board the Unité, that this case was undoubtedly one of cholera. It was apparently a case of the most malignant type, in which fibrine is poured into the intestinal canal with very little of the water and soluble salts of the blood. The diagnosis of the case was also confirmed by the speedy occurrence of other cases of cholera, presenting symptoms very similar.

Case IX. The next case occurred in a house three doors removed from the one in which Case V had occurred.—James George, aged 40, residing at No. 29, Lower Fore street, Lambeth; a butcher's carter.

On Wednesday, October 4th, he got up at his usual time, viz. 4 a.m.; and immediately complained of pain in the bowels; he passed suddenly a very loose and offensive evacuation. Before 5 o'clock he had two or three more evacuations, but certainly not more than this; he did not vomit or complain of noise in the ears, giddiness, or faintness. His wife was so accustomed to see him affected with bowel complaint, that she paid little attention to his symptoms, and, after taking a dose of brandy and rhubarb, he went to his work in the city. On his way to Newgate Market, he was attacked in St. Paul's Churchyard with violent vomiting and purging; the latter symptom continued until his admission into St. Bartholomew's Hospital at half-past 8 a.m., where he died at 9 p.m., having been ill seventeen hours from the period of the first stool.

The physicians at St. Bartholomew's were unanimous in considering this an undoubted case of Asiatic cholera, and reported it as such through the treasurer of the hospital to the Secretary of State.

It should be mentioned that this man was not acquainted with Murphy (Case V), nor with any one in the house (No. 26) in which Murphy lived. If the two men had been at all in contact, during the time Murphy could be said to labour under the disease, it must have been in the evening of Saturday, Sept. 30, when Murphy was returning from Kensal Green, and may be supposed to have been already infected, although the symptoms had not declared themselves. But it is unlikely that a mere casual encounter in the street, if such occurred, could have infected the patient George. It is more probable that, if he derived the disease from Murphy, it must have been through the particles of poison floating over the neighbourhood. Even with this supposition, the period of incubation is rather prolonged (three to three and a half days). Such a supposition becomes probable in this case, only when its probability has been shown on other grounds.

Case X. A daughter, aged 6, of the above patient (James George) was attacked on the same day (Oct. 4), and nearly at the same hour, with diarrhœa. At half-past eight she was conveyed to St. Bartholomew's Hospital, and admitted under Dr. Burrows, who considered it to be a decided though mild attack of Asiatic cholera. She ultimately recovered.

Case XI. The eleventh case occurred in the same court, in Chelsea, in which the sixth case occurred, and in the person of the mother of that patient Susan Cook, aged 40, residing at No. 7, White Hart court, Duke street, Chelsea.

This patient was seen by Mr. Keen on the 4th October; at that time she presented symptoms similar to those of her son in an exalted degree. She continued to present nearly the same symptoms till the 6th, when she began to improve. She passed urine on the 7th, and fæculent motions on the 8th. She eventually recovered.

Case XII. The twelfth case occurred in Horsleydown. A man, aged 77, residing in Horsleydown, was attacked at 5 a.m., Oct. 5, and died at 8½ p.m., 15½ hours after the first stool. Dr. Greenwood, who reported the case to the Board, had no doubt that it was a pure case of Asiatic cholera; and this opinion was shared by Mr. Bowie, who made inquiries into the case.

This patient lived in Horsleydown, and consequently may casually have been in contact with John Harnold (Case IV), who died between six and seven days before. I am not aware at what distance his dwelling was situated from the lodging of John Harnold, but I believe it was not in the immediate neighbourhood. The reporters do not say a word as to any possible connexion between this case and that of John Harnold.

CASE XIII. The thirteenth case occurred in an altogether different locality, viz. on board the Dreadnought Hospital-ship, lying off Greenwich.

Robert Gordon, aged 16, a sailor, was admitted on board the Dreadnought, in the beginning of September, with the sequelæ of smallpox. A few days after his admission he was attacked with diarrhœa, and continued for about a fortnight to pass frequently rather copious frothy yellow stools. These, however, were gradually diminishing in number, and it was intended to discharge him from the hospital. On the morning of October 5, he was employed in assisting the nurses, and appeared as well as usual; but about the middle of the day the characteristic symptoms of Asiatic cholera presented themselves, and he died in about twenty-four hours.

It will be observed that this case occurred in a patient under treatment. It was not brought into the Dreadnought. By the kind permission of the officers of the Dreadnought, I took the opportunity of inspecting the admission-book, and learned that no sailor arriving in a ship from any port in or near which cholera was or had been prevalent had been admitted into the Dreadnought, for any complaint whatever, for some considerable time. The disease, therefore, could not have been brought on board by the clothes of some non-infected individual arriving from an infected ship.

CASE XIV. John Healey, aged 25, a cousin of John Murphy (Case IV), and living in the same room with him, was taken ill at 8 a.m., Oct. 5. This man was also a labourer in the gas-works, and out of employ. He was a very temperate man, but for some months had lived very badly, being sometimes thirty-six hours without food. He had never been subject to bowel complaint. He was first attacked on the morning of October 5; I saw him the next morning at 9 a.m., and he then presented all the signs of a man about to rally from a mild attack of cholera; he subsequently passed into the stage of consecutive fever, and died on the sixth day with coma and a black tongue.

CASE XV. Joseph Hill, aged 5, residing in White Hart court, Duke street, Chelsea, either next door or two doors removed from the house in which Cases VI and XI occurred. At 12, noon, Oct. 5, he was seized with vomiting, and when visited two hours subsequently, had passed into a state of deep collapse, cold and pulseless. He was not purged till a short time before death. He died at half-past 7 p.m. the same day.

CASE XVI. On the same day, Oct. 5, two cases occurred in Spitalfields, and are reported by Mr. Hart in the 'Lancet' (Oct. 14, 1848, p. 419). The subject of the first was Mary Ann C., aged 11, residing at Wilson's place, Spitalfields. She was suddenly attacked shortly after 8 o'clock, a.m., Oct. 5, and after being in a state of collapse for some hours, reaction came on, and she rapidly recovered.

CASE XVII. A sister of the above, aged 3 years, was attacked at the same time, and presented symptoms of a similar kind, but of less severity. On the 8th instant the mother of these two children (Case XXVIII) was attacked with undoubted cholera, and died in twenty-eight hours.

CASE XVIII. William Cook, aged 40, the father of Richard Cook, (Case VI) and the husband of Susan Cook (Case XI), residing at No. 7, White Hart court, Duke street, Chelsea, a man of drunken habits, had diarrhœa on the 6th October, having had occasional looseness during the previous week. At 6 o'clock p.m., October 6th, he was seized with genuine Asiatic cholera. He died at 2 a.m., on the 8th, having been ill about thirty-six hours.

CASE XIX. William Cook, aged 14, son of the above, residing in the same house, was seized at 6 p.m., October 6th, with vomiting and purging; and was seen at 8 a.m., October 7th, in the cold stage of Asiatic cholera. He died at 1½ p.m. On examination after death, the appearances usual in cholera were found.

CASE XX. Elizabeth Morris, aged 14, residing at No. 7, White Hart court, Duke street, Chelsea, in the same house as the family of Cook. She was attacked with diarrhœa on the 6th of October. This continued all day; at night she began to vomit rice-water fluid, and had cramps in the legs. When first seen at 8 a.m., October 7th, she was in the cold stage. She died at 1 a.m., October 8th, having been ill between thirty-six and forty-eight hours.

CASE XXI. John Rutherford, aged 47, convict on board the Hulk Justitia, lying off Woolwich; admitted into the Hospital-ship at midnight October 6th, in a state of decided collapse. He died at twenty minutes to 10, October 7th, having been ill thirteen hours.

CASE XXII. ——— Rabett, aged 30, a sailor, had been for several months a patient on board the Dreadnought Hospital-ship, with dysentery and pleurisy. He was, however, convalescent, and was about to be discharged at the time he was attacked with cholera, the first symptoms of which showed themselves on Friday night, October 6th. On the 9th reaction seemed to commence, but the bladder remained empty; he became drowsy, and this drowsiness deepened into coma. He then presented the usual symptoms of that form of consecutive fever which is complicated with non-elimination of urea, and died about eight days from the period of attack.

CASE XXIII. James Bigwood, aged 44, convict on board the Justitia Hulk, lying off Woolwich. Admitted at 3 a.m., October 8th, in the state of collapse; and died at 2½ p.m. on the same day.

CASE XXIV. Louisa Hill, aged 10, sister of Joseph Hill (Case XV), residing in White Hart court, Duke street, Chelsea. Attacked in the morning of October 8th. She ultimately recovered.

CASE XXV. James Paterson, middle-aged, a sailor, arrived from Shields on the 2d of October. Since then, had lived on board ship in the Pool. During October 7th, he suffered from watery diarrhœa, at 11 p.m. he had vomiting and cramps of moderate intensity. At 9 a.m., October 8th, when admitted into the Dreadnought, he presented all the characteristic symptoms of cholera, and died at 8 a.m., October 9th. It will be observed that this man had been five days in the Thames, and probably eight or ten days had elapsed since he left Shields, when the premonitory diarrhœa commenced. No other case had occurred on board the ship.

CASE XXVI. A man aged 44, residing at No. 7, Prescott street, Clapham Common. Although this case occurred after some of those which follow it, I give it here, as it is an interesting case.

On Sunday, October 8th, this man was as well as usual, and his wife stated that he had no bowel complaint. The patient himself, however, stated to Mr. Greenwood, of Clapham, who attended him, that he had suffered for two or three days before his death from relaxation of the bowels. His wife was not cognizant of this, and states that if the relaxation had been great she must have known it. On Monday, October 9th, he was as well or even better than usual. He went to his work about 9 a.m., and was employed for an hour and a half in emptying and repairing a drain, some distance from the house. The drain was a small one running from a sink. Another man opened the drain, and the subject of this Report carried the contents in a pail to the place where they were to be thrown. He had been accustomed to empty privies and drains, and did not on this occasion complain of any particular effluvia. On this day he took for dinner beef and bread, and for tea bread and butter. He took no supper. He was not purged during Monday night. He was quite well on Tuesday morning. The wife does not think that the bowels were at all relaxed. He made a very hearty breakfast, and went to his work. He came in at 1 o'clock to dinner, having been affected since 9 o'clock with painless diarrhœa. Soon afterwards the symptoms of collapse appeared, and he died between 9 and 10 the same evening, having been ill about twelve hours.

This case was one undoubtedly of Asiatic cholera; the nature of the symptoms and the rapidity of their evolution seem to prove this; and in addition, the patient was visited by several of the medical men in the neighbourhood, who were unanimous in their diagnosis.

It becomes a question whether any predisposing or accessory influence is to be attributed to the effluvia from the drain, which the patient was engaged in emptying, on the day previous to his death. There was a doubt as to whether the patient really had suffered from any diarrhœa which might be considered premonitory, previous to this occurrence. If so, the effect of the drain would have been purely accessory, if not absolutely negative. On the other hand, if there had been no premonitory diarrhœa, and if these effluvia had a more active influence, of what nature was it? Did they render the frame of the individual obnoxious to the effect of the already present cholera-poison? Did they permit to be generated *de novo*, under the influence of some general occult atmospheric condition, the specific poison, which then happened to meet with an individual susceptible to its influence? To this notion there are many objections, of which I will only adduce three: 1. Such a development *de novo* has yet to be proved. 2. The development in so short a time (90 minutes) is unlikely. 3. If the specific poison could be formed so readily from drains under the influence of a peculiar choleraic atmosphere, cholera ought at this time to have been much more prevalent than it was, as in London there must have been many localities affording similar effluvia in as great abundance and concentration as those to which this patient was exposed. It is of course understood that the effluvia of the drain *per se* were quite incompetent to produce cholera,—a disease springing from a specific poison.

Altogether it seems most probable that the drain had no special effect, or that if it had any effect it must have been simply as an assisting accessory cause, posterior in point of time to the reception of the true choleraic poison into the body. At the same time there are still great difficulties about this case, which will probably remain unexplained till our knowledge of cholera is much greater than it is at present. The patient resided near Clapham Common, in a part of the town by no means overcrowded, not perfectly drained it is true, but still not very deficient in this respect; in a house not densely occupied, scrupulously clean, and dry; he lived tolerably well, and had not been exposed to weather. There was no cholera in the neighbourhood; he had not, as far as could be learnt, been out of the neighbourhood or been in contact with any infected person. In this neighbourhood also, even up to this time (Feb. 1849), there has been no second case of cholera, or at least none is known to the Board of Health. And yet this man, unsurrounded by the specific poison, unencompassed with the accessory causes which give that poison local power, died of malignant cholera in one of its severer forms. At least I do not see how the evidence to this effect can be resisted. Was there then some extraordinary susceptibility, some intense predisposition to the disease, which caused this man to fall a victim to a degree of intensity of poison quite inadequate to affect any second person, not so intensely liable? To this question it is at present impossible to give an answer; since we do not know the exact nature of the predisposition, nor, of course, the amount and extent of its influence.

The following cases, into the details of which I need not enter, but which were all unquestionable, complete the number which I consider sufficient for the present inquiry.

Case XXVII. A sailor, named Rouse, was admitted from the Pool into the London Hospital, on the 8th Oct., with undoubted cholera. He passed through the cold stage, and died of consecutive fever.

Case XXVIII. Mother of Mary Ann C., Spitalfields, Oct. 8th.

Case XXIX. A man, named Thomas Baker, residing in Friendly place, South street, Long lane, Bermondsey, was attacked on the morning of Oct. 9th, and died at 2 a.m., Oct. 10th.

Case XXX. David Davison, sailor, from Hartlepool, taken ill in the Pool, at 4 o'clock a.m. Oct. 9th; died in the Dreadnought at half-past 5 p.m. Oct. 9th.

Case XXXI. A convict in the Justitia Hulk, Woolwich, admitted Oct. 10th; died Oct. 10th; seven hours and a half ill.

In addition to these cases, the following appear in the Registrar-General's Report. Cases of cholera are reported in August and September, but these were examples of common autumnal cholera—they did not exceed the average; and the Registrar-General states, in the Quarterly Report for the quarter ending September, 1848, that, "so far as the returns down to the end of September go, there is no trace of the epidemic of cholera in England." (p. 3.)

In the week ending September 30th, 4 cases of cholera were reported, the weekly average, as estimated from five years' returns, being 7.

In the week ending Oct. 7th, 13 cases of cholera were returned, the weekly average of five years for this week being only 1.

These 13 cases are thus stated in the Registrar-General's weekly Report:

"Fatal cases of cholera returned in the week ending Saturday 7th. (All are certified by medical attendant.)

"1. In Old street (subdistrict), St. Luke's, at No. 39, Rahere street, wife of a gentleman, æt. 59, 'disease of the bowels, simulating Asiatic cholera' (38 hours' duration).

"2. In south subdistrict, West London, F. 27 years, 'cholera' (13 hours' duration).

"3. In St. Bartholomew's Hospital, West London, M., about 40 years, 'Asiatic cholera.'

"4. In town (subdistrict), Bethnal Green, at No. 4, Cheshire street, a weaver, F., 21 years, 'enlargement of the heart' (12 months' duration), cholera spasmodic (12 hours).

"5. In Spitalfields, Whitechapel, M., 23 years, 'cholera' (12 hours' duration).

"6. In Whitechapel, north, a girl, 4 years, 'English sporadic cholera' (7 days' duration).

"7. In St. Paul (subdistrict), St. George's in the East, M., 38 years, 'cholera' (2 days' duration).

"8. In Mile End Old Town, Lower Stepney, M., 47 years, 'cholera' (36 hours' duration).

"9. In Lambeth Church, second part (subdistrict), at Orsett street, daughter of a chairmaker, 11 months, 'cholera' (3 days' duration), convulsions (1 hour).

"10. In Rotherhithe, a boy, 11 years, 'cholera' (19 hours' duration).

"11. In same subdistrict, F., 38 years, 'cholera' (19 hours' duration).

"12. In same subdistrict, a girl, 2 years, 'cholera' (2 days' duration).

"13. In Greenwich, west (subdistrict), F., 37 years, 'cholera' (4 days' duration)."

Of these cases, two (Nos. 2 and 3) appear among my list; eleven are distinct cases. It is quite uncertain how many of these were examples of Asiatic cholera; choosing, however, the severest cases, we may suppose four (viz. Nos. 5, 8, 10, 11) to have been of this type. Some of the others may have been so, but of course this is still more doubtful. We shall presently see that their omission will not seriously prejudice either argument. I may remark, that the cases I have given, or rather those of them which were fatal, are registered among the later weekly returns of the Registrar-General.

I believe that no one who examines the evidence for himself, will have any difficulty in admitting, that the range of cases now reported, commencing with that of John Harnold (Case IV), were all examples of true cholera.

Taking this for granted, the accompanying table will show the next point in the inquiry. The table is divided into four columns:—in the first is the date of attack; in the second, the name of the individual; in the third, the locality he inhabited; and in the fourth, the infected person, if any, with whom he had been in contact or proximity.

Date.	Name.	Locality.	In contact or not with a person labouring under cholera.
Sept. 28	John Harnold	Horsleydown	With the mate of his vessel, who was said to have died of cholera—case not certain; with cholera patients in Hamburgh?
Sept. 30	John Murphy	Lambeth	Not in contact with any cholera patient
	Richard Cook	Chelsea	Not in contact with any cholera patient
Oct. 1	Jane Langham	Harp court, Fleet street	Not in contact with any cholera patient
Oct. 2	Owen Jones	Justitia Hulk, Woolwich	Not in contact with any cholera patient
Oct. 4	James George	Lambeth	Not known to be in contact with any cholera patient
	Daughter of above	Lambeth	In contact with her father
	Susan Cook	Chelsea	In contact with Richard Cook
Oct. 5	An old man	Horsleydown	Not known to be in contact with any cholera patient
	Robert Gordon	Dreadnought, Greenwich	Not in contact with any cholera patient
	John Healey	Lambeth	In contact with John Murphy
	Joseph Hill	Chelsea	Probably in contact with Richard and Susan Cook
	Mary Ann C.	Spitalfields	Not in contact with any cholera patient
	Sister of above	Spitalfields	With her sister
Oct. 6	William Cook	Chelsea	In contact with Susan Cook
	Son of above	Chelsea	In contact with his father and mother
	Elizabeth Morris	Chelsea	With the above
	John Rutherford	Justitia, Woolwich	With Owen Jones
Oct. 7	— Rabett	Dreadnought, Greenwich	With Robert Gordon
Oct. 8	James Bigwood	Justitia, Woolwich	With Owen Jones and John Rutherford
	Louisa Hill	Chelsea	With Joseph Hill
	James Paterson	Pool	Not known to be in contact with any cholera patient
	Rouse (a sailor)	Pool	Not known to be in contact with any cholera patient
	Mother of Mary Ann C.	Spitalfields	With her daughters
Oct. 9	James Baker	Bermondsey	Not known to be in contact with any cholera patient
	David Davison	Pool	Not known to be in contact with any cholera patient
	A convict	Justitia, Woolwich	With the convicts mentioned above
	A man, aged 44	Prescott street, Clapham	Not in contact with any cholera patient.

It thus appears, that, from the 28th September to the 10th October (twelve days), 28 cases of cholera occurred in ten different localities, situated, not in proximity, but in various and remote divisions of this immense city.

The inhabitants of these localities do not appear to have held any intercourse with each other; and in no single instance could it be discovered that the *first* person sick in any particular locality had been in contact or proximity with another individual previously diseased, in another locality.

The following considerations support this assertion:

1. Such contact was in no case alleged to have taken place by the patients themselves, nor by their relations. John Harnold had been in London only three days before his death, and it is not known how he was employed during those three days; but most probably he was at the docks. John Murphy, the second case, resided some miles away from Horsleydown or the docks; and, although I did not specially inquire whether he had been at the docks (being ignorant at the time of the case of John Harnold), I made general inquiries into his habits, and learnt that he remained usually at home, but, on the day preceding his death, had gone to Kensal Green, a district situated five or six miles on the Harrow road, in a contrary direction to Horsleydown or the docks. Jane Langham, again, lived near Fleet street, in a place far removed from either Lambeth or Horsleydown; and in her case it was proved, that she had not been out of the immediate neighbourhood for some time before her death. In respect to the cases at Chelsea, again, I begged Mr. Keen, who attended them, to inform me if any facts had come to his knowledge, which could lead to the suspicion that his first cases had been in contact or proximity with any diseased persons; Mr. Keen replied, that no such communication was traceable. What, indeed, except accident, could have brought John Murphy, the labourer in the gas-works at Lambeth, in contact with Harnold, the seaman of Horsleydown, or either of these with the wife of a coalporter in Fleet street, with a convict in the hulks at Woolwich, or with a sick man on board the Dreadnought Hospital-ship at Greenwich?

2. But if these several persons were in no way connected by trade, profession, or calling, if they were not exposed by proximity of habitation to the emanations emitted from the bodies of each, is it not possible that some extraordinary series of accidents may have brought them together; and that, without actual knowledge of the fact, John Murphy may have met John Harnold, Jane Langham, or the boy at Chelsea, and have infected, or have been infected by these persons?

To those who know the localities above named, and who are acquainted with this vast metropolis, this question will appear almost absurd. To those who are not possessed of this information, it may be mentioned that Horsleydown lies on the south side of the Thames, to the eastward of Bermondsey, and is situated close to the river, at a point nearly opposite to that where St. Katharine's docks are placed. From Horsleydown to Lambeth, by the river, is a distance of several miles, as the Thames here makes a large curve. As the crow flies, however, Horsleydown is about two miles distant from Lambeth, or rather from Lower Fore street, where the second case of cholera occurred. The two spots are separated by the districts of Bermondsey and Southwark, which compose an immense city, crowded with inhabitants. The spot in Chelsea, in which the next case occurred, is on the other side of the river, and close to Battersea Bridge; it is, in a direct line, about two miles from Lower Fore street, Lambeth, and four from Horsleydown. Harp court, Fleet street, is in the city of London, about half way between Temple Bar and Ludgate Hill, and is separated from the Thames by the breadth of Fleet street, and the length of the lanes running from Fleet street to the river. As the crow flies, it is about two miles, or one mile and a half from both Horsleydown and Lambeth. It is about four miles from Chelsea. Woolwich is situated about eight miles from Horsleydown, ten from Lambeth and Fleet street, and twelve from Battersea Bridge. The

Dreadnought Hospital-ship lies off Greenwich, about three miles above the Justitia, and five from Horsleydown. Spitalfields, Clapham Common, and Bermondsey are also districts widely separated from each other. The spot in Bermondsey, in which the case occurred on the 9th of October (eleven days after John Harnold's case), was at the part of Bermondsey most remote from Horsleydown. If the four cases which occurred in the Registrar-General's weekly return, of the week ending October 7, are included, the following localities will have to be added, Stepney (one case) and Rotherhithe (two cases), both places being some considerable distance from the localities given above. The fourth case occurred in Spitalfields. The localities would thus be increased to twelve; the cases to thirty-two. As far as can be known, therefore, to leave out these four cases, and the two additional localities, is not likely to prejudice the strict contagious argument; whilst any addition to the number of localities would, *cæteris paribus*, be unfavorable to it.

Now as these twenty-eight were the only cases of cholera occurring in London within ten days (with the exception of the Registrar-General's four cases, which, if included, would even increase the number of affected localities), if we seek to derive the disease of each patient from contact or proximity with a previous case, we must have recourse to the most extraordinary suppositions. We must suppose that John Murphy had somewhere, in the densely crowded streets of London, brushed past John Harnold (who had been only three days in town, and was probably at the docks all day, rather than in the neighbourhood of Lambeth); that Jane Langham, in the same extraordinary way, also had a fatal rencontre with one of these two individuals; and that in a similar manner all the first cases in the several localities must have obtained the disease, by this accidental passing in the street of an individual whose residence was many miles away.

But the probability of such accidental rencounters in London will appear to every one to be unsustainable for a moment; and, in addition, is it possible that the rapid transit of individuals should in this way communicate cholera? If so, cholera is the most virulent of contagions, and nothing could prevent it from half depopulating entire London. But if this rapid transit did communicate it, it must be remembered that the only time John Murphy could have met John Harnold, or Richard Cook could have met either of these two, or Jane Langham any of the three, must have been before the symptoms were developed, even in the shape of premonitory diarrhœa, which symptom, indeed, did not occur in the first two cases, and commenced at night in the third. But I need not delay on this topic, as the next argument is absolutely conclusive against the opinion in question.

3d. In two instances, if not in more, it is quite certain that there *could* not have been even an accidental encounter between these patients and others labouring under cholera. In the hulk at Woolwich, the first case occurred on the 2d of October. Now the convicts at Woolwich may be said to be in a kind of quarantine; they work in the dockyard, are watched by the appointed officers, and are allowed no intercourse with other persons. The Justitia Hulk was lying about three miles below Greenwich, and far apart from any other vessel, except the convict hospital-ship. No merchant vessel, indeed, ever anchors at this point of the river; or if any vessel should anchor, it is merely on account of the tide, and the place of anchorage would be many hundred yards from the spot where the Justitia was moored, which was close in shore; so that if cholera had been raging in Woolwich, and had been prevailing in the vessels in the Thames above Woolwich, the origin of cholera in the Justitia would still, in all probability, not have been attributed to contagion. But there was no cholera in Woolwich, or in the merchant vessels in the Thames; and the only cases in London which were anterior in point of time to this at Woolwich, were those of John Harnold, at Horsleydown (seven or eight miles distant), John Murphy, at Lambeth (twelve or thirteen miles distant), Richard Cook, at Chelsea (thirteen or fourteen miles distant), and Jane Langham, in Fleet street, (ten or twelve miles distant). The occurrence, then, of anything like contact or proximity between these individuals and Owen Jones, the convict at Woolwich, may be said to have been absolutely impossible. So again in the Dreadnought Hospital-ship, Robert Gordon was attacked on the 5th of October. The Dreadnought lies off Greenwich, and is three or four miles away from the Justitia, with which it holds no kind of communication. It is also many miles removed from Horsleydown, Lambeth, Chelsea, and Fleet street. Robert Gordon had been for a month on board before his seizure; he could not, therefore, have been in contact or proximity with any of the nine cases which occurred previous to his attack.

A question may be put here in respect to these two cases, which I shall have to put presently to all the cases. Although Owen Jones and Robert Gordon could not have been in contact or proximity with any of the known previous cases, may they not have been exposed to some other source of contagion?

To this it may be answered—

(*a*) That none such is known. On board the Justitia, as already said, the convicts were far removed from all other vessels, and were not allowed intercourse with any one. And on board the Dreadnought, as I have stated in a former part of this Report, no sailor arriving from any infected place, had been admitted with *any complaint whatever*, for some considerable time prior to the case of Robert Gordon. The Dreadnought also lies in a clear part of the river, and is several miles below the Pool where the merchant vessels lie.

(*b*) No other source of contagion can be indicated, except the cases above given, and unreported cases which may have occurred among the merchant vessels in the Pool. For these London cases are among the earliest in the kingdom, being only four or five days posterior to the cases at Hull; so that the number of possible cases is reduced to so small a number, that we can feel certain that nearly all chances of error arising from unreported cases are avoided.

It may therefore be concluded, with as much certainty as can ever be obtained from evidence of this kind, that—1, as there is no evidence of the persons mentioned above having been in contact or proximity with each other; —but on the contrary; 2, as such contact or proximity is, from the respective nature of the localities in which these cases occurred, and from the nature of the occupations in which the individuals were engaged, in the highest degree improbable; and, 3, as in two cases it is *absolutely* impossible that there should have been such contact or proximity;—therefore, the strict contagious theory, viz. that which supposes that an individual derives the epidemic disease from being in contact or proximity with a person already sick of the same malady, is not capable of explaining the mode of origin of the earliest cases of cholera in London.

The argument on which this conclusion is founded can be equally supported, if any of the cases now considered as cholera are denied to be of this nature. It is not worth while to go over the steps of the problem; but if the cases of Richard Cook, Jane Langham, or any others, are considered doubtful, and are left out, other cases become available, by which it is still more clearly proved that contact or proximity must have been impossible; and in addition, the period between the successive attacks becomes too great for the ordinary period of incubation, so that an additional argument arises against contagion.

We may ask here, whether any of these parties could have been exposed to any other "focus" of poison? I have already given reasons for believing that this could not have been the case with the cholera patients of the Justitia and the Dreadnought. There is no evidence of such latent or unknown source of contagion in the other cases. John Murphy had been for months resident in Lambeth; no other case had occurred at that time in Lambeth.* The family of Cook had been living in the same house in Chelsea in which they were attacked; no other person had had cholera in Chelsea since Mr. Haden's case, four months before. Mary Ann C. and her mother and sister lived in Spitalfields. No other case had occurred in Spitalfields at that time (unless the Registrar-General's case was anterior). James Baker was the first case in Bermondsey. The man in Prescott street, Clapham, was the first and only case in that locality, and had not been in contact with any diseased person. In fact it is needless to dilate on this point, as the chances are inconceivably small that all these persons should have been exposed to the influence of cases of cholera which are unknown to us. For it must be remembered that cholera is a disease so frequently fatal, and marked by symptoms so characteristic, that it is not likely any case would have been overlooked by the practitioner who attended it, or would not have been included, if fatal, in the Registrar-General's returns.

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* I think Mr. Wagstaffe reported a suspicious case; but this was several months previously.

But now, as the *strict* contagious theory has been shown to be inadequate to explain these cases, can any modification of it explain them?—such as that which alleges that it is erroneous to limit the extension of the poison to a few yards, but that the particles of virus passing off from an infected person may float hither and thither to an indefinite distance, so that persons miles distant from the affected individual may really derive their disease from him.

If this modification of the original contagious doctrine be kept perfectly consistent with its parent theory, which maintains that the only multiplying source is the human body, it must on no account be admitted that particles of poison emitted from a diseased person can be increased in number after they leave the body. The particles are merely so far changed, when they float into the air, by the actual condition of that air itself; they may become more or less volatile, more or less stable, according to its heat, its humidity, or its electrical condition; or they may be wafted to a greater or less distance by the force of winds, or by the direction of currents of air. But they do not increase in number until they have entered another human body, and find in it the means of generating a fresh supply of poisonous particles. If this limitation be not kept in view; if it be admitted that particles given off from a diseased person may actually generate —external to the body—other particles like themselves; then of course the question is entirely different, and the relative power of the human body and of other conditions is to be settled by evidence brought to bear on the point.

But now let me inquire whether the following hypothesis can be supported. Let us put aside all the cases previous to that of John Harnold. Let us suppose that this man derived the disease either directly from cholera patients at Hamburgh, or from the sailor who died at sea. Let us suppose that Harnold entered London an infected person, and that three days subsequently, when the disease declared itself, particles of cholera poison floated off from his body into the surrounding atmosphere. Is it then possible that these particles should have drifted to Lambeth, to Woolwich, to Fleet street, and to Chelsea; and then on to other localities in which the disease appeared, and which may then have been also infected by the particles given off by these secondary foci, which also floated into the air, and augmented the number of those emitted by the first and imported case?

Such an hypothesis as this is neither supported, nor is it absolutely controverted, by direct evidence. But there are strong reasons for believing that it is not tenable. Of these reasons the following are the most stringent:

1. The above modification of the contagious theory necessarily assumes that the specific poisons derived from the animal body are not mere chemical or inorganic gases, because (putting other arguments aside) if they were, they would diffuse themselves rapidly according to the usual law of gases, and would doubtless be so speedily diluted as to make it hardly conceivable that the atmosphere should be dangerously tainted by them, over any great extent.

But if not gases, these poisons must be supposed to be organic particles, not endued with life, or with means of multiplication. They must be particles thrown out with the excretions, with the carbonic acid from the lungs, with the perspiration from the skin, or with the excreta from the urinary or the intestinal surfaces.

But if this were the case, it is highly improbable that these particles would not, immediately after their entrance into the atmosphere, undergo the usual chemical changes to which organic matters are always subjected.

"The presence of the odoriferous principles of plants, the miasmata of marshes, and other matters of contagion," writes one of the most profound philosophers of our time, "although sufficiently obvious to the sense of smell, or by their effects upon the human constitution, cannot be detected by chemical tests. But it may be remarked in regard to them, that few or none of the compound volatile bodies we perceive entering the atmosphere, could long escape destruction from oxidation. The atmosphere contains, indeed, within itself, the means of its own purification, and slowly but certainly converts all organic substances exposed to it into simpler forms of matter, such as water, carbonic acid, nitric acid, and ammonia."*

If, therefore, such organic particles undergo these changes, we can hardly suppose it likely that they will thus drift in their original intensity through several hundred thousand feet of atmospheric air,—as must have been the case in the present instance, if they reached Lambeth and Chelsea from Horsleydown.

2. But whether the specific agent be an inorganic gas, or a cloud of organic particles, it is to be expected that, if given off in the manner supposed in the hypothesis, it would produce its greatest effects while most concentrated, viz. in the vicinity of the patient from whose body it was emitted. But this was so far from being the case in the present instance, that the agent must have passed innocuously over the tens of thousands of susceptible persons crowded in the dense lanes and alleys of Horsleydown, and have actually struck down its first victim at a spot separated from its source by myriads of houses and an immense space of ground; its second at a spot yet more remote; and its two next in localities still farther removed from all these places, and very distant from each other. If, indeed, the next cases had occurred in Horsleydown within two, three, or four hundred yards from the house where John Harnold died, we might have conceived the possibility of this driftage of poisonous particles; but it is surely an extreme assumption to extend this distance to an indefinite extent, more particularly when, as in the present case, there is no evidence of any cases occurring for some time subsequently between the two points of this extensive range.

3. There is another argument against such a view. There is strong reason to believe that the cholera-poison is not a very volatile one; it often limits itself in an extraordinary manner to one locality in a town; a street or narrow river sometimes separates a region in which the disease is raging severely, from one in which it finds no victims. And it must be understood that the class of persons in the affected and in the non-affected district is the same. Therefore it must surely be erroneous to note in one case the absence of volatility, and the next moment to ascribe to the poison so remarkable a volatility, that it shall drift for many miles in different directions, north, south, and east, as must have been the case, if the poison given off from John Harnold excited the disease in Murphy, Cook, Langham, or Jones.

We can hardly suppose, too, if the particles of poison were thus at the same time volatile and stable, that when three or four persons had been affected and contributed their quotas of particles to the atmosphere, the multiplication of cases would have gone on in a progressive ratio, but this was not the case.

There are other arguments against this view, drawn from the direct evidence in favour of other modes of origin; but to these I do not wish now to allude.

If neither the strict contagious theory, nor this derivative opinion, can satisfactorily account to us for the production of these first cases in London, shall we derive more satisfaction from the opinions of the party who have been termed, though as I think erroneously, non-contagionists?

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* Elements of Chemistry. By Thomas Graham, F.R.S., &c. &c. Second edition, p. 336.

This opinion of this party is distinguished by the fact that, without denying to the human body a power of reproduction of each specific poison in various degrees according to the poison, or perhaps also to the condition of the body, it yet contends that the poisons can increase when altogether disconnected from the human system.

In the case of each poison, this party seeks to learn what are the circumstances most favorable for this increase external to the living organism; and these circumstances (the so-called conditions of increase and existence) are determined by actual observation of the predilection of the individual poison which may be under consideration, for special sites and localities, or for particular classes of men.

As yet, the local conditions which appear to be the most favorable for the propagation of the cholera-poison are alleged to be the following: 1. A low, damp locality, particularly one situated on the banks of rivers, or on marshy ground. 2. A close, impure air, from the accumulation of individuals, and from the effluvia arising from them. 3. Any circumstances which may cause organic particles to be continually emitted into the air, and which form over the locality a kind of canopy, which is continually dissipating itself into the surrounding purer air, and is as continually renewed by exhalations from below.

These conditions being present, it is alleged, from various reasons, that the cholera-poison increases and augments itself when once it is present in the locality.

It may become present in such a locality in four conceivable ways:

1. By the arrival of an infected person, whose body throws off the particles of poison, which then meet in the atmosphere around them with the conditions of increase.

2. By the arrival of one or more non-infected persons, to whose clothes or baggage particles of poison may have adhered during their passage through an infected district.

3. By the introduction of the poison, either by currents of air or from the influence of some peculiar force,* which causes the cholera-poison to pass over tracts of country in determinate directions.

4. By an actual generation of poison in the locality itself; a peculiar atmospheric or other dynamic state having concurred to make up, with the previously existing conditions, the sum of those circumstances whose assemblage causes the generation of the particular poison proper to that conflux of conditions.

It may be believed by this party that the cholera-poison can travel in all these methods, or may be manifested in one spot after another, in consequence now of one, and now of another, of these modes of propagation. The relative frequency of any of these methods, or their actual occurrence, it may leave to be settled by evidence; but as a general rule, it may believe that the degree of the several local conditions (the state of the receiving bodies remaining constant) will determine the prevalence of the disease in one locality rather than in another, and that by taking these conditions into account, the places where cholera will prevail most severely, supposing the poison to have once entered, may be pointed out. At the same time, in countries in which cholera is not indigenous, in addition to these local conditions, a general atmospheric state (the nature of which is as yet unrecognised) may be assumed; and in the countries in which cholera always more or less prevails, an increased prevalence of this choleraic constitution may be supposed occasionally to occur. If this view turn out to be incorrect, it is yet alleged that it hypothetically expresses better than any other opinion the connexion of the phenomena under review. Into the proofs and general considerations connected with these subjects I do not wish now to enter, but simply to consider what support is given to any particular opinion by the early cases in London.

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* The existence of this force must be admitted in the case of influenza, whose course has a singular relation to that of cholera. What this force may be—whether (for instance) it is allied to electricity in some form—cannot at present be known. May it not be something which, even yet, we have not fathomed? Who can yet say, with the wonderful discoveries in magnetism which the genius of Faraday or Oersted discloses, that some other allied or separate forces have not yet to be recognised in Nature?

In twelve days, 28 cases occurred in ten different localities. In six of these localities, the circumstances which have been asserted to be efficient in aiding the activity of the cholera-poison were present in a high degree; in three others they were also present; but I have only general facts to bear on the subject, no special inquiries having been made. In the tenth locality, viz. in Prescott street, Clapham Common, in which a single case occurred, the conditions of development of the poison did not exist in a marked degree. The origin of this case, as stated in the account of the case itself, is rather obscure.

The six localities in which moisture, effluvia, and impure air were abundantly present were:—1, Lower Fore street, Lambeth; 2, White Horse court, Chelsea; 3, Harp court, Fleet street; 4, Hulk Justitia, Woolwich; 5, Spitalfields; 6, the Pool (part of the Thames).

In three other localities, viz. in Bermondsey, Horsleydown, and on board the Dreadnought, the poison must have met equally with moisture and effluvia. The case at Bermondsey occurred in the immediate vicinity of Long lane, which has long been notorious for its open ditch, its crowded and miserable houses, and for its general wretched sanitary condition. The cases at Horsleydown were also in a wretched neighbourhood close to the river; and on board the Dreadnought, although the ship is scrupulously clean, yet of course it is exposed to the effluvia from the numerous patients it contains, from the bank of the Thames near which it lies, and from the surface of the tainted water.

The point being conceded that in nine localities the poison would have met with favorable conditions of development, we have to inquire how the poison arrived in these localities;—was it brought there? did it travel there? or was it generated there?

The scrutiny to which these cases have been submitted, proves that in the greater number of the localities—in all, in fact, except Horsleydown—the poison was not imported by any person ill of cholera arriving in the locality. Nor could it have been brought by the clothes or baggage of any persons coming from infected districts in England, as there were no infected districts from which such persons could have come. There is no evidence either of any persons arriving from the Continent, from Hamburgh or from Dantzic, to these localities. If such persons did arrive in London, it is most probable that they were connected with the shipping, and were therefore many miles removed from Lambeth, Chelsea, and the other places in question.

In Horsleydown, however, the poison may have been imported; but if so, it is a very interesting point that it did not seem to propagate itself by contagion or otherwise. The next case which occurred in this locality was seven days after that of John Harnold, and after this there were only a few scattered cases for many weeks. The poison therefore died away, or rather remained at its lowest point of intensity.*

If therefore the poison was not brought to Lambeth, Chelsea, the hulk Justitia, or the Dreadnought, by infected persons or non-infected persons or clothes, did it enter in some other way, or was it generated in these localities?

Per viam exclusionis, it is necessary to admit either of these modes rather than the two former. It is not unreasonable to suppose that some moving force, altogether independent of the bodies of men, may have driven particles of cholera-poison into these localities; for certainly there has been some power which, independent of human intercourse, and in opposition occasionally to winds, has driven this disease from the steppes of Tartary to the English shores.

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* Although it so happened, that the first case in London, omitting the anterior sporadic cases, was in the person of a man who perhaps brought the disease with him, I think all the facts seem to prove, that the almost simultaneous appearance of other cases was a mere coincidence. I do not see how it can be otherwise; and yet, in the history of cholera, there are a good many of these coincidences.

With regard to the generation of the poison, there is no doubt that in Horsleydown, as well as in Lambeth, Chelsea, and in many parts of London, some of those circumstances which we have termed "local conditions of existence" existed in considerable intensity, and yet the cholera-poison did not develope itself in any commensurate degree. Was there not some essential condition wanting, whose deficiency prevented or nullified the effect which the other conditions would have had if conjoined with it? And if so, what was this condition?

And it may be remarked that this evident inability to propagate itself rapidly[*] was displayed in all the localities—at Chelsea, at Lambeth, and even at Woolwich; and it has continued to be marked, more or less, even to the present date (February).

At Chelsea, although the court in question was close to the river, was in a most filthy state, and was crowded with inhabitants, only six cases occurred; the disease then stopped, and has not since returned. In Lower Fore street, Lambeth, although a place which had actually been pointed out by Mr. Wagstaffe before the cholera appeared in England, as eminently displaying the local conditions favorable for the spread of the disease, only four cases occurred, and then the disease stopped for a month. And even on board the Justitia, although cases continued to be furnished until the convicts were moved from their unhealthy anchorage, yet they occurred slowly, and with intermissions of days, or occasionally even of a week, between each case.

I confess that I was much struck with this peculiarity, since in India (at any rate of late years) if in any large town presenting conditions like those of London, cases of cholera had been seen to occur simultaneously in several districts, the observer might almost certainly have predicted that that town was about to suffer from a general epidemic. The way in which the cholera almost died away out of London, is not explicable on the idea that the sanitary condition was too good to furnish the poison its local conditions of increase, nor is it explicable on the contagious theory, or on any modification of it.

The previous hypothesis of a choleraic constitution seemed to me the only likely way of accounting for the non-development of the poison after its introduction. When it is considered how cholera has lately passed like a broad belt over a particular part of Europe, leaving untouched the districts on either side, although these districts were populous, held free intercourse with infected districts, and presented hygienic conditions favorable for the reception of the poison, we shall see cause to believe that there is some element yet unindicated, connected with the actual progress of the disease,[†] which is wanted to complete the sum of conditions under which the poison attains its highest

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[*] This also tells against the strict contagious theory; as, if susceptible human bodies were all that the poison wanted, these have of course been abundantly supplied to it.

[†] As an illustration of this point, I may refer to the 'British and Foreign Medico-Chirurgical Review,' Jan. 1849, p. 8, which is taken from the description, given by M. Leseque (Archives Générales, Sept. 1848), of the course of the cholera in Russia.

"The peculiar force which impelled the cholera thus towards the north, is not only indicated by the general course of the disease, but by a closer study of its peculiarities. Whenever the malady deviated, so to speak, from its normal direction, and passed towards the west, it seemed incapable of propagating itself, and died away spontaneously, even in places which appeared to be well fitted for its reception. If, on its route, it encountered a large and densely-peopled town, it arrested itself there by preference; but the existence of a similar town out of the line of its course did not seem to be able to attract it, or to cause it to wander from its singular march. The exemption of the provinces to the east of the Volga might be perhaps explained by the nature of the country, in which the inhabitants are few and scattered; but the rich, fertile, and densely-peopled countries to the right of the Dnieper enjoyed an equal freedom from attack, which can only be explained by the fact, that they were situated beyond the line of the disease, which, as already stated, was principally between these development. May not this unindicated element be a peculiar, as yet, unrecognised atmospheric condition which accompanies the poison, or which even, by its meeting with certain local conditions, developes it? In London it may be supposed that this atmospheric condition has not yet been perfectly established, but that having been partially developed in the remarkable weather which ushered in the month of October, it has remained ever since stationary, or but slowly advancing to a more complete manifestation. In all these northern countries, to which the cholera-poison is foreign, it may be supposed that a particular atmospheric state is necessary.

Leaving, however, this conjecture—for at present it is little more—I may remark, in conclusion, that I have desired merely in this Report to apply the strict contagious theory to these early cases. I have decided that this theory cannot explain them. I have done so not merely on negative evidence that no contact or proximity between these early cases could be traced, but on positive testimony that such contact or proximity was impossible. These cases are, then, to be accounted for on other grounds; into these, however, I shall not at present enter: contenting myself with the remark, that it appears to me more probable that the cholera-poison should have entered the localities first infected by virtue of some peculiar force acting irregularly and partially, so as to drive the poison into one rather than into another locality;—or that the poison should have been actually generated in those localities under the influence of a general atmospheric condition co-operating with the local conditions proper to each place; —than that it should have reached those localities by direct emission from diseased individuals in the vicinity or at a distance, or from clothes which had been in contact with infected persons.

I feel, however, that, without enlarging my basis of inquiry, I have no right to push this argument farther. To do so with any effect, facts drawn from a more extensive series of observations would have to be passed in review.

Whether or not the evidence which has satisfied my own mind will prove as satisfactory to others, I do not know; but I can affirm that I have collected this evidence with impartiality, and have stated it with sincerity.

APPENDIX.

As far as we can at present judge from what is known regarding the spread of cholera in Great Britain, there appears to be much evidence in favour of the position that this extension has occurred in two ways, most commonly by independent manifestation in particular localities, but sometimes by transmission through the medium of diseased persons. Some excellent Reports published in the 'Edinburgh Monthly Journal,' indicate that in Scotland in some cases importation was improbable; but that, in a few cases, the disease appeared to arise from importation, and did really manifest contagious properties. In London, while I have no doubt that contagion had no influence in originating the disease, the cases arising after contact with the Tooting children seem to prove the fact of occasional contagion; as do also the cases recently put on record by Dr. McWilliam (Med. Gaz., June 15). If we are not disposed hastily to reject all the evidence on one side, and as hastily to receive all the evidence on the other, I do not see what conclusion can be arrived at, but that cholera is occasionally, but only exceptionably, contagious.

In the April Number of the 'American Journal of Medical Sciences,' is some interesting information respecting the appearance of cholera in New York and New Orleans. On the 9th of November, 1848, the packet ship New York left Havre, with 352 emigrants and 33 crew, for New York. There had not been a

rivers. Although communication was not interrupted, as the system of cordons sanitaires was abandoned by the government, no effect was produced by this cause. The most frequent intercourse with the dense population of the west, living under very unfavorable hygienic conditions, even the lowering of the temperature, which, it might be supposed, would have caused the extension of the disease towards more temperate climates, produced no effect."

single case of cholera at Havre when they sailed. They had no case on board till the 25th of November, fifteen days after leaving the land, when a young German, in robust health, was attacked with vomiting, purging, and cramps of the extremities; he died on the third day. On the following day, the 26th, another case occurred, and proved fatal on the second day. On the 27th, a girl died in two hours with the same symptoms. On the following day, a boy died in four hours and a half. On the 29th, a man died in seven hours. On the 30th, two more children died. On the 1st of December the ship came to an anchor at the quarantine station, Staten Island, and before the passengers landed on the 3d, twelve more cases had occurred. At the time there was no cholera on Staten Island. The sick emigrants were sent to an hospital—the healthy to some large public stores, in which were about seventy persons convalescent from other diseases. A person who had been treated at Staten Island for fracture of the patella, assisted in the removal of the sick; three days subsequently, he was attacked with violent symptoms of cholera, and died the same day. Another man, who had been in proximity with the emigrants at the stores, left Staten Island on the day after they landed and went to New York; he was attacked with cholera, was sent back to the quarantine ground, and died on the same day as the former case. Three other persons, who had been resident on Staten Island, and in proximity with the emigrants, were subsequently attacked. It does not appear that these last four cases had been in contact with the sick particularly; but they were the occupants of the storehouse to which those of the emigrants who were not affected with cholera had been sent. Two more persons were afterwards affected; making eight in all. No persons were attacked except those mixed up with the supposed healthy emigrants at the stores; 43 of whom appear to have been afterwards attacked. None of the crew were attacked. Among the emigrants, all the victims were Germans; of whom there were 270, all of them *long resident* in or near Havre. The disease then died away, and did not appear again either at the quarantine station, or at New York itself.

In this case there seems no doubt but that seven persons became affected with cholera after being in contact with a body of persons, some of whose number had suffered, and others were about to suffer from this disease. A still more remarkable fact is the origin of this attack at sea, fifteen days after leaving the land. It can hardly be supposed that it was derived from Havre, where cholera did not prevail; this supposition also necessitating the allowance of an unusually long incubative period. It is perhaps more probable to suppose it to have been actually generated in the ship, which, no doubt, furnished unhealthy hygienic conditions in abundance, under the influence of which, and of some peculiar epidemic constitution, the special cause may have been brought into being.

The second case referred to in the 'American Journal' is somewhat similar to this. It is given by Dr. Fenner.

On the 11th December, 1848, the ship Swanton, 39 days out from Havre, arrived at New Orleans with 280 German and French emigrants. About ten days previously, several cases of bilious cholera had occurred in New Orleans, which all recovered, and were probably unconnected with the subsequent epidemic. When the Swanton left Havre on the 2d or 3d of November, there was no cholera in that place. The vessel was out twenty-six days before any death occurred; the first was from consumption, afterwards sixteen or seventeen deaths took place, mostly from bowel-complaints, supposed to be dysentery. On the 12th of December, a woman from the vessel was taken to the Charity Hospital with undoubted cholera. On the 13th, another emigrant was admitted, and also died. *On the same day three other cases of cholera were admitted, all of which proved fatal; none of these were passengers of the* Swanton, *and they were from different parts of the city.* On the evening of the same day, December 13th, another suspicious case occurred in a resident, who had not been near the ship Swanton nor seen any of the passengers. On the 15th of December, 8 cases were admitted into the Charity Hospital, and several occurred in private practice. On the 16th, 11 cases were admitted, and the cases in private practice were rapidly increasing in number. By the 22d, forty-five deaths had occurred in the city, and after this the epidemic rapidly extended. It was stated that the first three victims were cooks, who went every morning to the principal market, within a cable's length of the vessel; but this was erroneous, as the Swanton was nearly a mile away, and these cases occurred on the 15th, after several other cases had been noted. The disease reached its zenith about the 28th, and declined steadily from the 1st of January.

After the disease appeared, almost every vessel that left the city soon had cases aboard; persons having the disease and dying of it, were carried to all the landings, towns, and cities up the Mississippi, as high as Cincinnati. In many of these places it spread to a limited extent; in others it did not; in no place did it prevail as an epidemic. It spread among the plantations along the river and in the interior of Louisiana. To some of these the infection appeared to be directly carried; at others it began without any communication with an infected district. It prevailed at Houston, Texas, while Galveston on the sea-board escaped, although on the line of travel from New Orleans to Houston. At the Charity Hospital, as many as 50 cases occurred among the nurses, servants, and patients.

Some other facts are well worthy of attention. The Guttenburg left Hamburgh in October, and six or seven deaths occurred before she got out of the Elbe. When she got to sea the disease disappeared, and did not again return. So that in this case, getting to sea out of the infected district arrested the disease.

The analogy between the cases of the New York and the Swanton is certainly remarkable: both vessels left a port in which no cholera case was known, with emigrants; the time of departure was nearly the same, viz. the Swanton on the 2d or 3d of November, the New York on the 9th; both were bound to different ports of the same country, and therefore probably followed nearly the same track; in the Swanton, which sailed on November 2, cholera appeared after twenty-six days, therefore on or about the 28th of November; in the New York it appeared on the 25th of November. In these cases it appears possible that both vessels may have been nearly in the same position, when they were attacked by cholera. Considering, then, all the circumstances of the case, the departure of those ships from a healthy port, the length of time they remained free from sickness, their possible vicinity to each other when they were attacked, the fact of their being emigrant ships, and therefore presumably badly cleaned and ventilated, it does not appear improbable that both ships became nearly at the same time subjected to some peculiar influence, which caused in both cases an analogous and independent manifestation of the cholera poison.

Dr. Fenner, the reporter of the New Orleans epidemic, says, "Whether it be a mere coincidence that epidemic cholera broke out in this city, just at the time when a vessel arrived, having some cases of cholera on board, or that said vessel brought the infection, which rapidly spread through the whole community, is an exceedingly debatable question." And at the end of his Report he remarks, that the few facts he has brought forward do not decide about the contagiousness or transportability of cholera. It would seem, therefore, that the channel of introduction, or the reality of actual importation of cholera, into New Orleans, may yet be a matter of dispute.

PART FOURTH.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Intimate Structure and History of the Articular Cartilages. By Dr. Joseph Leidy.

This paper contains a minute description of the arrangement and mode of multiplication of the cartilage-cells; the groups of which are stated to lie perpendicularly in the deeper part of the cartilage (that nearest the attached surface), and obliquely or irregularly as they approach the free surface, whilst *at* and near that surface they lie parallel to it. The deeper groups are composed of a larger number of cells than the superficial; and in the stratum forming the free surface, single isolated cells are not unfrequent. The free surface is covered *in the fœtal* state by a synovial membrane; of which the two characteristic elements—basement-membrane and epithelial cells—may be clearly recognised. But after birth, it appears to be gradually destroyed by pressure and attrition, and by the retirement of the superficial vessels towards the circumference; and appears in the adult to terminate at the margin of the cartilage, a very little in advance of the *circulus articuli vasculosus*. The phenomena of the multiplication of the cartilage-cells by division are described as closely resembling those of cell-multiplication in the simpler Algæ; and favour the idea that the cell has a protoplasmatic envelope, or primordial utricle, within the regular cell-wall, from which it seems to retire when the constriction that leads to the duplication first shows itself. The division of the nucleus does not seem to be essentially connected with that of the cell itself.—The matrix of the cartilage-cells is asserted by Dr. Leidy to have a distinctly filamentous arrangement, each filament consisting of a row of very fine granules. These are best brought into view by tearing a very thin shred from the fractured edge of an articular cartilage. The filaments, like the cell-groups, are arranged perpendicularly to the attached surface and parallel to the free surface, where they form a sort of membranous layer that holds together fractured portions of cartilage.—*Amer. Journ. of Med. Sciences*, April 1849.

On the Development of the Purkinjean Corpuscles in Bone. By Dr. Joseph Leidy.

Dr. Leidy's observations confirm the idea of Schwann, that the bone-corpuscle, or lacuna, is truly a cell, of which the canaliculi are radiating prolongations, analogous to those of the pigment-cells; and he further thinks that this cell is one of the residual cartilage-cells. The following is his account of the process of ossification, as observed in the frontal bone of a human embryo of two inches in length. "After the primitive ossific rete has been formed from the deposit of the osseous salts, inclosing groups of cartilage-cells in the areola, the further deposit takes place in a fibrous or line-like course from the parietes of the areola of the primitive osseous rete, to the interspaces of the cartilage-cells nearest to, or in contact with, the sides of the areola. At this period, the cells shoot out or extend their canaliculi between the fibrillæ just formed, and then the cell-wall and continuous walls of the canaliculi fuse with the translucent, homogeneous, or hyaline substance of the cartilage existing between the cells and the osseous fibrillæ, and with the fibrillæ themselves, by the deposit of the osseous salts. The period of the formation of the canaliculi appears to be quite definite, occurring during the deposit of the osseous salts, and not before; to such an extent is this the case, that I noticed in several instances, cells which had formed their canaliculi upon the side which was ossified, while upon the other side I could not distinguish any trace of them.—*Proceedings of Academy of Natural Sciences, Philadelphia,* Nov. 1848.

On Hæmatozoa in the Rat. By M. CHAUSSAT.

THE occurrence of worms in the blood has hitherto been accounted a rare phenomenon; but M. Chaussat states that he has found *filariæ* in the blood of all adult *black* rats which he has examined. He has not found them in the blood of young rats. He has recognised the eggs in the liver; where M. Lebert has observed them in the rabbit also.—*Gazette Médicale,* No. 13.

On the Arrangement of the Muscular Fibres in the Vena Cava Inferior of the Horse.
By M. CLAUDE BERNARD.

IT is asserted by this able experimenter that prussiate of potass, introduced into the stomach and absorbed by the radicles of the vena portæ, is not conveyed to the heart, but is directed towards the kidney by a sort of reflux in the inferior vena cava; and he finds this experimental result corroborated by the peculiar structure of that vessel in the horse. From the hepatic to the renal veins, there extends a ring of muscular fibres (non-striated), very apparent to the naked eye, composed of longitudinal fasciculi, which terminate abruptly above the entrance of the hepatic vein, and below that of the renal. Such a structure appears well adapted to produce the supposed reflux.—*Gazette Médicale,* No. 17.

On the Direction assumed by Plants. By PROFESSOR MACAIRE, of Geneva.

THE author first examines experimentally into the causes of the *Curling up of Tendrils;* which Knight endeavoured to explain by the unequal action of the light on both sides of the tendril, and which was attributed by Decandolle to the obstacle afforded to vegetation by the contact of the leaf-stalk with the body adhered to, on the side where it touches it. Of the plant on which he experimented, though a common weed in the gardens of Switzerland, he is unable to give the name with certainty; it was probably the *Smilax aspera.* He found that when the tendril is touched by any solid body whatever, on a point of the surface not too far from the extremity, it immediately contracts on one side, so as to form a curve over the surface of the body and to embrace it closely, until seven or eight coils have been formed around it; and this is done so rapidly that three turns of the helix are sometimes made in a quarter of an hour. The nature of the body presented has no influence on the process; the tendrils coiling as readily over one substance as another, so that they will cling to other portions of the plant from which they spring, though obviously destined to obtain support for it from others. As these and other phenomena cannot be accounted for by any action so slow as the ordinary processes of nutrition, it appears necessary to admit the existence of irritability as a vital property inherent in the tissues of the tendril; this property is found to cease when the tendril is separated from the parent plant; and, like the irritability of sensitive plants, it is excited, modified, or even suspended or destroyed, by the influence of vegetable or mineral poisons.

The next subject examined is the *Inclination of Stems towards the Light;* which was attributed by Decandolle to the more rapid and more complete solidification of the tissue, by exhalation and fixation of carbon, on the side of the stem exposed to light. Professor Macaire first inquires if such a special attraction is exercised by light on the green parts of a plant, as to cause the entire plant to move towards light if permitted to do so; and his experiments on plants of duckweed, and on germinating plants of various kinds attached to floats of cork, lead him to a negative conclusion. He found that, however long it might be necessary for a stem to grow, in order to reach the light, its base, attached to the cork float, always remained in the same spot. In one instance, a germinate seed of mustard having been placed on a float in a tumbler surrounded by dark paper, but near an aperture admitting luminous rays, the plant put forth a stem which passed all round the tumbler, to spread its leaves in that part of the vessel in which the luminous aperture was; once there, the stem did not extend itself beyond it, but grew erect, although the light was not strong enough to render it entirely green. Thus, although a very slight motion of the float would have brought the entire plant within range of the light, its position remained altogether unchanged, whilst the stem went, as it were, in search of it. The observations of Professor Macaire are opposed to the hypotheses of Decandolle in this case, as in the preceding; since he found that the stems grew *straight* towards the light, without the incurvation or bending which that hypothesis assumes. Where young plants already vigorous were placed on the floats, in the dark portion of the vessel, their green stems took on little or no ulterior development; but from the neck of the root there grew out another stem, white and etiolated, which spread itself along the water to reach the diaphragm and the light portion of the vessel, where it grew erect and put forth its leaves.

The next subject examined is the *Direction of Leaves;* that is, the tendency of those which have two surfaces of different hues to expose the deeper coloured to the sky, and the paler to the earth. Professor Macaire's experiments lead him to the same conclusion with that of other physiologists, that light is the only agent in the turning over of leaves, and that it does not act by a physical attraction properly so called, but by its influence upon the individual parts of the tissues on which it falls. This influence is the more rapid and energetic, all other circumstances being alike, the greater the difference between the two surfaces of the leaves experimented on. It was maintained by Bonnet and Dutrochet that the turning over of leaves always takes place by a flexion or torsion of the foot-stalk; but Professor Macaire has demonstrated that the flat portion of the leaf, or even a separate portion of it, can turn itself over. Thus when an entire branch of geranium was immersed in water in such a way as to expose the under surface only of its leaves to the light, all the young leaves turned themselves over in three days by moving on the point of insertion of the flat part of the leaf into the foot-stalk; and in other experiments in which the light was kept from falling by means of a screen on the upper surface of leaves, and was directed by means of a mirror towards the lower, the margins of the leaves bent down in such a manner as to bring their upper surface within the influence of the mirror. Upon repeating such experiments with glasses of different colours, it was found that the leaves turned themselves over most readily in *blue* rays, and next in *violet;* but that they remained motionless in *red.*

Professor Macaire then inquires experimentally how far these results are attributable to the influence of light on the nutritive functions in which the leaves are concerned, and comes to the conclusion that their explanation is to be sought here. He found that the exhalation of fluid from the leaves is always greatly augmented by the exposure of their under surfaces to light, the increase being double, triple, or even more. It is obvious that this is one principal cause of the unhealthiness of leaves which results from the inverted position being forced upon them. Another cause is to be found in the diminution of the rate of decomposition of carbonic acid, which takes place under the same circumstances, and to about the same extent. According to Professor Macaire, the exhalation is greater under blue glass than it is in diffused light, and the difference between the amount of exhalation from the upper and under surfaces of the leaves respectively is most strongly marked; on the other hand, the amount of exhalation under red glass is reduced to about a sixth, and the difference between the quantity exhaled from the two surfaces of the leaves is proportionably lessened.—*Philosophical Transactions,* 1848.

Microscopical Examination of the Contents of the Hepatic Ducts, with Conclusions founded thereon. By T. WHARTON JONES, F.R.S.

IN the contents of the larger hepatic ducts there are seen, on microscopical examination,—1st, the detached columnar epithelium-cells of the ducts themselves; 2d, free oval nuclei, which appear to have been derived from cells of this kind; 3d, free round nuclei, apparently derived from the proper hepatic cells; 4th, minute granules, free or in amorphous flakes, globules of oil, and fragments of cell-walls. In addition to these, in the smaller ducts are found cells resembling those of the parenchyma of the liver, except in being usually paler, on account of the contained granules and globules being fewer and more minute.—Having, by way of comparison, examined the contents of the pancreatic duct, and recognised in it, with its own columnar epithelium, the free nuclei and granular amorphous matter resulting from the disintegration of the secreting cells developed within the vesicles of that gland, the author considers that, from the analogy of the products of the secreting action in the two cases, an argument may be fairly drawn in favour of the analogy in essential nature between the cells of the hepatic parenchyma and the endogenous cells of the pancreas and other glands of ordinary construction. This deduction helps to confirm the view of the structure of the liver propounded by Dr. Leidy, of which an account was given by us not long since.—*Phil. Trans.*, 1848.

On the Artificial Production of Diabetes. By M. CLAUDE BERNARD.

IT is asserted by M. Claude Bernard that, by passing a sharp instrument into the fourth ventricle, the urine is speedily caused to present a saccharine impregnation, its other constituents remaining unchanged. His first experiments were made upon rabbits; but he has since performed the operation upon a dog, in whose urine a notable quantity of sugar was found at the expiration of twenty minutes, although none existed there previously.—*Gazette Médicale, 2 Juin*, 1849.

On the Composition of the Salts of the Blood, and of their Relations to the Formation of Vesical Calculi. By M. VERDEIT.

THE author states that the proportions of the different saline compounds in the blood are greatly affected by the nature of the diet; the blood of animals nourished exclusively on flesh being rich in phosphates, with alkaline bases, and containing scarcely any alkaline carbonates; whilst the blood of animals restricted to vegetable diet presents the reverse condition, the alkaline carbonates being present in large amount, and the phosphates existing in it in very small quantity. These differences present themselves not only between the blood-salts of carnivorous and of herbivorous animals respectively, but also between those of the same animal nourished for a time upon an animal and a vegetable diet exclusively. Thus the blood of a dog fed upon meat for eighteen days yielded 12·75 parts of alkaline phosphates in 100 of ash; but when it had been partly fed for fifteen days upon bread and potatoes, the proportion fell to 9 per cent.; "and if," says the author, "I had been able to sustain it on an exclusively vegetable diet, the amount would have fallen to 2 or 3 per cent., as in the blood of the ox or the sheep." These observations, if confirmed, will have a very important bearing upon the treatment of calculous disorders, since urinary deposits consist, in by far the greater number of cases, either of uric acid or of phosphates. Now, uric acid is sparingly soluble in water, and still less so in acid urine [this does not agree with the statements of Liebig, which have been confirmed on this point by others]; but it is rendered much more soluble by the presence of alkalies in the urine. Now, as the acidity of the urine is due to the excess of phosphoric acid, which is the result of an animal diet, a vegetable diet, which will render the urine alkaline like that of the herbivorous animals, is indicated. On the other hand, if the phosphates constitute the materials of the deposit, their amount in the blood and urine may be lowered in the same manner.—*Gazette Médicale, 2 Juin*, 1849.

PATHOLOGY AND PRACTICE OF MEDICINE.

On Crises and Critical Days. By M. Bricheteau.

M. Bricheteau testifies in the present paper to the great benefit he has frequently derived during a long practice from the observation of crises as inculcated by Hippocrates. "Several times has it occurred to me to suppress a prescription too hastily written, on learning that an abundant sweat or a deposit in the urine has occurred in the night. If asked why the doctrine of crises is now of so little account, we may reply with Van Swieten, that physicians in full employment, who alone have the opportunity of observing them frequently, are so occupied by the great number of patients they have to visit, as to be unable to keep an exact account of the symptoms and progress of disease; and the patients themselves often force their attendants into an erroneous course, and oblige them to deviate from the track pointed out by the masters of our art, by their impatience to get well and their astonishing propensity to swallow medicines. The sad truth, indeed, must be told, that it is far easier for a practitioner to bleed or purge his patient after a rapid examination of the tongue and pulse, and to prescribe a variety of internal and external remedies from the indications of certain symptoms, than it is to observe day by day the successive phenomena of disease, discuss their value, and withhold the hand in presence of critical changes, in spite of the observations of the invalid or his friends! To a certain point the very progress of the medical art itself has been inimical to the reception of the doctrine of crises. Our improved means of diagnosis renders the study of general symptoms, the conditions of the various apparatuses secondarily affected, and the indications which these may furnish of the resolution of disease, less necessary than heretofore. . . . To exhibit the importance of the observation of these crises, it suffices to mention the advantages derivable from sparing the patient the useless administration of medicines, the application of painful revulsives, or the employment of drugs capable of deteriorating their organs, enfeebling their powers, and retarding their convalescence. Who is there unaware of the incendiary ravages of medicines when given without discernment? The most penetrating mind fails to seize the natural course of the disease amidst the accidents produced by a perturbatory medication."

According to Galen, a crisis is a change which occurs in the course of a disease, whether for good or evil, conducive to cure or death. It is in fact a change. It is generally favorable, announced by precursory phenomena, and characterised by abundant excretions, hemorrhages, &c. Although showing itself generally as a consequence of the administration of medicine, this is not its origin, as it may present itself when no medicine whatever has been given; and most of the ancients attributed the phenomenon to the spontaneous action of the *vis medicatrix*, effecting an advantageous modification in the economy. If the change be only a result of the re-establishment of excretions suspended at the commencement of the disease, of the re-establishment of a general equilibrium, or of a return to the normal condition, and if in place of being, as the ancients supposed, an active cause it is only a result, it matters little, since it is still an essential and integrant part of the disease, which, if the account of a case is to be rendered complete, is always to be noted when it presents itself. It has been pretended that, in order that a crisis may be entire and complete, a rigorous expectancy, dangerous to the life of the patient, is essential; but this is an erroneous view of the subject, since Hippocrates and Galen employed various internal and external medicinal agents to aid nature when too enfeebled, and to develope imperfect crises.

The days on which crises are determined, are called critical, and the others non-critical. The regarding as critical too great a number of days, has greatly exaggerated the difficulty of applying the doctrine of crises to the bedside of the patient. The principal critical days were considered to be the 7th, 14th, and 20th. Others, as the 9th, 11th, and 17th, were of a secondary rank, while the 3d, 4th, and 5th occupied a still inferior position; and the 6th had so bad a reputation as to be termed the tyrant. At the 8th and 10th were sometimes produced abortive crises, while none were exhibited on the 12th, 16th, and 18th. The 7th day has always been considered the critical one *par excellence*. The days of inferior critical power were said to be indicators or precursors, announcing by an amelioration or precursory phenomenon a perfect crisis on one of the three radical days. Thus the 4th is the indicator of the 7th, or, in the words of Hippocrates, those whose ailments will determine on the 7th day have an abundant urinary deposit on the 4th. The 11th is the indicator of the 14th, and the 17th of the 25th. These indicating days sometimes themselves furnish crises, and become exceptionally radical. M. Bricheteau gives the abstracts of many cases in which, by attentive observation of these various days, he has been led to their more satisfactory treatment."—*Rev. Med.-Chir.*, t. iv, pp. 259-67.

On Eruptive Fevers without Eruption. By M. TROUSSEAU.

SOME practitioners doubt the accuracy of the facts related by some of the great observers of former times, concerning the existence of eruptive fevers without eruption. They have been especially considered as apocryphal in our times, when the localization of disease plays so large a part in the nosological systems. But when we consider that all pathologists admit that these diseases attack the mucous membranes as well as the skin, it is obviously improper to consider them as diseases of the skin; and in many cases we observe the eruption to be so very slight, that we can very well imagine it may in others be absent, without the necessity of denying the existence of the disease.

In illustration of such occurrence, Dr. Trousseau details the history of a Lilliputian epidemic that occurred in a retired hamlet near Paris. The hamlet only contained three families, of twelve inhabitants, none of whom had had scarlatina. It appeared in a mild form; every one of them was ill, but in different degrees and forms. Eight persons had the rash and angina in their most distinct forms, but four suffered only from severe fever, with angina and desquamation of the tongue. In these the convalescence was tedious, especially in one case wherein anasarca followed. Who can doubt that these four were likewise subject to scarlatina, although, had they been presented to the observer in an isolated manner, he being unacquainted with their antecedents, the nature of the case would have been overlooked.

Two facts which have recently occurred in the hospital alike serve to show the possibility of the occurrence of eruptive fevers without eruption, and the difficulty of judging correctly concerning such cases when seen in an isolated condition. A boy, æt. 7, was brought in suffering from croupal cough, laborious respiration, and high fever; an emetic and some small doses of calomel were given, and next day the character of the cough was much better, but there was still much fever and oppression, and auscultation indicated a capillary bronchitis. As the eyes were observed to be somewhat injected and weeping, the tongue red at the tip, and a little blood plugged the nostrils, it was deemed probable that it was a case of measles, but it was only by very diligent search some rose-coloured non-prominent spots could be detected on the forearms. On compressing the arm, so as to inject these more, their measly character became evident. In a few hours they entirely disappeared, never more to return, "though the entire body of the child was whipped with nettles twice in one day (!)." It is evident that the diagnosis of this disease could not have been made had not the arms been examined just when they were, and that even this small manifestation would not have taken place had the arms been exposed to the cold, instead of enveloped in a warm dress. About the same time a child, æt. 2, was brought in, suffering from a severe form of pleuropneumonia, having been ill for eight days. The next day an eruption of measles spread all over the body. No physician, let his sagacity be what it might, could have recognised the measles the first day, and yet it is evident that in this child, as in the other, the rubeolar eruption had predominated on the pulmonary mucous membrane, so as to produce an antagonistic fluxion, preventive or retardatory of, or diminishing, the cutaneous eruption. Such antagonistic fluxions may not unfrequently suffice to prevent all cutaneous eruption, and then we have eruptive fevers without eruption. In scarlatina it is to the mucous membrane of the pharynx and mouth that the critical phenomena are diverted.—*L'Union Médicale,* No. 54.

[We were lately in attendance upon a little boy, in whom the eruption of the skin and the angina were so slight, that we merely at first conjectured the possibility of scarlatina, although subsequent desquamation and the peculiar depression, such as is produced in some children by the presence of its poison in the blood, enabled us afterwards to be quite certain on this point. A brother of the above, æt. 5, from being an active, lively lad, became a moping, dull one, suffering much from lassitude and anorexia. Repeated careful examination discovered not a trace of eruption or angina, and no suspicion of the possibility of scarlatina could have been excited but for the teaching of the other case. Nevertheless in a week or two's time, decided anasarca with albuminous urine manifested itself, requiring a long course of aperients, tonics, and baths for its removal.]

On Blue Pus. By M. OLIOLI.

THE case furnishing the blue pus occurred in the person of a woman in one of M. Maisonneuve's wards. She had formerly been operated on for a cancerous breast, and now returned with a reproduction of the disease. Canquoin's caustic was applied, and after the eschar fell off, the pus discharged was observed to be of a greenish-blue, and that whether medicinal applications were made to the part or not. M. Olioli had seen a similar case at Turin, when, after amputation, blue pus was produced, though only simple dressings were employed. M. Bouchardat analysing the pus in the present case, found in it an *organic colouring matter of unknown nature,* but no other principle capable of explaining the colour. Two cases are recorded in the 'Gazette Médicale' (1831 and 1834), in which the serum of a blister was coloured blue. MM. Persoz and Dumas conjectured that, in certain ill-conditioned suppurations, hydrocyanic acid might be generated, and subsequently a compound analogous to Prussian-blue formed; but subsequent chemical analyses give no support to this conjecture. In some researches which M. Conte made concerning the composition of this pus, he also found it containing a *peculiar organic colouring matter.—Gazette des Hôpitaux,* No. 47.

[In a well-marked case of the discharge of blue pus, recently occurring at Dublin, the analysis of Dr. Apjohn led him to believe that the colour very probably, though not certainly, depended on the presence of Prussian-blue.—See *Dub. Med. Press,* April 1849, p. 263.]

On Cephalæmatoma. By M. CHASSAIGNAC.

M. CHASSAIGNAC believes that whatever operation is performed for the relief of this affection, it proves very hazardous to the infant, and he takes the occasion of a recently occurring case, which he watched closely during life, and examined after death (from other causes), for drawing attention to the remarkable activity of the absorbent power at this age, and to the mode in which Nature thus operates a spontaneous cure. The sanguineous effusion gradually becomes encysted, though for a few days the blood is in direct contact with the bone and periosteum; an organic membrane separated into two layers, and which afterwards takes on all the characters of periosteum, then surrounds it, and the absorption proceeds. In proportion as the quantity of effusion diminishes, the osseous rim or border which surrounds it also diminishes in diameter, by reason of a double simultaneous action, viz. resorption at its external circumference, and a concentrical reproduction at the inner one. Very voluminous tumours in this way disappear; and so great is his fear of exciting suppuration in them, that M. Chassaignac strongly advises all interference to be withheld, save in the extremest cases.

M. Debout agrees with M. Chassaignac, that in the great majority of cases absorption will take place during the first six weeks of life; but after this period sometimes a serous cyst is formed, containing a reddish fluid, very like that found in mammary cysts the results of contusions. This cyst generally inflames and suppurates, giving rise to necrosis and fever, which may be prevented by opening the cyst. This operation, however, may be followed by erysipelas, or the formation of abscess, and M. Trousseau's procedure offers the greatest security against these accidents. With a narrow bistoury, so held as to cut from within outwards, he makes a puncture at the lower part of the tumour, and endeavours by pressure to empty it of its entire contents, and then applies strips of diachylon and a bandage, so as entirely to exclude the access of air. In eight or ten days these are removed, and all is found to be doing well.—*Bulletin de Thérapeutique,* t. xxxv, pp. 448-55.

Dr. Vogler, writing upon this subject, observes, that he is an example of how careful medical men should be in drawing general deductions from even a quarter of a century's experience. Thus, a very short time since he published a paper, in which he stated, that during this space of time he had never had occasion to open a cephalæmatoma, having always succeeded in dispersing the tumour by lotions of sal ammoniac, alcohol, and water, in from three to six weeks. Yet since then has he, in the course of only one year, met with three cases in which he felt called upon to open the tumour. In the first case, he did so because a severe ophthalmia in a child but a fortnight old forbad the use of wet applications. After the blood was discharged, suppuration was set up, but led to no bad consequences. The second case occurred in a child half a year old, apparently as a result of contusion. The swelling had all the appearance of a deep-seated abscess, but on an incision being made pure blood flowed out. A somewhat tedious suppuration followed, but all did well. In a third case (æt. 15 days), the swelling was very tense, and as the parent neglected applying the lotion, the author opened the tumour and discharged the blood. Very insignificant suppuration followed.—*Medicinische Zeitung,* 1849, No. 13.

On Melæna in Children. By M. Rilliet.

M. Rilliet, one of the authors of the classical work on the 'Diseases of Children,' takes the occasion of two cases of intestinal hemorrhage in children, which have come under his notice, to furnish a summary of what is known upon the subject. The cases he relates are unique, inasmuch as they are examples of this rare disease occurring in twins. The first child, though not large, was well formed, and had cried strongly after birth. The meconium had been discharged by castor-oil; the child had sucked, and seemed in all respects doing well, when several hours after birth it passed two or three stools wholly composed of blood, and became deathly pale and cold, and was unable to swallow. It cried, but there was no vomiting or convulsion. Cold compresses were applied to the abdomen, the warmth of the extremities being maintained; rhatany was employed both in injections and external applications, but the former being always returned with blood, the latter were alone continued. The child gradually rallied after six bloody stools had been passed. While this child yet continued in an alarming state, the other twin was seized with vomiting and purging of blood, and decoction of rhatany was in the same way applied to its belly. It recovered, though the bleeding continued more obstinate. Both children remained well, though long continuing exceedingly pale.

Different authors have ascribed the affection to various causes, but an analysis of the published cases leads Dr. Rilliet to doubt the efficacy of most of those assigned, e. g. the condition of the health of the parents, the nature of the labour, the too early division of the funis, the weakness or plethoric state of the child, the presence of irritating matters in the intestines, external violence, or the rupture of vessels; and although the affection usually occurs at from the first to the fourth day, it has been met with as late as the sixth, eleventh, or twentieth, boys seeming more liable to it than girls. Dr. Rahn-Escher, of Zurich, believes that much depends upon hereditary influence, a diseased state of the digestive organs being communicated from parent to child. This view would seem to be best supported in those cases where the parents also suffer from hemorrhages, or where several of their children do so simultaneously or successively. Billard states that the majority of fifteen children dying from it, were remarkable for plethora, but the experience of others does not bear out this view. Numerous pathological observers agree in stating that there is no important vascular lesion, the mucous membrane being sometimes not more injected than natural; others have found the abdominal veins gorged with blood, and the mucous membrane softened and reddened. The naturally injected state of the intestinal tube in the child may act as a predisposing cause when there is atony of its vessels, or obstruction to the abdominal circulation. The difficulty with which respiration is established at birth, especially if the lung is incompletely dilated, may exert an influence.

The bleeding in most cases has followed the meconium, but unpreceded by any premonitory symptom. It is usually abundant and frequent, rich in colour, and either fluid, or in part coagulated. Hæmatemesis, though more rare, may yet be abundant; and Etlinger gives a case in which a pound of blood was discharged by vomiting and stool. Sometimes the hæmatemesis is more abundant than the intestinal hemorrhage, but it is always accompanied by the latter. The hemorrhage may attain its maximum in twenty-four hours; but it may be prolonged to the third or fifth day, and in very rare cases to the tenth even.

Of 23 cases in which the issues have been recorded, 12 recovered and 11 died. In 9 of the 12 recovery seemed complete, but in 3 the constitution was deteriorated. In treating the disease, sometimes mere demulcents, at others astringents, the mineral acids and the internal use of cold, and in others applications to the belly and enemata, have been tried in different cases. M. Rilliet believes that the best treatment consists in the free exposure of the child to the air, the keeping its extremities warm at the same time, the application of astringents to the abdomen, and the careful administration of nourishment, or, if the child's condition requires it, of stimuli,—*Gazette Médicale*, 1848, No. 53.

On Catarrh of the Mouth. By Dr. Pfeufer.

Dr. Pfeufer opposes the prevalent idea, that the appearances of the tongue indicate the state of the stomach, and that its loaded state or perverted sensibility shows the existence of gastric affection. He denies that it undergoes greater changes in the diseases of the stomach than in those of any other part of the body. The stomach may be most extensively diseased by cancer, in which hyperæmia or blenorrhœa may be present, and yet the tongue continue clean and the appetite good, as also may be the case in chronic inflammation of the organ. The tongue may be loaded, and the appetite lost, in that catarrhal state of the mucous membrane of the stomach termed *gastricismus*, but it is by no means rare to find this not the case. The appetite may be gone, and the tongue be quite clean.

On the other hand, changes may occur in the cavity of the mouth without being at all indicative of disease of the stomach; the tongue may be thick and loaded, an indifference to food exist, and the taste be blunted and pasty, yet whatever the person does take produces no gastric suffering. The affection is, in fact, a *catarrh of the cavity of the mouth.* It may come on quite suddenly under the influence of mental emotion; it accompanies a variety of affections of the mouth and throat, and may be excited by various stimulating articles of diet, or by medicines acting as local irritants. It sometimes passes away in a few hours, and at others lasts for days, and seldom requires treatment, and certainly not the use of emetics and purgatives, which are usually resorted to, the diligent rinsing the mouth with cold water hastening its removal. Occasionally, however, the affection lasts for weeks or months, and then is usually treated by a great variety of medicines as an obstinate dyspepsia; those who can afford it being at last sent to the Spas. These means, proper enough in catarrh of the stomach, are here useless; and with a gargle, composed of a grain or two of corrosive sublimate in a pound of water, the author has several times cured in a few days a disease that had resisted the long train of antidyspeptic medication for months. Local sources of irritation, too, must be sought; for it has more than once happened to Dr. Pfeufer to relieve cases, by the removal of some sharp fragments of a tooth, which had been subjected to all the approved modes of treating *gastricismus*, and had resisted these, as well as long courses of mineral waters and the like! An acute form is also sometimes very obstinate, recurring again and again, and giving rise to loss of appetite. It is often very analogous to nasal catarrh, with which it is sometimes conjoined, and then the affection of the mucous membrane is primary. In other cases, however, and especially when produced by mental emotion, and when it is so often erroneously attributed to "bile," there is apparently a primary affection of the nerves of taste and sensation of the tongue, and then the catarrhal secretion is secondary. —*Henle und Pfeufer's Zeitschrift*, Band vii, 180.

Case of Obliterating Phlebitis of the Vena Porta. By M. MONNERET.

The patient, æt. 42, who formed the subject of this case had suffered, while residing in his youth at the Antilles, from yellow fever, dysentery, &c.; but his health of late years has been tolerably good, save occasional diarrhœa. On the 20th of September he brought up, without obvious cause, nearly a quart of black, coagulated blood, and suffered afterwards from bloody stools. His health gradually deteriorated; he became dropsical, and died 25th October; the superficial veins of the abdomen being in a varicose state. On examination, the liver was found to be so much atrophied as only to equal the two fists of the subject in size. It was of a uniform yellow colour, but not granular; only the principal branches of the vascular system of the organ were visible. The vena porta was much dilated in all its chief divisions, which were filled with a highly resistant coagulation, evidently formed long prior to death, being in some parts whitish, dry, and only removable by the aid of the scalpel. On examining the lining membrane of the vessel, to which it so firmly adhered, this was found to be rugous, deprived of its natural polish, thickened in some parts, and thinned in others. The middle coat of the vein, in its colour, consistence, and thickness, exactly resembled that of the aorta, presenting in several places the same yellow colour as a diseased artery. Glisson's capsule was hypertrophied, and of a whitish colour. The gall-bladder was small, white, and thickened, in some parts fibro-cartilaginous, and contained two calculi and a little bile. The choledochus was very narrow. The vena cava was free from obstruction. M. Fauconneau-Dufresne states that, as far as his investigations have gone, this is only the eighth case on record.—*L'Union Médicale*, 1849, No. 13.

On Angina Pharyngea. By M. VELPEAU.

THERE are distinctions in this disease, based on the different tissues affected, which it is important to bear in mind. In the first and more frequent variety, the free surface of the mucous membrane is inflamed, the inflammation spreading over the whole pharynx. In a second the inflammation occupies the tonsils and the mucous membrane in all its reduplications. The third variety is formed by the inflammation of the cellular tissue covering the external surface of the tonsils. In the first form the disease comes on suddenly, but rarely lasts long. It produces redness, but not swelling, and is frequently unattended with fever. It may be often arrested by local application, as alum; and if general treatment, such as bleeding, vomiting, or purging, is indicated, the disease is generally cured in two or three days. In the second form, alum would only exasperate the disease, and general means only slightly modify it. Left to itself, it may last from six to twelve days, often terminating by suppuration. Emetics are only useful in assisting the abscess in breaking. It is an affection of frequent occurrence, but is rarely dangerous even when both tonsils are much swollen.

The third form, *cellular angina*, is a more serious affection. A dull, permanent pain comes on, which at first, not extending to the inner side, does not interfere with deglutition. Owing, too, to the seat of the disease, the tonsil does not swell, but the whole of the affected side, tonsil, and soft palate, is raised by the swelling pushing up the various tissues. There is inflammation also of the corresponding cervical region, the parotidean and subhyoidean depressions becoming filled up with a red, tender, doughy tumefaction. This form of the disease may last from eight to thirty days, and terminate by any of the ordinary consequences of phlegmon. Leeches to the angle of the jaw, and other means of an antiphlogistic character, are indicated, and not unfrequently recourse is obliged to be had to the bistoury to prevent the injurious burrowing of matter; but the danger of wounding some of the important parts situated in this region often obliges us to hesitate. M. Velpeau alludes to a case in which he was about to open an abscess by incision into the pharynx, when, owing to the objections of the patient, he was induced to apply large flying blisters to the angle of the jaw, at the base of the inflamed part, and this was attended with the most complete success in dissipating the inflammation.—*Gazette des Hôpitaux*, 1849, No. 12.

Epidemic of Rheumatism.

DR. HARTZ furnishes an account of a small epidemic of this disease, which prevailed at Kayserberg from the 1st of March to the 15th of April, during which time he was called to no less than 52 cases in a not very populous district. These were composed of 41 men and 11 women, from the ages of 17 to 38. With two or three exceptions, they were all persons enjoying generally good health, even of robust constitutions, and of sanguineous temperaments. In almost all, the attack came on suddenly, without apparent cause, or other than taking cold, usually assigned as the cause by the patients themselves. In a third part of the cases the disease simulated a pleuropneumonia, from which it was distinguished by auscultation.—*Gazette des Hôpitaux*, No. 53.

SURGERY.

On Scapulalgia. By M. MALGAIGNE.

A SURVEYOR was attacked while employed in the country with a violent pain in the left shoulder, which continued very intense for forty-eight hours. Cupping and other means giving him very little relief; he came to the hospital supposing himself suffering from rheumatism. He kept his arm close to his side, and all attempts at moving it thence caused dreadful pain in the shoulder, where, however, there was little swelling and little tenderness, save at a *circumscribed spot* opposite the anterior part of the head of the humerus, where, on M. Malgaigne making pressure, the patient cried aloud. From this sign, together with the fact of no other joint being affected, M. Malgaigne declared that the case was not one of rheumatism but of *scapulalgia*, i. e. a simple arthralgia of the shoulder-joint. He ordered complete immobility of the parts to be secured by means of a clavicle-bandage, and for the first time for seventeen days the patient obtained sleep. At the end of ten days the bandage was removed, and the movements of the limb were found as painful and as difficult as before. He could only sleep by lying on the opposite side; and from the prolonged flexion, contraction of the biceps and rigidity of the elbow had been produced. M. Malgaigne sought for the tender point which had served him as an indication of the inflammatory stage of the scapulalgia; but repeated pressure against the anterior portion of the head of the humerus now giving no pain whatever, M. Malgaigne concluded that all that which remained depended upon the retraction of the ligamentous tissues. As is his practice in all cases of non-inflammatory articular rigidity, he forcibly extended the arm, notwithstanding the cries of the patient, until the biceps and the articular ligaments were thoroughly stretched, and rotation and abduction were freely practised. No sooner had the pain which this procedure caused disappeared, than the patient could move his arm without any suffering whatever—all his other pains being, in fact, dissipated as if by enchantment—some paralysis of the deltoid, from so long inaction, alone remaining. In cases more promptly treated, even a temporary paralysis of this kind does not take place; but if, after the inflammatory symptoms are removed by antiphlogistics and absolute rest, the effects dependent upon retraction are not forcibly overcome, atrophy and anchylosis are the results. In all the other joints there are also special points at which pain may be produced by pressure as long as inflammatory action is present, as behind the great trochanter in the *hip-joint*, at the head of the radius for the *elbow*. It is true there is pain also beyond these points, but this is comparatively of little importance. As long as this tender spot is present absolute rest must be continued, but as soon as it has disappeared, we may be certain the morbid element consists in mere retraction, and should waste no time with baths, pomades, and the various revulsives, because pain yet continues diffused around, or is produced on motion. The patient's courage must be appealed to, and without further delay we must, with the requisite firmness, thoroughly elongate the retracted tissues. In this way alone can anchylosis and its consequences be prevented, especially when the hip-joint is the one in question. During ten years M. Malgaigne has never found this valuable criterion in arthralgia and white-swelling fail him.—*Revue Médico-Chirurgicale*, t. v, pp. 177-80.

Since the publication of the above paper, another illustrative case has been published. It occurred in the person of a woman, æt. 26, of good general health. About two years since, she was seized without apparent cause with a pain in the shoulder, which, after a while, rendered all movement of the arm impossible. She was long treated in vain by various local applications, and then for some months in an hospital as for rheumatism. From the physician's wards she was transferred to one of those of M. Malgaigne, and on her admission she could not move her elbow from her side, or in the least degree extend her arm, the slightest movement of her shoulder also producing great pain. This case M. Malgaigne recognised as an example of scapulalgia advanced to a certain degree of anchylosis, perhaps even to complete anchylosis; and he confines his attention in treating it to purely mechanical measures, the case being, in fact, still in progress of treatment. Every day he induces some degree of abduction of the arm, and obliges the patient to hold it away from the trunk, by placing the point of a pin beneath it, which pricks the arm whenever it is lowered. By this simple contrivance a considerable improvement has been already secured.—*Gazette des Hôpitaux*, No. 64.

On Traumatic Cataract. By M. TAVIGNOT.

To what extent does a cataract produced by a traumatic cause comport itself differently from one occurring spontaneously? In the first place, the spontaneous one is oftenest lenticular, the traumatic one capsular or capsulo-lenticular. Traumatic cataracts are almost always more or less soft, while a considerable proportion of the spontaneous ones are hard or hardish. Moreover, in spontaneous cataract there is not usually any accompanying inflammation of the eye or its appendages, which is necessarily present in the traumatic form. Such inflammation may give rise to adhesions between the iris and the anterior crystalloid, or to a painful condition of the ciliary nerves, which is always more or less reflected on to the retina, producing in the course of time either an enfeebled or amaurotic state of it. Sufficient attention has not been paid to the effect of the inflammation on the cataract itself. As long as inflammation continues, the opacity either increases or remains stationary, inflammation and absorption being two incompatible phenomena. Traumatic cataract is usually found but in one eye, while in spontaneous, both eyes are generally successively or simultaneously affected. Although it is contrary to M. Tavignot's experience, that a traumatic cataract in the one eye can give rise to a spontaneous one in the other, yet he is far from denying the reciprocal influence of the two eyes, though not demonstrated to that extent. Of its existence, however, we have a proof in the reflection of the inflammatory action occurring in traumatic cataract to the opposite eye, giving rise to sympathetic iritis; and, as M. Petit observes, even cataract may be produced in the sound eye, not, however, because such exists in the injured one, but from the amount of sympathetic inflammation set up in the former. A last point of difference is, that while the spontaneous cataract, in process of formation, is always, though in a variable time, tending to become complete, the traumatic one, produced and maintained by inflammation, has no tendency when that inflammation subsides to make further progress. This is especially the case with regard to the anterior capsule, which may remain for the rest of life partially opaque, partially transparent.

The traumatic lesion may operate either by producing mere contusion or by opening the capsule to a greater or less extent. The most constant result of a *contusion* of the eyeball is an effusion of blood into the chambers of the eye, which at first prevents our ascertaining the exact amount of injury. But in giving a prognosis concerning the visible lesions, we must never forget that the globe has been subjected to a true concussion, the result of which may be a paralysed state of the retina, with or without a dilated condition of the pupil. The various other structures of the eye may also be more or less dangerously injured, and the lens becomes opaque, as a consequence of its relations with the neighbouring parts now acutely inflamed. The primary traumatic cataract may in some cases be a very simple one, all the inflammatory disorders being limited to the crystalline apparatus, and inducing very little general reaction; but the consecutive traumatic cataract is always accompanied by numerous disordered conditions of the interior of the eye, it being itself one of the complications of these different morbid elements.

Traumatic cataract from solution of continuity in the capsule may be best practically considered under three different heads:

1st. There may be a mere prick or slight wound of the anterior capsule. It is easily repaired; but a more or less well-defined opaline opacity remains, being sometimes quite circumscribed, and at others gradually diffused over the capsule, rendering it unfit for vision. In the case of this diffused opacity we seldom fail to observe, during the first three days, that the lens becomes in some degree displaced forwards, so as even to push the iris forwards into the anterior chamber. The pressure renders the iris convex, more or less dilated, and almost immoveable. In fact, the crystalline has not moved; but under the influence of the inflammation of the crystalloid, an intra-capsular dropsy, projecting the anterior capsule forwards, has occurred. At this period, so great is the opacity, that vision is abolished; but yet has the lens thus far preserved its transparency, and it only loses it by prolonged contact with the newly-formed fluid.

2d. The capsule has become opened to an appreciable extent, and irregularly, one of the lips of the wound projecting forwards, being free, or adherent to the pupillary margin, which is itself either intact or divided. When a wound of the capsule remains thus gaping, without tendency to cicatrize, the torn membrane becomes rapidly opaque, its lips curl upon themselves, and either remain free and floating, or contract adhesions with the surrounding parts, the result in all cases being rather to enlarge than diminish the size of the aperture. Does the crystalline, thus placed in these entirely new conditions, and in contact with the aqueous humour, escape destruction? In some cases, in the author's opinion, it preserves its normal vitality after the destruction or loss of its capsule. Various surgeons have observed it retaining its transparency after being deprived of its capsule, whether continuing in its normal position or thrown into the anterior chamber. As long as it retains its transparency it will not disappear by absorption; but once become opaque, it softens, and is absorbed in a variable period. In the majority of cases, an exposed lens sooner or later becomes opaque, takes on a milky-white appearance, then softens, and disappears. Its resorption may be effected in two modes. It may remain *in situ*, inclosed in the wounded capsule, and then the absorption takes place very slowly; or the opaque body becomes divided into fragments, which successively separate, and fall through the aperture of the capsule into the anterior chamber, and are rapidly absorbed. The capsule, however, remains, and may be an obstacle to vision, though not an absolute one, owing to the aperture in it made by the wound. Moreover, it is not uncommon to find this accidental opening gradually enlarge, so as to acquire the diameter of the pupil itself—a complete cure being thus effected.

3d. The capsule has been largely opened, and the lens has passed more or less forwards, towards or into the anterior chamber. This amount of injury is in general effected by contusing bodies, and the lens is not here, as in the last case, displaced because its capsule is torn; but by reason of the violence of the injury, it tears the capsule itself. Sometimes the aperture is insufficient, and the lens, as it were, remains strangulated. The lens may be luxated either forwards or backwards, and in the former case it is occasionally retained by a too narrow pupil (which, however, it usually passes easily), and then may give rise to a dangerous iritis. Arrived in the anterior chamber, it sometimes gives rise to very acute inflammation, and at others to a mere slight reaction. In the first case it rapidly becomes opaque, and in the other retains its transparency.

In the *progress* of a traumatic cataract we should distinguish between the period of development and that of resolution. The first is of short duration, opacity soon occurring; but the latter is generally delayed by the persisting inflammatory action, which must first be dispersed; and in several cases, no spontaneous cure occurring, the cataract continues indefinitely.

The *prognosis* in this cataract is unfavorable, as compared with that of the spontaneous, and this in proportion to the persistence of the inflammation. Mackenzie is the only author who has indicated the pathological circumstances which render the spontaneous cure of these cataracts possible. This depends upon whether the capsule is lacerated or not. If it continues perviously open, we may leave the case to nature, favouring absorption by appropriate means, and removing complications. If the capsule is intact, or the wound has united, we must, by the removal of present inflammation, prepare the patient for future operation.—*Gazette des Hôpitaux*, Nos. 14, 26.

On a Case of Blenorrhagia occurring in a Perforated Urethra. By M. Ricord.

A man, æt. 24, entered the hospital, who, when seven years old, had fastened a packthread around his urethra, and given rise to the production of mortification, the consequence of which has been to leave a circular constriction where the anterior two thirds of the organ joins the posterior third. Both anterior and posterior to this constriction, is an aperture, into which an instrument can be passed, all urine passing out at the posterior one. Erections take place as in the normal condition, and a month prior to admission, the patient had contracted a blenorrhagia, which as yet had not been subjected to any treatment. The discharge, which was abundant, occupied both portions of the canal. On the 3d of February, copaiba was prescribed internally, care being taken that none of the urine should come in contact with the anterior portion of the canal; and by the 8th, all discharge from the posterior portion, along which the urine flowed, had disappeared. The patient was now directed to allow the urine to traverse the anterior portion, which had as yet undergone no change in the amount of discharge, and in a very few days the disease became cured in this as well as the other portion.

This case, and others similar to it, which M. Ricord has recorded, prove, he observes, that the specific action of cubebs and copaiba do not depend upon any revulsive action or modification of the condition of the blood, but upon their direct and local action upon the diseased parts with which they are brought into contact by means of the urine containing them. The urine impresses some modification upon these substances, which is not imitable by art; for in whatever way we introduce cubebs or copaiba as injections, they not only do not act beneficially, but may do injury.—*L'Union Médicale*, No. 46.

On the Employment of Collodion in Ophthalmic Affections. By M. HAIRION.

To be enabled to protect the inflamed cornea from the contact with the air, prevent the movements of the eyelids over its surface, and retain topical applications long in contact with it, are objects which, if realized, would much diminish the severity of keratitis and conjunctivitis. Attempts at accomplishing these ends by the use of court plaster and the like had failed in the author's hands, when collodion offered itself to his notice. He usually applies it to the eyelids of one eye, and afterwards, if both eyes are diseased, to the other; but circumstances may render its simultaneous use necessary. The adhesion never lasts longer than forty-eight hours, frequently not so long, and has to be reaccomplished. The discharges from the eye usually work out a small passage, or a little space may be left at the angle of the eye, without interfering with its immoveability. This often forms an admirable means either of securing rest and darkness for the inflamed conjunctiva or cornea, or of enabling us to make effectual application of various ointments to the ocular surface. Then again, in the various perverted conditions of the eyelids, as in trichiasis, distichiasis, entropion, ectropion, &c., the ease with which, by collodion, the desired rectification can be secured, renders it a most valuable palliative and even curative agent.—*L'Union Médicale,* Nos. 29, 31.

Neuralgia of the Glans Penis. By Dr. SPENGLER.

DR. SPENGLER relates an interesting case of this rare affection. It occurred in the person of a healthy man, 40 years of age, who, having been cured of gonorrhœa four weeks, had connexion with his wife, ten weeks after the first infection. This was attended with the most intense suffering in the glans penis, which continued for two or three days, and always returned whenever he repeated the act. At last, abstinence from connexion procured no immunity; for, if he had a pollution, or even an erection, though this was not painful while it continued, the like intense pain came on afterwards. In the intervals he was quite free from pain or tenderness; no marks of inflammation were present; and an instrument found the canal quite free. The case being considered a pure neuralgia, every variety of treatment was resorted to in vain for weeks and months, until the man became hypochondriacal, and contemplated amputation or suicide. At last, Dr. Spengler cauterized the canal, with the effect of producing a bloody, puriform discharge, and some relief; and, by repeating this four times, at long intervals, he became quite cured. The author quotes a somewhat similar case from the 3d vol. of the 'Mem. of the London Med. Soc.' (1797), in which a man, after an impure connexion, contracted an excessive pain in the glans, to which no means that were tried afforded any relief, and which, in fact, was only cured several months later by the acquisition of a new gonorrhœa.—*Casper's Wochenschrift,* No. 46, 1848.

On Counter-openings. By M. DIDAY.

THE author observes that surgeons have either given no directions, or only such as are erroneous, for the mode of making counter-openings. The intention of these being to prevent the burrowing of pus, certain conditions are to be observed.

1. The internal orifice of the canal about to be created is to be situated at the very extremity to which the detachment of the tissues has extended. 2. The new canal must traverse the tissues in the most favorable direction for the free issue of pus. Inguinal suppuration has a great tendency to produce separation, the burrowing inwards at the genito-crural bend being greatly favoured by position and the lamellar character of the cellular tissue. If a counter-opening is made *perpendicularly* to the surface of the skin, large as this may be, the detachment of parts still goes on, as the pus, to enter it, would have to penetrate against the action of gravity. The artificial canal must therefore be itself *vertical;* to accomplish which end the tissues must not unfrequently be passed through obliquely rather than

directly, the *route* being, in fact, a longer but more accessible one. 3. The opening must be large enough, though this is a matter of less consequence if its situation and direction have been well chosen. 4. The new track should be of as equal a diameter as possible throughout. 5. It must be so made as to allow of the introduction, not only within the lips of the incision, but into the whole extent of the canal, of a tent of lint or charpie, without which its permeability cannot be maintained.

Supposing the above indications to be correctly laid down, they serve to show the erroneousness of the present practice, viz. the projecting the point at which an incision is to be made on a grooved conductor, and cutting down upon it from without inwards. This is sometimes difficult, and causes unnecessarily large incisions, and undue manipulation of the part. It is, in the author's opinion, a capital error to incise from without inwards; and after having ascertained, by means of a probe, the direction of the sinus and that of the desired opening, he passes in a small trocar with its canula, not exceeding a probe in size, and, having reached the extremity of the sinus, thrusts the trocar forward with some degree of force, so as to make its point appear through the skin at the desired place, after which he removes it, leaving the canula *in situ*. The incision, however, usually requires further dilatation, and to this end he places the point of a straight bistoury in the end of the canula, which projects externally, and by pushing in the one while he withdraws the other, the entire track of the wound is equably enlarged. The canula is again introduced for the purpose of affixing the tent to it.—*Gazette Médicale*, 1849, No 1.

On a Substitute for Tents in dressing Abscesses. By M. NONAT.

IN place of introducing tents, which are not only painful, but give rise to other inconveniences, M. Nonat passes a crayon of nitrate of silver into the opening, to the depth of from ½ to 1 centimetre, withdrawing it immediately. The pain caused by this is trifling and evanescent, and the eschar produced prevents the union of the walls of the trunk of the abscess. It is in abscess of the breast that this procedure has been found especially useful; the deepest abscesses being thus brought to heal in seven or eight days. The cauterization has to be repeated every two or three days.—*Gazette des Hôpitaux*, No. 23.

On Amputation of the Penis by means of the Actual Cautery. By M. BONNET.

THE actual cautery has been occasionally resorted to for the purpose of arresting dangerous hemorrhage after this operation; but during the last five years M. Bonnet, of Lyons, has employed it for the execution of the amputation itself, by which means he not only prevents hemorrhage, but what is of more consequence, phlebitis, and purulent absorption. He describes his success as very gratifying. After heating the iron, the patient is subjected to the influence of chloroform, and wet compresses are disposed around the penis. An assistant holds the organ by means of a forceps, and the surgeon, standing on the left of the patient, gently applies the iron perpendicularly to the axis of a healthy portion of the back of the penis, the division of the fibrous structure often requiring more than one such application. This accomplished, the iron is gently passed into the corp. cavernos., so as to coagulate the blood of its vessels, and the section is completed without loss of blood; four or five applications of the iron being wanted in all. When the cancer extends to the scrotum or abdomen, the operation is more tedious, as the ramifications must be followed by the cautery, though the pubis itself has to be reached. If the inguinal glands are affected, they should be removed by a bistoury, and the cautery applied. There is generally far less reaction than after ordinary amputation of the penis. In about a week the eschars begin to be detached in fragments, and they have all fallen in fifteen or twenty days. By this operation, then, we can much more readily act on healthy tissue, we are enabled to attack cases the knife could not reach, and we prevent hemorrhage.—*Gazette des Hôpitaux*, No. 18.

Swallowing an Iron Fork without fatal consequences.

M. **Velpeau** detailed to the Académie de Médecine, and authenticated the particulars, of an extraordinary case which occurred in the practice of M. Chemin. This surgeon was called, May 15, 1847, to a countryman, æt. 32, who, finding some difficulty in swallowing a bone, endeavoured to assist its passage by means of an iron fork, 25 centimetres in length by 3 or 4 in breadth; but this bringing on vomiting, he let go the fork, and it at once slipped into the stomach. At first he suffered little pain; but after a while this became intolerable, especially on taking a little food or drink; occasionally, too, he vomited abundance of watery fluid. The fork lay at first along the great curvature of the stomach, with its prongs turned towards the left side, and after a fortnight approached towards the pylorus, where it remained near four months, during all which time vomiting of black matters occurred several times a day, the mouth being continually filled with a watery fluid. His suffering was intense; the pulse was normal, tongue moist, appetite gone, attempts at digestion insupportable. At last the foreign body passed the pylorus, and in six weeks had traversed the small intestine, to become lodged for thirteen months in the right flank, opposite the ileo-cæcal valve. During the passage there was great occasional pain, especially on any movement, however slight. After remaining at the right iliac region for five months, the fork began to be dissolved. The patient now suffered from violent colic, constipation, distension of the belly, and difficulty in passing urine, black and brick-coloured matters being occasionally voided by stool. Next followed another eight months of alternating constipation and diarrhœa, some diminution of the pain, an enormous and incessant appetite being also observed; and gradually the patient resumed his laborious occupations. On the 10th December, 1848, the patient was seized with a most violent colic and constipation, both of which were at last relieved by repeated doses of castor-oil, a large quantity of fæces being passed; after which he felt quite well, and believed himself cured. However, on the 8th February, 1849 (twenty months after swallowing the fork), violent pain and expulsive efforts produced bloody stools, in the midst of which he found a considerable portion of the fork. Since then he has continued quite well.—*Gazette des Hôpitaux*, 1849, No. 66.

MIDWIFERY, &c.

On Heart-Clot. By Dr. Meigs.

Dr. Meigs in the present paper advances the opinion that many of the sudden deaths observed after delivery, and where the uterine hemorrhage has been insufficient to account for them, are due to the formation of a clot or false polypus within the cavities of the heart. He sets out with the proposition that the coagulability of the remaining blood is augmented after great hemorrhages, and such increased coagulability operates as follows, even after all hemorrhage is suppressed: "Loss of blood produces a tendency to fainting or lipothymia; during an attack of fainting, the motions of the heart are enfeebled, the diastole slow—torpid, for the blood moves languidly in both the venæ cavæ, pours itself out in a slow current into the auricle, which it sluggishly distends, and sometimes is then instantly converted into a solid clot. If a clot be formed in the right auricle, it will also be formed in the *iter ad ventriculum dextrum*, filling up the cone of the tricuspid valve; and the nucleus of it will cause the coagulum at length to occupy the cavity of the right ventricle, and extend itself to a greater or less distance along the tractus of the pulmonary artery. If the whole pulmonic side of the heart should be perfectly occupied in this way, the death of the individual would be instantaneous; and I doubt not that many of the examples of sudden death after delivery in hemorrhagic labours are produced by this formation of cardio-morphous coagula." It is familiarly known that the tendency to fainting during bleeding and after hemorrhage is greatly augmented by the assumption of the erect posture, and the author cites several cases from his midwifery practice, in which the women lost their lives in consequence of their having been allowed to raise themselves or be raised too soon after delivery. This he attributes to the formation of the coagula, these preventing the due circulation and aeration of the remaining blood, and thus giving rise to defective innervation. The time required for extinguishing life varies with the size and extent of the coagulation. In a case he relates, the patient lived forty-eight hours after the commencement of the symptoms, having suffered the extremest respiratory distress, and her pericardium and peritoneum becoming filled with serum. The auricle, tricuspid, and ventricle were filled not with a red "enthanasial" (the author has an astonishing predilection for hard words) clot, but with a firm yellowish-white "pre-enthanasial" one. A large coagulum speedily causing death will be found red, while, when the process is more protracted, it is of a dull white. He suggests that the death of the Princess Charlotte, in whose body no explanatory cause could be found, must have been due to the complete filling of the heart with such a coagulum. In some of the less marked cases prompt interference may rescue them from impending peril. "On a great many occasions I have been called to see patients, who, after hemorrhagic labours, have disobeyed my injunctions as to horizontal rest, and who, being prematurely lifted upright in bed, had fainted.

"I have not a doubt that among those of these persons in whom I found the heart fluttering, irregular, and feeble in its action on my arrival, incipient coagulation existed. I have thought, as I entered the room of the patient, that her auricular blood had begun to thicken, but was driven out from the auricle before its thorough coagulation, in consequence of the startling effects of a dash of cold water on the face, or of clapping the hands, or snatching the pillow from under the head or shoulders, allowing the head to fall so as to favour the restoration of the vascular tension, or even hyperæmia (of the brain), and thereby re-establishing the perfect and powerful excitation of the innervative force. The re-excitation of the innervative force of the brain would probably soon enable a heart so situated to discharge itself of the inchoate coagulum." A clot once formed is not removable; though when small, and giving rise to much disturbance of the heart's action, the struggle may be a protracted one.

Although the enfeebled sounds of the heart may aid the detection of the disease, it is upon "transcendental" diagnosis we must rely. "By transcendental diagnosis I mean one made by a process of the mind, fitter to be called sentiment or conviction, than a regular ratiocinative process. To enter an apartment one has quitted only half an hour before, and to find a patient hopelessly ill, with signs of imminent death, yet who had no serious symptoms of illness before—to find her making desperate voluntary efforts to breathe, without any signs of laryngeal, phrenic, or pulmonic inflammation or accident—to see her face pale and ghastly—to observe her conscious of impending asphyxia from loss of oxygen—without the leaden or iodic hue of cyanosis. These are the grounds of a diagnosis which may be called transcendental, one in which the consciousness of the physician informs him that a mechanical obstruction within the heart exists, and that such an obstruction can alone give rise to the phenomenon."

Dr. Meigs has never met with any purely cyanotic symptoms in these cases; a fact which he considers as confirmatory of his doctrine that cyanosis is essentially "not blueness of the surface, but a state of the nervous mass produced by the absence of oxygen in the brain-capillaries;" a rather unintelligible statement, probably due to his very peculiar diction.—*Philadelphia Med. Examiner*, No. 51, pp. 141-52.

[The cases of sudden death which cannot be immediately accounted for by the amount of uterine hemorrhage, stand in need of yet further explanation; for certain it is that in some of these the coagula in question have not existed, while persons in whose hearts they have been found have not died in any such manner. Dr. Meigs only cites two cases in which he has had the opportunity of verifying their existence in death after hemorrhage.]

On Neuralgia of the Cervix Uteri. By M. MALGAIGNE.

M. MALGAIGNE believes this to be by no means the rare disease it is generally thought to be. There are, indeed, two forms of neuralgia of the female genital organs—neuralgia of the cervix, and neuralgia of the appendices. Neuralgia of the cervix is often combined with leucorrhœa, or with an engorged state of the neck or body of the uterus. Its essential characteristic is the presence of a painful point, generally a solitary one, and almost always seated anteriorly and towards the left. It is very rare for this tender spot to exist without being accompanied by neuralgic pains in the abdomen, loins, epigastrium, &c. These painful irradiations vary, both as regards their seat and extent, and are very likely to be mistaken for other affections. In several cases mere repose gives great relief; but in obstinate cases the greatest variety of means are sometimes unavailingly tried. M. Malgaigne, calling to mind that in other neuralgiæ, the causes of which he could not discover, he had by their obstinacy been driven in despair to divide the affected nerve, resolved to proceed in an analogous manner in this case, and the incision of the painful point of the cervix was attended with great success. He has now performed the operation several times, and in but two instances has hemorrhage occurred, in both of which the curved scissors had made the incision horizontally. The vertical section has been attended with the loss of only a few drops of blood.—*Rev. Méd.-Chir.*, iv, 333-40.

Intra-vaginal Respiration recommended in some cases of Parturition where the Child's Life is in Danger from Pressure on the Cord. By M. M'CULLOCH, M.D.

THE writer desires to make known a new mode of practice, by which he thinks that the amount of infant mortality will be lessened in many cases in which death, from pressure on the cord, is otherwise inevitable. "If we take the average of French, German, and British practice, we find by the most authentic statistics that danger to the life of the child from this cause occurs in breech presentations once in 53 labours; in presentations of the superior extremities, after turning, once in 261; and where the cord presents, once in 245 cases. Thus we have in 649 labours 12 presentations of the breech, 7 of the inferior extremities, 2½ of the superior extremities, and 2½ of the cord, occasioning a loss of life at birth of about two per cent. of all children born. This great mortality I think I have succeeded in diminishing in my own practice in three footling cases within the last eight months, by enabling the child to breathe while the head was still within the cavity of the pelvis, after the pulsation in the cord had been several minutes extinct. And this novel state of existence was maintained by keeping the child's mouth open with my finger, and the perineum expanded to allow air to have free access into the vagina. In future, I shall endeavour to accomplish this object with greater certainty by introducing into the mouth the end of the largest-sized gum-elastic male catheter, with several perforations in it, and continue as before to allow as much air as possible to approach the face and nostrils. In all cases of malposition in which the practice I have suggested becomes desirable, the chance of respiration being established will be greater, if the means are used before the circulation becomes feeble in the umbilical vessels; and if the child is born alive in the absence of pulsation in them, it may fairly be conceded that we have been successful in our efforts to preserve life. Some cases may occur where a small-sized male catheter, with a suitable curve, might be used with advantage as a tracheal pipe. I need scarcely add that the infant's body should be kept warm, gentle traction employed, and all the other ordinary means of exciting contraction of the uterus used until the delivery is completed.

"If the practice I have here recommended is found to answer in other hands as it has in my own, something will have been achieved in obstetric science, in circumstances where the most skilful have hitherto frequently had to lament their inability to save the child's life."—*British American Journal*, May 1849.

On Turning by External Manipulation. By Dr. SPENGLER.

As this operation can only be performed before the liquor amnii has been discharged, Dr. Spengler observes that the opportunity of effecting it seldom falls to the lot of the private practitioner in Germany; but in midwifery establishments it is more easily resorted to, and Ritgen states that he seldom now employs other means. Dr. Spengler was called to the present case, occurring in a robust woman, on account of a hemorrhage after a fall, which, however, had ceased on his arrival. She was at full time. On examining the abdomen, he found it wanting in the usual roundness, and the integuments being relaxed, he was able on the right side to feel the lower parts of the child, and on the left a firm round body, which he concluded was the head. The os uteri was sufficiently open to admit the point of the finger, and its lips were thick and yielding; no presenting part could be felt. The sounds of the child's heart were inaudible, nor had the mother felt it move since the morning. The placenta-murmur was plainly heard at the left side. Believing he had diagnosed a cross-birth, the author proceeded to endeavour tó rectify the position. The woman was laid on her left side, and a small firm cushion placed under the child's head. During a pain, with one hand applied to the child's head externally, and the other towards its breech, he endeavoured to direct the one down towards the brim of the pelvis, while he raised the other upwards. Little success at first attended these efforts, and the woman was unable to lie any longer on her left side. After two or three hours' manipulation, the patient sometimes lying on her back and sometimes on her side, with the cushion pressing on the child's head, some movement was at last impressed upon the infant's extremities; and after a while the bag of waters could be felt pressing against the os uteri, having a hard body behind it. The pains were slack, and on account of the thickness of the membranes these were not easily ruptured. After the discharge of the waters, the pains continued too slight, and the author having no more ergot, resorted to a practice which we have more than once found useful under similar circumstances. He says that it occurred to him, that as plugging the vagina may be employed successfully in the artificial induction of premature labour, the same mode of inducing uterine action might here prove available; wherefore, he filled it with sponge and charpie, and in a very short time very active pains came on, expelling a dead child. The woman resumed her household affairs in a week.—*Neue Zeitschrift für Geburtskunde*, xxv, 187, 170.

Puerperal Tetanus. By Dr. AUBINAIS.

DR. AUBINAIS, of Nantes, relates three cases of this affection which have come under his observation, believing that it has not received sufficient attention from writers upon obstetrics. The exciting cause of the disease is almost always the suppression of the lochia, or of profuse sweating during the milk-fever, by wet or cold, and hence probably the reason why it is so much more common in rural than in civic districts. In the *first* case, the milk-fever was high on the fourth day, when the woman (æt. 30) crossed a wet floor, while the skin was covered with sweat, with naked feet. She was soon seized with prolonged shivering, abdominal pain, and suppression of the lochia, and gradually the tetanic condition, commencing with difficult deglutition, markedly prevailed, inducing a slight degree of emprosthotonos. She died seven days after the symptoms began. In the *second* case, a poor woman, æt. 28, having suffered from an attack of eclampsia, for which she was bled, was delivered a few days after of a seven months' child. She went on very well until the fifth day, when, having drank a large quantity of cold water, she was seized with shivering, followed by trismus and difficult deglutition, which went on to complete tetanus. On account of her enfeebled state, blisters were alone resorted to, the lochia being also restored by warm baths. The spasmodic symptoms did not give way for twenty days, and forty elapsed before she was quite restored. In the *third* case, the woman (æt. 34) likewise, while in a state of perspiration, exposed herself to wet and cold four days after confinement, with the effect of suppressing the lochia. The tetanic symptoms did not appear until twenty-four hours after this, but then became strongly developed, and obstinately resisted the most active antiphlogistic treatment until the tenth day, when they began to give way. She was bled six times, 100 leeches were applied, and musk and valerian were afterwards given.—*Rev. Méd.-Chir.*, t. v, pp. 149-59.

MATERIA MEDICA AND THERAPEUTICS.

On Large Doses of Sub-nitrate of Bismuth in Gastro-Intestinal Affections.

By M. MONNERET.

M. MONNERET states that he has been for some years engaged in experimenting upon the medicinal qualities of this substance, and that he finds that, given in far larger doses than are supposed to be permissible, it is of the greatest value in gastro-intestinal affections, especially those attended with fluxes.

Simple Diarrhœa. It is especially in the diarrhœa of infants, resulting from imperfect lactation, that he has found it so very useful, large doses curing such in a few days. So, too, after weaning, and the injudicious diet so often adopted, the bismuth, in doses of three teaspoonfuls a day, soon removes all symptoms of disordered digestion, especially the serous diarrhœa so often produced in these cases. Simple diarrhœa is of much rarer occurrence in the adult; still, examples are met with in which we cannot attribute it to either phlegmasia or ulceration of the intestines, as in the granular disease of the kidneys, in chlorotic women, in women become anæmic from cancerous degeneration of the uterus, and in some cases of chronic disease of the heart, or in commencing phthisis. Those cases in which the diarrhœa is serous, and results from an atonic state of the gastro-intestinal membranes, best yield to this medicine. It should be given in gradually increasing quantities from two drachms to twelve or more per diem.

Diarrhœa symptomatic of Intestinal Lesion. In spite of the presence of symptoms indicative of inflammatory action, the author has frequently administered the bismuth to children with great success. So, too, he has found it very useful in some subjects of phthisis suffering from obstinate colliquative diarrhœa, and even in the diarrhœa of typhoid fever, when the intestinal canal was certainly the seat of very numerous ulcerations. In this last case its utility is not very permanent; but it has the advantage of inducing the tolerance of articles of diet or medicine, which without it would be rejected.

Cholerine. The frequency with which gastro-intestinal disturbance precedes true cholera is well known, and the desirability of its suppression is evident; and to this end large doses of bismuth succeed better than any other means. It is most suitable in the cases in which with the diarrhœa there are nausea, vomiting, gastralgia, colicky pains, borborygmi, and anorexia. The cases are innumerable, in which the author has been enabled rapidly to relieve these symptoms without any aid from opium, by giving eight or ten drachms per diem. The only inconvenience resulting, is a certain amount of constipation.

Gastralgia. Various observers have already admitted the great utility of this substance, both in idiopathic gastralgia and in the forms dependent upon hysteria, hypochondriasis, chlorosis, &c. &c.; but it has frequently failed in the hands of others, by being given in doses of two or three scruples per diem, in place of twelve or fifteen drachms; and it is taken with such ease, that the patients voluntarily continue its employment after the pain has ceased, until the gastro-intestinal functions are thoroughly re-established. The idiopathic forms of gastralgia, so often met with in sedentary or literary persons, and in those suffering from depressing emotions, or from abuse of alcohol or coffee, are those which are most successfully combated. It has often, too, enabled the stomach to bear those tonic aliments, whose slow and painful digestion, accompanied by cephalalgia, drowsiness, and pain in the back and limbs, is so often observed in chloro-anæmic patients. This medicine only produces a temporary relief in gastralgia, dependent upon organic disease of the stomach, except when this viscus secretes a large quantity of acid fluid.

Vomiting. Vomiting may depend upon a simple gastric neurosis, and then the bismuth can be very usefully employed. It is also useful in the vomiting of pregnancy, and that accompanying dysmenorrhœa; but its efficacy is less certain than in affections of the gastro-intestinal tube, and is never so great as when diarrhœa, colic, and flatus are present. From whatever cause pain manifests itself during digestion, we may relieve it by mixing the sub-nitrate freely with the articles of food.

Administration and action. It should be given in powder, and best so with the first spoonful of broth or gruel. It excites no disgust, especially if placed between two bits of bread soaked in broth. Children take it readily with milk or ptisans. The author has never given less than from two to three drachms per diem, nor more than twenty, and he has never observed the slightest inconvenience from these large doses; and it is his custom to give it to the children in his hospital by spoonfuls or tablespoonfuls, without observing more exactitude, so innocuous is it. So imperfectly is this fact known, that the chemists hesitate in preparing his prescriptions in which these large doses are ordered. The author cannot conceive why this substance was ever set down as an irritant poison, as he has never found the slightest irritation from the largest doses given to either the healthy or the sick; and post-mortem examination proves that, beyond patches of black discoloration, it produces no effect on the mucous membrane, the consistence of this remaining quite normal. The action of the substance on the canal seems to be quite negative, that is to say, the anormal symptoms for which it was prescribed quickly disappear. Great attention has failed also to detect any marked effect upon any other part of the system. Perhaps the urine is somewhat increased, and the pulse becomes slower, but this is probably due to the relief of the gastro-intestinal affection. The action of bismuth is then purely a *local* one; and this local action, so far from being an irritant one, as so commonly stated, diminishes the activity of the phenomena of which the mucous membrane is the seat. It is not to be supposed, however, that this inert substance can act actively and directly upon the canal, as a soluble body after absorption does do; and it is probable that it acts merely as an inert body, just as charcoal might do, mechanically protecting the over-excited secretory organs and papillæ of the nerves from the too immediate contact of the fluids, and especially of aliments. By its mechanical apposition, it limits the amount of exhalation. The allegation of its antispasmodic properties has solely arisen from observing the diminution of the symptoms, without considering whether this might not arise negatively from the preservation of the gastro-intestinal surface from the causes of irritation. In this way we might call darkness an antispasmodic, because it allows the repose of the retina, and the subdual of the irritation which gives rise to photophobia.—*Gazette Médicale,* Nos. 15, 16.

Of the utility of bismuth in smaller doses in gastric affections we have almost daily proof; but the doses we are in the habit of employing are so very much smaller than those recommended by M. Monneret, that their beneficial action can scarcely be explained upon the mechanical theory he advocates, which is, however, admirably suited to the explanation of the *modus operandi* of the very large doses he employs. As somewhat confirmatory of his view, we may mention that M. Belloc, in a recent communication to the Académie, makes mention of the very great benefit derivable from the employment of *charcoal* prepared from poplar shoots, in gastro-intestinal affections. It rapidly subdues pain, facilitates digestion, excites the appetite, and enables medicines that the alimentary canal would not otherwise tolerate to be easily borne.—See *Rev. Méd.-Chir.,* t. v, pp. 75-86.

On Aconite. By M. Teissier.

M. Teissier has undertaken a prolonged series of researches in order to establish, if possible, the therapeutical value of this substance, and especially its *anodyne* and its *antiphlogistic* power. The existence of the former is incontestable, but it differs from opium in giving relief, not as that does to all descriptions of pain, but only to certain kinds of it. This speciality of the action of aconite is one of its characteristics, its anodyne property being only in fact a secondary one. Its principal, and in some measure specific, action is exerted upon the *skin*, eliminating through its vessels injurious principles, and restoring the natural functions when these have been disturbed. Thus aconite is a very appropriate remedy in diseases induced by chills, or when morbid principles are detained in the cutaneous tissues, as in the exanthemata; and M. Teissier has especially derived benefit from its anodyne properties in diseases of a rheumatic or catarrhal origin. The antiphlogistic power of the substance is quite subordinated to the influence exerted by it upon the skin, and is especially observed in individuals of a nervous or lymphatic temperament, and those predisposed to rheumatic and cutaneous affections; and in such it is beneficial in lumbago, angina and bronchitis, influenza, rheumatism, especially when recent and not very acute, &c. M. Teissier prefers the *tincture*, to obtain the anodyne effect of which he rapidly increases the dose from 10 or 20 drops to 1 or 2 drachms per diem.—*Revue Médicale*, 1849, t. i, p. 333.

Concealing the Taste of Fish-oil.

Now that the swallowing cod-liver oil bids fair to become the fashionable mania of the day, it may be as well to state the simple and effectual means communicated by M. Fredericq, of disguising its abominable taste. This merely consists in masticating a morsel of dried orange-peel just before and just after swallowing the dose. —*Rev. Méd.-Chir.*, t. v, p. 114.

Collodion in Bed-sores.

Air-cushions and water-beds are not obtainable by the poor, and it has been the practice to protect parts subjected to injurious pressure either by powdering them with some inert substance, or covering them with diachylon. The first of these measures is of little use, and the other does more harm than good, as by softening the tissues to which it is applied, it only renders them more susceptible to the influence of pressure, especially if the leather on which it is spread gets into folds. Collodion adheres far more closely than diachylon, and is quite exempt from moisture; and in the cases in which it has been tried in the French hospitals, has answered well.—*Gazette des Hôpitaux*, 1849, No. 38.

Removal of Stains of Nitrate of Silver.

Accident first led M. Martinenq to the observation, which he has since repeatedly confirmed, that the stains produced by nitrate of silver on linen, &c., may be readily removed by wetting the linen in a solution of *bichloride of mercury* (1 part to 31), rubbing it well, and then washing it in cold water.—*L'Union Médicale*, 1849, No. 15.

BOOKS RECEIVED FOR REVIEW.

The Retrospective Address on Diseases of the Chest, delivered at the Meeting of the Prov. Med. and Surg. Association, Derby, 1847. By Edward J. Shearman, M.D. Worcester, 1848. 8vo, pp. 47. With Four Plates.

Report on the Epidemic Cholera as it has appeared in the Territories subject to the Presidency of Fort St. George. Abridged from the Original Report, printed at Madras in 1824. By William Scot, Surgeon and Secretary to the Medical Board. London, 1849. 8vo, pp. 212.

On the Cryptogamous Origin of Malarious and Epidemic Fevers. By J. K. Mitchell, A.M., M.D. Philadelphia, 1849. 12mo, pp. 137.

An Account of the Origin, Spread, and Decline of the Epidemic Fevers of Sierra Leone; with Observations on Sir William Pym's Review of the "Report on the Climate and Diseases of the African Station." By Alexander Bryson, M.D., R.N. London, 1849. Post 8vo, pp. 174.

Report on Quarantine, from the General Board of Health. London, 1849. 8vo, pp. 172.

On the Treatment of Headache by Benumbing Cold; with an Account of other Remedial and Anæsthetic Uses of Congelation. By James Arnott, M.D. Brighton, 1849. 8vo, pp. 54.

A Treatise on the Diseases of the Heart and Great Vessels, &c. &c. By J. Hope, M.D., F.R.S. Fourth Edition. London, 1849. Fcap. 8vo, pp. 611.

Pathological and Practical Observations on Strictures, and some other Diseases of the Urinary Organs. By Francis Rynd, Esq., A.M., M.R.I.A. &c. London, 1849. 8vo, pp. 195.

Lectures on the Causes and Treatment of Ulcers of the Lower Extremity. Delivered at the London Hospital during the Summer of 1848. By George Critchett, F.R.C.S., &c. London, 1849. 8vo, pp. 121.

Portraits of Diseases of the Scalp, with the safest and most efficient Modes of Treatment. By Walter C. Dendy. Fascic. I.

Surgical Anatomy. By Joseph Maclise. Fascic. III.

The Pathology of the Human Eye. By John Dalrymple, F.R.C.S. Fascic. II.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. Fascic. V.

Practical Observations on the Prevention, Causes, and Treatment of Curvatures of the Spine. By Samuel Hare, Surgeon. Third Edition, revised and enlarged. London, 1849. Post 8vo, pp. 245. With Engravings.

A Practical Treatise on Morbus Coxarius, or Hip-joint Disease. With Cases and Illustrations. By William C. Hugman, M.R.C.S., Surgeon to the Verral Institution. London, 1849. 8vo, pp. 81.

A Treatise on the Pathology, Diagnosis, and Treatment of Neuroma. By Robert W. Smith, M.D. T.C.D., M.R.I.H. Dublin, 1849. Folio, pp. 30. With Fifteen Plates.

On the Extraction of Teeth; with an Account of a New and much less painful Mode of Operating. By Henry Gilbert, M.R.C.S. London, 1849. 8vo, pp. 66.

Die Erkenntniss und Heilung der Ohrenkrankheiten vom Dr. Wilhelm Kramer. Zweite gänzlich ungearbeitete und sehr vermehrte Auflage. Mit 2 Kupfertafeln. Berlin, 1849. 8vo, pp. 870.

Memoirs on the Ganglia and Nerves of the Uterus. By Robert Lee, M.D., F.R.S. London, 1849. 4to, pp. 36. With Five Engravings.

Practical Observations on Diseases of the Uterus. By Robert Lee, M.D., F.R.S. With coloured Plates. Fascic. I. and II. 4to.

Parturition and the Principles and Practice of Obstetrics. By W. Tyler Smith, M.D. London, 1849. Fcap. 8vo, pp. 395.

Obstetrics: the Science and the Art. By Charles D. Meigs, M.D. With 121 Illustrations. Philadelphia, 1849. 8vo, pp. 685.

An Essay on the Diseases of Young Women. By Walter Johnson, M.B., Medical Tutor, Guy's Hospital. London, 1849. 12mo, pp. 256.

Practical Remarks on the Use of the Speculum in the Treatment of the Diseases of Females. By Thomas R. Mitchell, M.D. Illustrated with cases and coloured plates. Dublin, 1849. 12mo, pp. 83.

A Practical Treatise on Inflammation of the Uterus and its Appendages, and on Ulceration and Induration of the Neck of the Uterus. By James Henry Bennet, M.D. London, 1849. 8vo, pp. 527.

Hints on Obstetric Practice, with Illustrations. By John Bremner, Surgeon, Newtyle. Part I. Edinburgh, 1849. 8vo, pp. 98.

The Misapplication of Anæsthesia in Childbirth, exemplified by Facts. By G. T. Gream. London, 1849. 8vo, pp. 72.

Effects of Chloroform and of strong Chloric Ether as Narcotic Agents. By John C. Warren, M.D. Boston (N. E.), 1849. 12mo, pp. 66.

Anæsthesia, or the Employment of Chloroform and Ether in Surgery, Midwifery, &c. By J. G. Simpson, M.D, F.R.S.E., &c. Philadelphia, 1849. 8vo, pp. 248.

Essay on the Use of Alcoholic Liquors in Health and Disease. By John Chadwick, M.D. London, 1849. 12mo, pp. 123.

Eighth and Ninth Annual Reports of the Registrar-General of Births, Deaths, and Marriages in England. London, 1849. 8vo, pp. 616.

Questions and Observations in Hygiene, recommended to the consideration of Naval Medical Men. By Frederick James Brown, M.D., London, Assistant-Surgeon R.N. London, 1849. Post. 8vo, pp. 64.

Principles of Scientific Botany; or, Botany as an Inductive Science. By Dr. J. M. Schleiden, Extraordinary Professor of Botany in the University of Jena. Translated by Edwin Lankester, M.D., F.R.S., F.L.S., &c. London, 1849. 8vo, pp. 616. With Plates and Wood-Engravings.

An Exposition of the Case of the Assistant-Surgeons of the Royal Navy. By a Naval Medical Officer. London, 1849. 8vo, pp. 28.

Beneficence of Design in the Problem of Evil, vindicated by the Law of Causation in the Physical Construction of Matter. By a Journeyman. Tenth Bridgwater Treatise. New York, 1849. 12mo, pp. 213.

*A Short Sketch of the Life and Writings of the late Joseph Clarke, Esq., M.D., containing Minute Results of his Private Practice. By Robert Collins, M.D. London, 1849. 8vo, pp. 88.

A Physician's Holiday, or a Month in Switzerland in the Summer of 1848. By John Forbes, M.D., F.R.S. With a Map and Illustrations. London, 1849. Post 8vo, pp. 520.

Letters on the Truths contained in Popular Superstitions. By Herbert Mayo, M.D. Edinburgh, 1849. Post 8vo, pp. 152.

Lectures on Medical Missions. Delivered at the instance of the Edinburgh Medical Missionary Society. Edinburgh, 1849. Fcap. 8vo, pp. 320.

THE

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OCTOBER, 1849.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *Traité Théorique et Pratique des Maladies des Yeux.* Par L. A. Desmarres, m.d., Professeur de Clinique Ophthalmologique, &c. Avec 78 Figures intercalées dans le Texte.—*Paris*, 1847. 8vo, pp. 904.

Theoretical and Practical Treatise on the Diseases of the Eyes. By Dr. A. L. Desmarres, Professor of Clinical Ophthalmology, &c.

2. *Die Operative Chirurgie von* Johann Friedrich Dieffenbach. Zweiter Band.—*Leipzig*, 1848.

The Operative Surgery of J. F. Dieffenbach. Second Vol. (Chapters XCIV to CXX inclusive.)

We have here placed together the works of two men, the most opposite in character and pursuit,—the one the type of the special, the other of the universal surgeon;—the one a young man writing to make a reputation, the other a veteran imparting the fruits of long experience;—the one, surgeon of a large public hospital, frequented by numbers of students continuously watching, recording, and verifying his practice and statements; the other, professing and teaching clinical eye-surgery in a small room on the third floor at the back of a dirty house in one of the smallest streets in Paris. In stating this fact, we do not wish to disparage Dr. Desmarres; but in judging of a book, something should be known of the author. We should know what opportunities for observation he has enjoyed, and be careful that, in matters of authority, a Parisian professor of clinical ophthalmology should take a lower position than a surgeon of a British eye-hospital, containing many beds, and attended by numerous students. In Paris, there is nothing which approaches to our eye infirmaries. M. Sichel, some years ago, commenced giving gratuitous advice to persons suffering from diseases of the eyes; and students, who paid him ten shillings a quarter, were allowed to witness his practice. After a time, he added three or four beds to his establishment, and supported it by the fees of the students; as such it still remains. M. Desmarres was one of M. Sichel's pupils, who quarrelled with him, imitated him, and is becoming a rival. It is astonishing, considering the small means of MM. Sichel and Desmarres, how much they have done. The results of their labours have been far greater than those of many surgeons among ourselves, who have been placed under far more favorable circumstances; but still, in considering practical books, we must not shut our eyes to the true position of the author, or forget that those who are watching his practice seldom do so for more than a few months.

We need not say much as to the value of the work of Dieffenbach; it is the most perfect book on operative surgery which has ever been published in any language. We purpose shortly to give a close analysis of the volume before us. At present we shall merely select from the chapters on the operative surgery of the eye such practical remarks as are likely to be useful to our readers, or appear to illustrate the statements of Desmarres.

Dr. Desmarres' work is a systematic treatise on the diseases of the eye, nearly on the same plan as the treatises of Lawrence, Mackenzie, and Jones. The work opens, like that of Jones, with an *Ophthalmoscopie*, or rules for examining the eye in a state of disease. The rest of the work is divided into three parts, comprehending diseases of the eyelids, diseases of the globe of the eye, and diseases of the lachrymal apparatus. The diseases of the conjunctiva, semilunar membrane, and caruncula lachrymalis, as well as those of the muscles of the eyeball, are considered in the second part. No notice is taken of the diseases of the orbit—a material omission.

General plan. The *first* part of Dr. Desmarres' work is divided into four chapters, which treat of congenital and acquired deformities, inflammations, tumours, and syphilitic affections of the eyelids. The *second* part contains fifteen chapters, embracing diseases of the conjunctiva, cornea, sclerotica, anterior chamber, iris, crystalline capsule, crystalline, vitreous body, ciliary body, and retina; general diseases of the globe of the eye, functional affections of vision, diseases of the muscles of the eye, and diseases of the semilunar membrane and caruncula lachrymalis. The *third* part consists of three chapters, embracing diseases of the lachrymal gland, puncta and lachrymal canals, lachrymal sac, and nasal duct.

Had this work been an English one, or one likely to be translated into English, we should have pointed out evident faults in this arrangement, and the difficulties and inconsistencies into which it has led the author; but we need not occupy our readers' time by doing so. We think it better to make such an analytical and critical survey of the book as shall enable us to convey to our readers the most important practical information it contains, comparing the views of the author with those of Dieffenbach, and making some additions of our own.

Symblepharon. Dr. Desmarres notices the usual distinction of *total* and *partial* symblepharon; also, that variety which may be called *insulated*, round which a probe may be passed, and that in which the morbid union, dipping into one or other of the conjunctival sinuses, is, in all its extent, *continuous* with the conjunctiva, except at its exterior edge, and round which, therefore, the probe cannot be passed. He takes no notice of the attempt of Ammon to bring two totally different sorts of diseases together, under the name of symblepharon, calling the one *anterior*, and the other *posterior;* the posterior being nothing else than the contracted state of the sinuses of the conjunctiva, which generally accompanies xeroma; while his anterior symblepharon is the disease commonly known by this name, and which consists in an union, more or less complete, of the eyelids to the eyeball, the consequence of ulceration.

It is an important fact, which does not appear to have attracted Dr. Desmarres' attention, that total symblepharon is sometimes complicated with staphyloma of the cornea and iris. When this is the case, it becomes necessary to extirpate the staphyloma, in order to relieve the patient from very severe pain caused by the distension of the eye, pent in by the adherent eyelids. No attempt should ever be made to separate the lids from the ball of the eye with the knife, unless it be for the purpose of removing a staphyloma. In a singular case, which we have had occasion repeatedly to see, of total symblepharon, a fistulous communication exists between the staphyloma and the cellular tissue of the upper eyelid. From time to time, the upper eyelid becomes greatly distended, and the whole organ painful. The surgeon is then called to puncture the swelling, which he does with a narrow bistoury, giving issue to a large quantity of aqueous humour. This relieves the patient till the fluid again accumulates; and, no doubt, temporary relief might be afforded, in a similar way, to the more common cases, in which there is presented to our notice a simple combination of symblepharon and staphyloma, without any fistula of the cornea.

Insulated symblepharon is always cured by simple division of the morbid band of connexion; but it is otherwise with continuous symblepharon, how narrow soever the adhesion may be. Whatever care is taken to prevent the reproduction of bands of adhesion, and a gradual reapproach of the disunited parts, these events are almost sure to follow, so that the patient is generally left just in the same condition as when he submitted himself to the knife. Tearing open the bottom of the wound day after day with a pin, so as to destroy the pyogenic membrane, as Amussat recommended for the permanent extension of cicatrices, will, we fear, prove in general inefficacious in cases of symblepharon, although it might succeed in keeping open an artificial coloboma palpebræ, were such a thing required, or an artificial slit of the prepuce, for the cure of phymosis.

On account of the difficulty of remedying symblepharon by operation, we have long felt inclined to think those oculists right, who tell the patient just to put up with the inconvenience he suffers. If the union is of small extent, the drag on the eye is slight, and scarce worth thinking about; if more extensive, the union is sure to return, just as the cure seems finished, and the cicatrization of the wound made in the operation comes to a completion. Let alone, the adhesions gradually become elongated, and the eye more mobile. Cut into, they become more solid, and the eye more confined. Ammon and Dieffenbach, however, men not to be beaten, invented each an operation, of a curious and artificial kind, for symblepharon; which we have not observed noticed in any English work.

In Ammon's operation, nothing is done in the first instance to the symblepharon; but by two incisions, meeting at an acute angle, the adherent lid is divided in its whole thickness, so as to form a triangular flap, having as its basis the edge of the lid, and including the symblepharon. Over this triangular flap, the two remaining portions of the lid are then stretched, and brought close together by the twisted suture. They unite to one another, but not to the subjacent flap, on the inside of which is the symblepharon. At the end of fifteen or twenty days, when the union is consolidated, the second part of the operation is performed, which consists in dissecting out the triangular flap, which has been left adherent by the symblepharon to the globe of the eye. After each division of the operation, compresses wrung out of iced water are to be applied over the eye to moderate the inflammation.

Dieffenbach's operation consists in folding in the eyelid, so that its cuticular surface is brought into contact with the globe of the eye, after the morbid union is divided. If it is the lower sinus of the conjunctiva, for instance, which is affected, he proceeds in the following manner. He makes an incision from the inner angle of the eye along by the side of the nose, and another, equally vertical, from the outer angle to the lower-outer edge of the orbit. He then detaches the lid from the globe of the eye by dividing the symblepharon, and shaves away the cilia. He next folds in the quadrilateral flap upon itself, so as to produce a complete entropium, and fixes it in this position by sutures. Four are generally required. They are supported by strips of adhesive plaster, and the parts are covered with a fomentation. When the globe of the eye is once cicatrized, the artificial entropium is to be removed, and the flap fixed again in its original situation by sutures.

We doubt much whether the deformity, which must result from such operations, will not greatly counterbalance the gain accruing from the cure of the symblepharon.

Andreæ remarks* on Dieffenbach's operation, that it would be simpler, instead of an artificial entropium, to have recourse to an ectropium, after the two vertical incisions were made; and to keep the lid everted, till the wound made in dividing the symblepharon was healed.

Entropium. The account given by Dr. Desmarres of entropium extends to twenty-one pages. He gives a figure, illustrative of a method of operating for entropium, by the extirpation of a vertical fold of skin, which, it seems, was long practised by a surgeon of Lyons, of the name of Janson; remarking that it is suited to the more serious cases. We have great doubts, however, whether *chronic* entropium, with morbid changes in the texture of the tarsus and conjunctiva, can be cured by any operation consisting merely in the removal of a piece of skin, or even in sections and partial extirpation of the orbicularis. Clipping out a transverse fold of integument is always sufficient for the cure of *acute* entropium. Dr. Desmarres observes, that a transverse excision of the skin will not always succeed in reducing an entropium, when it goes to a great degree, and especially when the palpebral opening is narrow. A vertical loss of substance, he says, has the double advantage over the transverse, that it acts more powerfully upon the inverted lid, and that, after the cure is accomplished, it does not increase the vertical diameter of the palpebral opening. It must not be forgotten, however, he adds, that the vertical cicatrix is more apparent than the transverse, the latter being concealed in the natural folds of the skin.

To perform the operation for entropium according to the method of Janson, the surgeon lays hold of a vertical fold of the skin of the inverted lid with his finger and thumb, or, what is preferable, with a pair of entropium forceps, and extirpates the fold with curved scissors, taking care to let the extirpation extend close to the cilia. The wound is to be brought together with stitches, or, what is better, with the twisted suture, introducing three or four pins, and leaving them till they fall out of themselves. In this way, three or four small transverse wounds are produced, which contribute along with the vertical section to draw the edge of the lid into its natural direction. In some bad cases, several vertical sections may be had recourse to, at some distance from one another. Lisfranc sometimes denuded almost the whole lid. Segon made a crucial wound, combining the method of Celsus with that of Janson, and approaching the plan followed by Acrel of a lozenge-shaped wound. Dieffenbach does not describe the operations for entropium in this volume, having treated the subject in his first volume, in the section of Plastic Surgery.

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* Grundriss der gesammten Augenheilkunde, vol. ii, p. 73. Leipzig, 1846.

Trichiasis. When only a few cilia are turned towards the globe of the eye, and evulsion has been repeatedly tried without permanent effect, Dr. Desmarres recommends a small fold of the skin, as near as possible to the edge of the lid, to be raised by means of a small double hook, and removed with a cataract-knife. Some drops of blood are discharged, and next day, he says, the upper edge of the wound (if the lower lid is the part operated on) inclining towards the lower, carries with it the misdirected cilia, and removes them from contact with the eyeball. The operation may be performed at several points of the same eyelid, if necessary. (p. 88.)

Dr. Desmarres does not give a favorable opinion of Carron du Villards' plan of cauterizing the bulbs of the cilia. This plan consists in pushing into each bulb, in the direction of the corresponding eyelash, an entomologist's pin, to the depth of at least a line and a half, and when all the pins are implanted, uniting them by a noose of well annealed silver wire. The group of pins is then to be laid hold of with a pair of curling-tongs, heated to a white heat. The pins immediately grow white, and the bulbs and their produce are destroyed. That the eyeball may not suffer in this process, several folds of brown paper, dipped in water, are to be applied over it, and held there with a wooden spoon.

This process, which Dr. Desmarres tells us he has tried with all possible care, is far from being so easy of execution as its author seems to hint. It causes much pain, and, if any considerable number of pins requires to be inserted, it throws the patient into a state of great excitement. The pins, besides, are not easily kept in their places, and it often happens that one or more escape at the very moment when the operator is trying to connect them together with the silver wire. Another inconvenience is, that the operator cannot appreciate the degree of heat which he directs against the diseased parts, so that a great extent of tissue may be destroyed in the vicinity of the edge of the eyelid, which then presents inequalities more or less deep. One patient had a relapse, and, what was worse, a coloboma. (p. 91.)

Dieffenbach describes three methods of operating for the cure of trichiasis and distichiasis; plucking out the cilia, removal of the ciliary margin of the lid by the knife, and turning the margin outwards by excision of a portion of the skin of the eyelid. The second plan is recommended by Jäger and Berlinghieri, but Dieffenbach does not approve of it. It certainly cures the disease, but the eyelids do not close completely afterwards; and if both margins are taken away as far as the angles, the form of the palpebral opening is lost, and becomes circular like that of the owl. He prefers the third method, and after excising a portion of skin near the margin, does not bring the edges of the wound together, but leaves it to heal by granulation. He says the contraction of the cicatrix produces more eversion of the lid than union by suture, and in this way he has cured "innumerable" cases of trichiasis.

Cooking a nævus. The proposal of Carron du Villards to roast the bulbs of the cilia with pins and a pair of hot curling-tongs, does not appear to have answered; but a somewhat similar plan for destroying nævi seems to have been more successful.

Through the longer diameter of the tumour, the surgeon puts one or more long pins, such as those used by entomologists. Their length should be sufficient to allow their ends to be bent towards each other, and fixed by a small metallic noose. A lighted candle is then to be brought into contact with the pins, thus connected together, till they are heated to a white heat, taking care to put some drops of oil on the tumour. The heat transmitted through the tumour is sufficient to cook it (*pour la cuire: c'est le mot.*)! It swells up, crepitates, and then sinks down, to rise no more. In removing the pins, fragments of vessels sometimes come away, and an active suppuration does the business of the tumour in a few days, so that no reproduction of it can take place. (p. 155.)

Anthrax. This disease, it is well known, is sometimes epidemic amongst cattle. Our author seems to coincide in the opinion, that it is apt to be transmitted from the lower animals to man, by means of the œstrum, or gad-fly, which, after having drawn the blood of infected animals, may happen to attack man. It was doubtless in this way, he thinks, that the man, who was the subject of Thomassin's observation, contracted malignant pustule; although Thomassin published the case to prove, from the man's wife becoming affected, that the disease, in certain circumstances, is transmitted from the patient to those who come near him. In the month of August, says Thomassin, a labourer believed he had been bit by an insect; a malignant pustule soon showed itself on the lower eyelid, with swelling of the whole head and neck. His wife, with a pin, pricked the little vesicles which covered the pustules, and, with her fingers, moist with the serosity which escaped, she wiped away the tears from her own cheek. In about two hours after she had rendered this officious service to her husband, she perceived a tumour on her cheek, which in a few hours made an alarming progress. These two patients were cured in the hospital at Dôle, but both were disfigured. (p. 126.)

Molluscum contagiosum. Dr. Desmarres remarks, that we meet with warts on the eyelids of persons of all ages and all constitutions; but that scrofulous children and adults, and women at the climacteric period, are particularly predisposed to them. He then relates the following case:

> "I observed on a little girl of four years of age, from the asylum of the fourth Paris arrondissement, a sort of eruption of warts on the face, and especially on the eyelids, each of which had fifteen to twenty on it. They were small, soft, smooth on their surface, pretty globular, and covered with thinnish integument. Some of them, and these the largest, of the size of a hemp-seed split in two, were divided into two or three parts, and had no longer the almost rosy colour of the smaller

ones. When one pressed them between the fingers, there issued a white, milk-like, greasy matter. None of them adhered to the free edge of the lids. They had some analogy, except in point of size, with those which Schön observed on the eyelids of the right eye of a scrofulous girl, and the most perfect resemblance to those which Lawrence describes in his excellent work. (Traduction par Billard d'Angers, p. 282.) The child who was the subject of my observation was also scrofulous, and belonged to very poor parents, with whom she was ill lodged and ill fed. I used no local treatment; I prescribed merely purgatives, and afterwards syrup of quinine, anti-scorbutic syrup, a good diet, and great attention to cleanliness; and at the end of three months the greater part of the warts had disappeared. In other children I have excised these warts, in the centre of which there is commonly a small black point, which appears depressed, and resembles a good deal the prick of a pin. Cauterization was used to destroy the cyst which surrounded them." (p. 135.)

The disease here described by Dr. Desmarres is different in its nature from warts. It is the "albuminous tumour" of Mackenzie, the "glandiform tumour" of Tyrrell, the "tumeur folliculeuse" of Rayer, and, in fact, neither more nor less than molluscum contagiosum. That this disease will sometimes disappear spontaneously, or under the influence of tonics, such as quinine, is well ascertained. There is no good reason, however, for trusting to the slow process, by which the tubercles disappear spontaneously or under the influence of medicine. It is easy to divide each of them into two halves with the lancet, and press out the whole substance between the thumb-nails. The mass, thus extirpated, presents a lobulated appearance. Sometimes, when the tubercle has reached the size of a horse-bean, the interior only comes away on pressure, while the harder exterior remains adherent, simulates the appearance of a cyst, and requires the renewed and very firm pressure of the thumbs, to make it start from its bed. Numerous instances are recorded of this disease being propagated from children to adults, with whom they were much in contact. The reader who wishes to see a full account of the disease, may consult the communications of Dr. Henderson and Dr. Paterson, in the 'Edinburgh Medical and Surgical Journal' for July, 1841.

Encysted tumours of the eyelids, and chalazion. Two sections, occupying ten pages, are devoted to these diseases by Dr. Desmarres, who states, that encysted tumours of the eyelids (*kystes des paupières*) are very frequent, and that they are met with as well along the edge of the lid as at any other part of its surface. We shall venture to assert, on the contrary, that encysted tumours very rarely occur on the surface or at the edge of the eyelids. A not uncommon situation, however, for an encysted tumour, generally congenital, is close to the temporal extremity of the eyebrow, under the orbicularis, and sometimes adherent to the periosteum of the frontal bone. The expression—*les tumeurs sont très fréquents*—is sufficient proof that our author is confounding some other sort of growth with the truly encysted tumour, under the name of *kystes des paupières.*

The perusal of his account of these tumours convinces us, that Dr. Desmarres has confounded chalazion, which has no cyst, with the really encysted atheromatous, mellicerous, or steatomatous tumour. He subdivides the tumours of which he speaks as *kystes des paupières*, into two classes, subcutaneous and submuscular. The former he describes as forming a roundish, superficial prominence, gliding easily under the finger when pressed, and presenting, when laid hold of, a greater or less degree of elasticity. It may sometimes be displaced, he says, to a considerable extent, by rolling it as it were under the skin. As soon as the compression ceases, the tumour returns to the place it previously occupied. The subcutaneous tumours are in general very small, attaining the size only of a large green pea. The submuscular cysts, he says, are commonly of very limited mobility, because they have contracted adhesions to the tarsus. Some of them, separating the fibres of the orbicularis to either side, traverse that muscle, and form a greater prominence under the skin . . . . . . The compression which the orbicularis exercises upon them, ends in gradually wasting the cartilage, and often in perforating it on its inner surface. When the thinning is already considerable, if the eye is everted, the conjunctiva opposite the tumour is found inflamed to a certain extent, and in the very centre of the inflamed part there is a yellowish point, which is rendered more evident by the compression caused by the eversion. (p. 139.)

Now all this, and more, which we think it unnecessary to quote, set down by our author as descriptive of encysted tumour, can apply, we are of opinion, to nothing but the common non-encysted *tarsal tumour*, *fibrinous tumour*, or *chalazion*,—known to the vulgar by the expressive name of the *pea in the poke.*

When we turn to Dr. Desmarres' account of chalazion (*chalaze*), we find that, at the very first, he falls into the error of attributing the origin of this disease to an indurated follicle, or a hordeolum which has not suppurated, but become chronic. As to "*follicule indurée*," if he means by this a follicle of the skin, most assuredly such is not the origin of chalazion; neither does it arise from a hordeolum. *Hordeolum induratum*, or *grando*, is quite another thing from chalazion. These two diseases differ in their seat, the substance which they contain, and their origin. As to what chalazia really are, nothing definite is known, except that their contents consist of an imperfect fibrinous matter. Lawrence hints that they may be diseased Meibomian glands. Ryba considers them to be the result of chronic inflammation of the cartilage of the eyelid.

Dr. Desmarres distinguishes three varieties of chalazia, according as they project externally, internally, or in both directions at the same time. It is well known to practitioners that the disease, in whatever direction it may ultimately project, always originates between the orbicularis and the cartilage; and that the extirpation of the tumour should always be on the inner surface of the eyelid.* The operation is about the simplest in surgery. The lid is everted, the tumour divided in its whole length, through the cartilage, with the lancet; the lid is then firmly compressed, and the soft substance forming the tumour, of a yellowish-red colour, and gelatinous consistence, escapes in a mass. Instead of this simple manœuvre, Dr. Desmarres has first of all invented an instrument, which he calls a forceps-ring (*pince-anneau*), somewhat like a small pair of sugar-tongs, the one blade being solid, and to be applied to the cutaneous surface of the lid, and the other presenting a ring, to be applied to the conjunctival surface. By pressing together the blades of this forceps, the blood is prevented from flowing from the incision through the conjunctiva and cartilage. A matter of not the smallest moment. The incision being made, and the bleeding checked by the pressure of the *pince-anneau*, instead of making the tumour start from its place, by the application of the thumb-nails on each side of the incision, Dr. Desmarres directs his reader to seize the chalazion with a hook, or a pair of mouse-teethed forceps, (*pinces à dents de souris*), and dissect it out; a thing totally impracticable, on account of the softness of the tumour, and very likely to end, in the hands of an operator determined not to be beat, in the formation of a button-hole through the lid. The pith of Mr. Pott's adage, that fingers were made before forceps, is very well illustrated by Dr. Desmarres' superfluous machinery for the extirpation of a chalazion.

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* Dieffenbach insists upon this very strongly, and quotes a case in which he performed this operation on one lid from the inside, the three other lids having been operated on from the outside. No deformity followed the operation, and the lid offered a strong contrast to the others.

After some description of scirrhus and caries of the eyelids, the second part of the work of Dr. Desmarres is commenced by some remarks on the ophthalmiæ, and an arrangement of the different forms of conjunctivitis, which is by no means perfect. The following remarks on the *ophthalmia neonatorum* are worthy of notice.

"The most efficacious treatment, especially at the commencement of this disease, and while there is yet no trace of *ramollissement* of the cornea, consists in suddenly arresting the inflammation by means of the nitrate of silver. If the disease is just beginning, a collyrium of this salt, of the strength of a grain and a half to two drachms and a half (*un décigramme pour dix grammes*) of water, dropped from hour to hour, between the eyelids, will suffice; but if it has already reached the second or third period, it will be necessary to have immediate recourse to cauterization with the same salt, in solution or solid. If we choose the former, about three parts of nitrate of silver are dissolved in one part of water,* and we touch with a pencil charged with this fluid all the surface of the conjunctiva, from the free edge of the eyelids to the cornea exclusively; this cauterization is to be repeated at the end of seven or eight hours during the first two days. After the first, sometimes only after the fourth cauterization, the swelling begins to abate: it is no longer necessary then to repeat it but at intervals of twenty-four hours during some days, till the secretion and swelling have almost disappeared. In the interval of the first cauterizations, we apply over the eyes compresses wet with cold water; at the same time we wash the surface of the conjunctiva frequently with a slightly astringent collyrium.

"If we prefer cauterization with the pencil, as the application of it is more easy, and the effect more active, we begin by separating the eyelids with two elevators, then touch the ocular conjunctiva round the cornea, taking the greatest care not to cauterize this last part; that being done, and the elevators being removed, we pass the pencil rapidly over the whole conjunctiva of the upper and lower eyelids.

"To prevent the caustic, which always remains in excess on the cauterized parts, from extending to the cornea, and from becoming a secondary cause of *ramollissement* of this part, each time that the pencil touches the conjunctiva, I cause to be injected upon it a considerable quantity of water, acidulated with hydrochloric acid (*deux cuillerées à café d'acide pour deux verres d'eau*), and instantaneously transform all the excess of nitrate of silver into an insoluble chloride, which separates from the conjunctiva in small whitish flocks; this plan, tried a considerable number of times, has always been followed by the best results.

"I have found great advantage from numerous scarifications of the palpebral conjunctiva, half an hour after each cauterization; it is an excellent means for preventing the temporary swelling which commonly follows the use of the caustic.

"It is useful, at the same time, especially when the inflammation seems very acute, to apply near the eye, a leech, to be followed, if need be, by another next day; in the mean time some slight purgatives should be given to the little patient.

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* Nitrate of silver being soluble in 1 part of cold or ½ part of hot water, it is impossible to dissolve 3 parts of the salt in 1 part of water. If the salt be powdered and mixed with water in this proportion, a kind of paste is formed.—REV.

"I have no sort of confidence in the treatment of this ophthalmia with antiphlogistics, emollient lotions, and the mercurial preparations vaunted by many practitioners, in the first period of the disease. Revulsives seem to me to have no sort of efficacy. Mild collyria, praised by the greater number of authors, succeed only in those cases where a simple catarrhal ophthalmy has been mistaken for purulent conjunctivitis.

"If the cornea begins to ulcerate, or undergo *ramollissement* to a large extent, cauterization with nitrate of silver is far from being always efficacious; it always causes the swelling and the secretion to subside, but it by no means arrests the progress of the ulceration, at least, in many cases. It is then that we must distrust the concentrated collyrium of nitrate of silver, because it hastens still more the progress of the evil, and we must have recourse to purgatives, frictions on the brow with belladonna, mixed with an equal portion of blue ointment, and weak astringent collyria. Still, it must not be concealed, that these means, like all others, often fail." (p. 208.)

In the first stage of the disease, we consider the application of the solution of nitrate of silver every hour, unnecessarily frequent. Thrice in twenty-four hours would be sufficiently often; while, every two or three hours the puriform secretion should be carefully removed by means of a slightly astringent collyrium, such as a solution of one grain of bichloride of mercury, and six grains of chloride of ammonium in eight ounces of water, employed warm, with a small bit of sponge. We also regard scarification of the conjunctiva, after each cauterization, as excessive; much greater benefit is derived, indeed, from applying a leech to the external surface of each upper eyelid, than from scarification of the conjunctiva. Notwithstanding Dr. Desmarres' protest, our experience leads us to recommend one grain of chloride of mercury, to be given every twenty-four hours, and blisters to be applied behind the ears. Should onyx or abscess of the cornea occur, or the cornea appear softened or ulcerated, extract of belladonna should be added to the above-mentioned collyrium, and the same substance should be carefully painted on the lids and brow at least thrice a day. The chloride of mercury should now be omitted, and from half a grain to a grain of sulphate of quinine should be given every eight hours. This last remedy, though of the highest importance, is altogether overlooked by our author. Neither does he refer to the propriety of regulating the diet of the nurse, who is often found to be counteracting the antiphlogistic treatment applied to her child, by the use of alcoholic stimulants.

Gonorrhœal ophthalmia. Dr. Desmarres' treatment for this very dangerous disease consists, in the first place, in phlebotomy, which, he thinks, should be repeated morning and evening, as long as the state of the pulse permits it. Leeches are to be liberally applied near the eye. Every three hours, from a grain and a half to three grains of chloride of mercury are to be given internally, with the addition of a small dose (*quelques centigrammes*) of extract of belladonna. The patient is to keep his bed, and be put on spare diet. Cauterization is to be employed, as in the ophthalmia neonatorum.

Granular conjunctiva. This being a very troublesome and obstinate complaint, our readers may wish to know Dr. Desmarres' method of treatment.

When the so-called granulations are small, few in number, and pretty red, he recommends solvent salves, such as those of red or white precipitate; the bulk of a barleycorn being introduced under the eyelids morning and evening. After employing one of these salves for some time, it is well to stop and watch the effects. Calomel applied locally is sometimes very advantageous.

When the granulations are very numerous, large, disposed to bleed, and form a thick layer on the surface of the conjunctiva, Dr. Desmarres, despairing of their solution, excises them with a pair of scissors bent on the flat, taking care to leave the conjunctiva as entire as the removal of the diseased product will permit. To facilitate the operation, he inverts the upper eyelid, and introduces under it the index-finger of his left hand, with the nail turned forward. This puts the eyelid on the stretch. He employs no forceps.

Scarifying the granulations from time to time, he finds useful. If made too deep, however, scarifications, as well as excision maladroitly performed, produce cicatrices on the inner surface of the eyelids, equally injurious to the cornea with the granulations themselves.

Cauterization with nitrate of silver and sulphate of copper is of great use. The sulphate of copper is of service chiefly when the granulations are pretty vascular; when they are pale and almost cartilaginous it is worthless. Pure nitrate of silver, on the other hand, is in general too powerful. Dr. Desmarres recommends the oculist to provide himself with a series of pencils, formed of nitrate of potass and nitrate of silver, in the proportions of one half, one fourth, and one eighth of caustic, the remainder being nitrate of potass. These pencils are hard, and not easily affected by the atmosphere. He applies the pure nitrate of silver pencil when the granulations are pale, of old standing, and of great thickness. This application recalls a sort of life into them. Next day, or two days after, if they have not become sufficiently red, he touches them again. When the superficial eschar falls off, they are found to be vascular, disposed to bleed, and less in size. Then it is that cauterization every second day, with sulphate of copper, should be commenced. By and by the redness of the granulations diappears, and they tend to reassume the pale yellowish hue they had at first. Now is the time for the double nitrate of potass and silver. Dr. Desmarres considers it of the greatest importance to keep up in the granulations a vascular activity, sufficient to make them disappear (p. 223).

Vasculo-nebulous cornea. The next section is headed *Pannus,* and confirms our opinion of the faults of Dr. Desmarres' arrangement. He is now entering on one of the sequelæ of different ophthalmiæ, and actually calls his readers to the consideration of vasculo-nebulous cornea or pannus, which is the result, not of granular conjunctiva alone, but of corneitis also, before he has treated of the latter disease.

Pannus literally signifies *cloth,* and appears to have been used originally as synonymous with pterygium crassum, a disease in which there is an appearance as if a bit of red cloth were stuck upon the surface of the eye. After a time, pterygium and pannus were employed distinctively; the former being applied to the well-known triangular hypertrophy of part of the conjunctiva bulbi, while the latter was used to signify a thickening, with vascularity, of the covering of the cornea. A thickened, opaque, and vascular state of the cornea may result from three different ophthalmiæ. It may be the consequence either of scrofulous conjunctivitis, of corneitis, or of chronic puro-mucous ophthalmia. This last ending in granular conjunctiva, a vasculo-nebulous state of the cornea, especially of its upper half, is apt to follow. In scrofulous corneitis, the whole cornea having become nebulous and rough, red vessels are seen to shoot over its surface, till the whole is as red as if covered with a bit of red cloth. This is the condition to which the term *pannus* has most frequently been applied; while *vascular cornea* has commonly been employed as a distinctive appellation for the more scattered redness sometimes seen on the cornea in scrofulous ophthalmia, and *vasculo-nebulous cornea* for the condition resulting from granular lids.

We should not have thought it necessary to say so much on a question of nomenclature, had it not been that a peculiar method of curing vasculo-nebulous cornea having been introduced by Jaeger and Piringer, these oculists, or others who took upon themselves to explain their peculiar method of cure, announced it as a plan for curing *pannus;* whereas it was never meant, as far as we understand, to be applied to any other state than the *vasculo-nebulous cornea resulting from granular lids*. The plan in question, which is pretty fully noticed by Dr. Desmarres (p. 231), is nothing else than the reinoculation of the conjunctiva of the diseased eye with puriform fluid taken from the eye of another person—of a child especially, affected with mild ophthalmia neonatorum—so that the eye may again undergo the process of an acute purulent ophthalmia; and being treated energetically and systematically, may have the chance of being cured, both of the acute disease and of the chronic affection under which it had previously laboured. This proposal, we need scarcely say, must be attended with considerable danger—the danger of the new ophthalmia proving uncontrollable, and the cornea being thereby destroyed even in the best constitutions; and is, therefore, one not to be tried rashly in any case, and ought never to be ventured on in cases of pannus or vascular cornea resulting from scrofulous conjunctivitis or corneitis.

If Dr. Desmarres' account of the treatment of vasculo-nebulous cornea by inoculation is correct (for it is to be observed that he does not derive it from Jaeger or Piringer, but from other authorities at second-hand), there are several particulars about it worthy of note. 1. The conjunctiva corneæ should be, not merely red and opaque, but actually fleshy or sarcomatous; and the whole cornea should be in this state. 2. Though both eyes be diseased, one only needs to be inoculated, as the inoculation infallibly propagates itself from the one eye to the other; but this does not happen if the other eye is sound. 3. The ophthalmia produced in diseased eyes by inoculation, is always much less acute than it would be in a healthy eye; and, what is very fortunate, it seems to exhaust itself on the abnormal tissue, which becomes softened, swells, and then undergoes absorption. 4. It is only when the inflammation passes beyond these, its proper limits, that it requires to be repressed by antiphlogistics, such as cold water externally, and Seidlitz water as an internal remedy. If the pain is very acute and fever high, venesection, leeches, cupping on the temples, scarification of the conjunctiva, and more active purging, are to be employed. 5. Inoculation does not cure leucoma; it only removes the film which conceals it, and thus displays it to view. This should form partly, then, the basis of the inoculator's prognosis. 6. On the other hand, if by mischance he should inoculate an eye, in which the cornea is partly sound, and not entirely vasculo-nebulous, the sound part will become opaque from the inoculation. Some of these particulars smack a little of the incredible, so that it would require a *stout* stomach to swallow them. An American gentleman has, on this occasion, assumed the part of Palæphatus, but even his ἡ δὲ ἀληθεια ἥδε is scarcely sufficient to make them go down.

Dieffenbach devotes a chapter to the operation for the removal of pterygium, and another to pannus and leucoma. He regards pterygium not as an inflammation or degeneration *of* the conjunctiva, but as a particular organic formation *upon* it, and from which it may be separated. He raises it with a hook, and separates by scissors. We may compare his views with what Desmarres says of

Opacities of the cornea. After describing the various sorts of specks or opacities of the cornea, he enumerates a great many remedies which have been proposed for their cure. He regards the whole of them as acting only by increasing the action of the absorbents, and thinks that daily excitation of the eye, by touching it with a feather, may produce as good an effect as any of the vaunted specifics. He tried a salve, composed of axunge and porphyrized pumice-stone, and found the results equivalent to those of all other topical applications. He omits to mention the form in which he used iodide of potassium, as well as several other substances, so that we are at a loss to know whether they were employed in salve or in solution. Hydrocyanic acid, we presume, he used in the form of vapour, but even this important particular he does not state. He merely mentions that this powerful narcotic seemed to possess no advantage over other remedies for the cure of opacities of the cornea, and that he did not succeed with it in cases of photophobia. Against opacities resulting from corneitis, we know of no remedy so useful as the vapour of hydrocyanic acid. The extravagant falsehoods, published some time ago, regarding its effects in other diseases of the eye, such as cataract and amaurosis, over which it has no power, have thrown it too much into the shade. We have seen great benefit result from the use of a solution of caustic potass, two grains to the ounce, applied daily to the opacity with a camel-hair pencil.

Dieffenbach advises removal of the opacity by the knife. He says that the objection, that the subsequent cicatrix will be as opaque as the layer removed, is contradicted by experience. The operation consists in total or partial excision. Partial or superficial excision is very simple. The eyelids are kept open by the speculum, the edge of the opacity fixed by a small hook, and with a small thin knife the most superficial layer of the central portion of the opacity is carefully dissected off. This completes the operation. Cold water is applied; and after a few weeks, a further portion of the opacity is removed, and so on until the whole is taken away. The general result is, that the leucoma loses its cartilaginous whiteness, is smoother, smaller, and clearer, partly from contraction of the new cicatrix, and partly from clearing of the deeper layers of the cornea. Complete excision is performed when a leucoma is thick and prominent, about the size of a pea, and lies directly before the pupil, so that the patient cannot see. It is fixed by the hook, and removed as before, without opening the anterior chamber. The edges are brought together by very fine sutures; and Dieffenbach says that the result is great diminution in the size of the leucoma. If the whole thickness of the cornea is opaque, he passes the sutures before using the knife, cuts away all the leucoma, and instantly ties the knots, to impede escape of aqueous humour. He says he did this in one case successfully. We saw M. Malgaigne perform the superficial excision four years ago with perfect success; and we saw a patient upon whom he had performed the same operation two years before, whose cornea was so perfectly clear, that it was impossible to determine upon which eye he had operated. Six months since we performed it ourselves, and very great improvement has resulted. It is by no means difficult to do. The opacity strips off with the assistance of but a few flat touches with the edge of the knife; and we think the operation worthy of more attention than it has yet received in this country.

Procidentia iridis. Whether this be the consequence of an accidental wound, or of a perforating ulcer, of the cornea, Dr. Desmarres recommends the sedulous application, in cases of procidentia iridis, of extract of belladonna infused in iced water, by means of pledgets laid over the eye, and frequently renewed. This both keeps down the hyperæmia of the iris, and favours contraction of the protruding portion of that membrane. A solution of belladonna should be dropped, also, within the eyelids, every few minutes, so that the pupil may be as speedily and completely dilated as possible. (pp. 304-13.) Dr. Desmarres expresses a confident hope that the iris may be reduced within the cornea, even after several days, if these means are had recourse to, before the protruding portion has become gangrenous from compression. The fact is, however, that the prolapsed part of the iris never falls into gangrene, nor is destroyed by suppuration, but it becomes coated with lymph, shrinks, and is concealed at last by a leucoma or cicatrix of the cornea. There can be no doubt that belladonna has often been the successful means of causing the iris to contract, and the hernia to retire into its natural place, after several hours' prolapsus; as to days, we have great doubt. The common opinion seems to be that, after twenty-four hours, lymph is thrown out between the wound or the ulcer and the hernia, the consequent adhesion being such that no means will avail to separate it; but there can be no harm in trying the ice and the belladonna, as Dr. Desmarres directs.

In a case of extensive wound of the cornea, the instillation of belladonna had been continued for two hours without effect. The anterior chamber had become plump. As the iris had had full time to be narcotized, Dr. Desmarres thought the prolapsus might perhaps be reduced, were he to reopen the wound of the cornea. He separated the lips of the wound with the curette, and a slight touch with the instrument on the iris made it slip into the anterior chamber. This was a lucky case; he has never again succeeded in the same way.

A solution of two grains of sulphate of atropine in half an ounce of distilled water would answer better for dropping within the eyelids, in such cases, than a solution of belladonna extract.

The subject of hernia of the iris, through an ulcer of the cornea, is again taken up by Dr. Desmarres, when he comes to treat of the diseases of the iris (p. 397); and he then favours us with what he calls *a new process* for the reduction of such a protrusion. This new process consists in cauterizing, with nitrate of silver, in the neighbourhood of the ulcer. The doctor urges, that for some days after the hernia has happened, the adhesion between the cornea and the prolapsed bit of iris is very feeble, and may be destroyed, if we augment the vascular activity of the parts which furnish the means of adhesion, or if a new inflammation is produced on another part of the eye. To irritate the parts, he says, in which the vessels run which go to the ulcer, is to augment the secretion of the edges of the ulcer, and to produce around the hernia a fluid which shall separate the adhesion: "*C'est enfin rendre à l'iris les moyens de glisser dans l'ulcération, agrandie par le fait même de la sécrétion.*" (p. 398.)

We must confess that we have not the least degree of faith in any such process as that described by Dr. Desmarres. We think he must, in some way or other, have deceived himself, as to the effects which have followed the application of caustic in the cases in question.

Having narcotized the iris by belladonna, he touches the conjunctiva, close to the edge of the cornea, in three or four points, smartly, with the pencil of nitrate of silver; or he draws a line with the pencil, along the edge of the cornea, next to the hernia; and if the first cauterization fails to make the hernia recoil, he repeats it a second, third, or fourth time, at the interval of two or three days, taking great care to avoid touching the hernia itself, which would instantly cause it to grow bigger.

The only probable explanation we can offer of the conclusion to which Dr. Desmarres has come on this subject, is, that the hernia has shrunk, as it always tends to do after adhesion has formed between it and the cornea; and the anterior synechia which persisted, being very slender, has escaped his notice. He by no means pretends that his new process always succeeds in reducing the hernia. On the contrary, he gives directions, when it fails, to cauterize the hernia directly, and to try compression; or even, if it be large, to excise it.

Dieffenbach gives five successive chapters on puncture of the cornea, the operation for hydrophthalmia, keratocele, staphyloma with prolapsus of the iris, and the opaque and pellucid staphyloma of the cornea, agreeing with the French in terming what we call conical cornea, pellucid staphyloma. Before referring to these chapters we may refer to Desmarres' notice of

Black spots on the sclerotica. We have remarked such spots on the eyes of several individuals, but have seen no notice of them in books. Dr. Desmarres makes the following remarks, under the head of *Taches noires pigmenteuses de la sclérotique:*

> "We sometimes see on the sclerotica, spots of a dull black, or more commonly of a dark brown colour, of variable extent, and which are congenital. I have observed several individuals who presented this phenomenon; in one of them the sclerotica over the whole of its external half had assumed the dull black colour of which I speak; near the cornea, the spot had the tint of milk-coffee. In another case, the whole right sclerotica was black in different degrees, while that of the opposite side was of the normal colour. The young man who presented this singular phenomenon, had never suffered in his eyes, and called upon me about another matter. His irides were of a very dark colour; his cilia, eyebrows, beard, and the hair of his head, very black; his skin very brown.
>
> "Such spots of the sclerotica present no elevation, a circumstance which distinguishes them from commencing staphyloma of that membrane; they appear to me attributable to an anomalous secretion of choroid pigment." (p. 353.)

Sclerotitis, or rheumatic ophthalmia. Among the physiological or subjective symptoms of sclerotitis, Dr. Desmarres mentions photophobia, as one of the most prominent. The access of light is certainly not nearly so distressing in this disease as in phlyctenular or scrofulous ophthalmia.

Dr. Desmarres takes this opportunity of noticing a particular hypothesis on the subject of photophobia:

> "According to Augustus Bérard, who has adopted the ideas of Dr. Cade, blepharospasm and lachrymation are owing to what he calls a *cyclitis*, that is to say, an inflammation of the ciliary body.
>
> "According to this hypothesis, the pupil, in contracting, draws on all sides at the ciliary body, and produces the insufferable pains of which the patients speak. It is plain, that if this theory were well founded, those affected with mydriasis would be exempt from photophobia; but that is not the case. A young man, long affected with mydriasis, in consequence of a blow on the eye, but otherwise seeing well, was afterwards attacked with acute corneitis: I observed that the pupil remained immoveable; yet the photophobia was excessive.
>
> "However this may be, whenever the sclerotica becomes injected, photophobia follows, and certainly this is to be ascribed to no other cause than concomitant irritation of the retina. The aversion to the light is so much the greater as the vessels of the sclerotica are more numerous." (p. 360.)

We shall not pretend to decide on a question so obscure as the cause of photophobia, and about which such a diversity of opinion has existed, some locating the cause of this symptom in the nerves of the conjunctiva, others in the retina, and others, as we learn from Dr. Desmarres, in the ciliary body. We shall merely mention, that after operations with the needle for the cure of cataract, we have seen the most intense photophobia recur whenever the pupil was allowed to contract, even when there were no fragments of the lens pressing on the iris, as well as when there were; the photophobia immediately yielding to the application of belladonna. The same thing is seen every day in scrofulous ophthalmia. It would appear, as if to paralyse the ciliary nerves were, in such cases, the surest plan for relieving the photophobia. Such paralysis would render motionless both the ciliary circle and the iris.

Inflammation of Jacob's membrane. Having reached the diseases of the anterior chamber, Dr. Desmarres considers inflammation of the membrane of Descemet at considerable length, repeating, of course, a good deal of what he had previously (p. 271) said, under the head of *kératite ponctuée ou pointillée.* It is well known that the disease is a kerato-iritis, in which not merely the internal surface of the cornea suffers, but the anterior surface of the iris, and even the uvea and the crystalline capsule. Dr. Desmarres adds the membrane of Jacob. He says:

> "It participates sometimes in the disease; the retina then becomes detached, and presents itself in the form of shining yellowish folds, undulating at the bottom of the eye. This complication, which Himly seems to have observed, is moreover rare; it is the *subretinal dropsy.*" (p. 375.)

On referring to Himly, we find he makes no reference to such a disease as sub-retinal dropsy; nor, in his account of inflammation of the membrane of the aqueous humour, does he say anything of Jacob's membrane, nor of detachment of the retina. German and English works, translated into French, Dr. Desmarres is evidently familiar with; but his references to Tawdmyton (for Fawdington), Have (Lane), and the like, along with other circumstances, would lead us to suspect that he is not acquainted with the originals. In a subsequent part of his work, namely, under retinitis, Himly, indeed, observes, in a note (Himly's Krankheiten und Missbildungen des menschlichen Auges, vol. ii, p. 394), that Canstatt separates inflammation of Jacob's membrane from that of the retina, and gives as symptoms of the former, that not only is the pupil contracted, but also the colour of the iris is changed, mostly into a greenish-red; the iris, without any preceding iritis, is often pressed forwards towards the cornea, and the pupil filled with a cobweb-like exudation; the inflammation spreads from the serous membrane of Descemet, and hence after retinitis there arises indeed a capsular cataract. Even in this extract, which we believe to be the only part of his work where Himly speaks of inflammation affecting Jacob's membrane, there is nothing like the observation attributed to him by Dr. Desmarres.

Paracentesis corneæ. Dr. Desmarres entertains an unfavorable opinion of this operation in hypopium, but thinks well of it in aquo-capsulitis. Speaking of hypopium, he says:

> "Opening the cornea is recommended by several practitioners, and in particular by some English physicians, among whom I may cite Monteath, Tyrrell, and Mackenzie. The operation is at once useless and dangerous; useless, because in the great majority of cases the pus is spontaneously absorbed; dangerous, because the wound of the cornea may provoke the complete suppuration of that membrane, and even that of the whole organ. I do not think that some cases of success can justify the employment of such a means." (p. 367.)

We shall afterwards see that Dr. Desmarres must have renounced this unfavorable opinion of paracentesis in hypopium, as he adopts just the contrary view in a later part of his work. (See p. 778.)

Under the head of treatment of aquo-capsulitis, he says:

> "There is nothing particular to be mentioned here . . . . Only let us notice, that paracentesis oculi, indicated by Wardrop, is very useful in the acute stage, and that we may practise it without danger when the cornea is diseased, even to a great extent. I have not seen, like Mr. Middlemore, that the puncture of the cornea was followed by a more severe inflammation; always, on the contrary, the patients have been immediately relieved." (p. 378.)

With regard to hypopium, it cannot be denied that the pus present in the anterior chamber is sometimes absorbed; but it is equally true that even the best-directed treatment often fails to produce this wished-for effect, that the pus accumulates, the cornea ulcerates, and the eye bursts. The treatment to be pursued in such cases consists in depletion, the exhibition of calomel with opium, and the application of belladonna; but along with these remedies, great advantage is to be derived, as far as our experience goes, in puncturing the cornea, and thus taking off the pressure of the aqueous humour, and relaxing the turgid state of the whole eyeball. If there is an ulcer of the cornea present, the puncture should be made in a sound part, and at the opposite edge of the cornea from that near which the ulcer is situated. So far, then, we are of a different mind from Dr. Desmarres. As to paracentesis in aquo-capsulitis, we are unable to give an opinion. We have only to remark that Mr. Middlemore, though certainly rather against than for the practice, nowhere says that "the puncture of the cornea was followed by a more severe inflammation." His objections are chiefly hypothetical, except where he says,—"I am confident, from the experience obtained in the instances in which I have performed it, that the pain its performance occasions, and the injury it inflicts upon the inflamed membrane, and also upon the cornea, are imperfectly repaid by the temporary relief it affords."

There is nothing particular in Dieffenbach's description of paracentesis of the cornea. He says it is sometimes injurious, often beneficial. In cases of hypopium he makes a free semilunar incision of the cornea. In hydrophthalmia he recommends either simple puncture, or an incision in the sclerotica to evacuate some of the vitreous humour, or a large incision in the cornea to evacuate the humours with the pus, according to the nature of the case. The operation of keratocele, a prolapsus of the membrane of Descemet, consists in incising or excising the tumour; the former in cases of small, the latter of large tumours. In flat tumours of this kind he recommends the application of caustic potass, or of the undiluted liquor plumbi.

In a recent case of staphyloma, or prolapsus of the iris, he would assist its return by enlarging the opening in the cornea. In large, chronic, degenerated protrusions, he would remove them by the knife. He would also remove opaque staphyloma of the cornea, when it hinders closure of the eyelids, causes irritation, or the eyeball appears varicose. He fixes it with a hook, transfixes with a knife transversely, cuts upwards, instantly turns the cutting edge downwards, and completes the excision. He says that in some cases when the base of the staphyloma was only partially opaque, he has brought the edges of the wound together by sutures, preserved the form of the eye, and some power of vision. In cases of small staphyloma of the sclerotica, he punctures; in larger, makes an incision, subsequent contraction of the cicatrix thickening, diminishing, or curing the staphyloma.

Dr. Desmarres concludes his twelfth chapter with a strong recommendation of paracentesis oculi. He practises it generally through the cornea, sometimes through the sclerotica; and has recourse to it three or four times in the same day, or even in the same hour. The cases in which he advises this practice, are cases of cataract which have been operated on with the needle, acute iritis, aquo-capsulitis, hypopium, inflamed staphyloma, keratocele, acute retinitis, and congestive amaurosis. Some of the cases related, in illustration of the good effects of paracentesis of the eyes, are very striking and satisfactory. Dr. Desmarres makes no apology for having entirely laid aside the hostility to paracentesis in cases of hypopium expressed at p. 367, and which we have already noticed. At p. 778 he has become a decided friend of the very practice he formerly repudiated. He says:

> "The evacuation of the aqueous humour being, according to my observations, a powerful means of promoting the absorption of products effused into the eye, I have frequently had recourse to it when the ordinary treatment proved insufficient for the removal of pus effused into the anterior chamber. I was led to put paracentesis in practice in this case, by observing, like Werneck, that it favours the absorption of fragments of the crystalline after division of the cataract."

Transplantation of the cornea. Dieffenbach has a chapter on this subject, which he treats rather as a scientific phantasy, or a physiological experiment, than a surgical method, but still describes minutely the steps of the operation. He has succeeded in transplanting the cornea of one animal to another, removing some of the conjunctiva of the globe with it, and making the conjunctiva the bed for the sutures. He appears to think the matter worthy of more attention in cases of complete leucoma, staphyloma, &c. This reminds us of an ingenious Dutch acquaintance, who had an artificial eye constructed of lenses of different forms and density, in imitation of those of the living eye, and with a pamphlet to show that if the retina was healthy, the eye might be emptied, and filled by his machine. He never found a case suitable for its application, but the ingenuity he manifested procured him a court appointment at Madrid, so that his *châteaux d'Espagne* became more substantial than usual.

Eyes with more pupils than one. Dr. Desmarres' work embraces the congenital malformations of the eye as well as its diseases. Among those of the iris, he mentions multiplex perforation, or more pupils than one. He says: "Wardrop has seen several instances of this." (p. 380.) On turning to Wardrop, all that we find is as follows:

> "Sometimes there is more than one opening in the iris, from original malconformation. In the cabinet of Meckel there is a calf's eye preserved, in the iris of which there are two regularly-formed pupils close to one another.
>
> "Sometimes a double pupil is met with in one eye, whose other parts are single; and sometimes both lens and pupil have been found double."

For an authority on this last point, Mr. Wardrop refers us to Voigtel's Handbuch. He says nothing of any instance he had seen himself of multiplex pupil—another confirmation of our suspicion that Dr. Desmarres quotes authors, not translated into French, without ever having read them. We should not wonder if the next Frenchman who writes on malformation of the eye should state, on the authority of Desmarres, that double pupils are quite common in Great Britain.

Iritis. Dr. Desmarres divides iritis into *acute* and *chronic*, and distinguishes the acute into three degrees, the first degree being equivalent to the *serous* iritis of some authors; the second and third degrees being the *parenchymatous*. He dismisses as ineptitudes, the specific distinctions of idiopathic, rheumatic, syphilitic, scrofulous, arthritic, traumatic, and other iritides; denying that there are any appreciable physical characters by which these pretended species can be recognised. On this head we are of opinion that Dr. Desmarres betrays both a want of candour, and the most lamentable ignorance.

In proof of his want of candour, we shall quote the following passage:

> "Who can believe that in the ophthalmia called rheumatic, the pupil is perpendicularly oval, that it is transversely or perpendicularly oval in the pretended arthritic ophthalmia, that it is oblique from below upwards, and from without inwards, in syphilitic ophthalmia, while the authors themselves who adopt these divisions, admit that inflammation may affect one point of the iris more than another?" (p. 422.)

It is, without doubt, against the father of modern German ophthalmology, Professor Beer, that the above is intended to be raised as an objection; and we have seen the same contemptible stuff urged before, although at this moment we do not recollect where. The simple answer to it is, that neither Beer nor any other author ever founded the specific distinctions of iritis upon such transformations of the pupil as are here set down by Dr. Desmarres. That changes of place and form in the pupil have been observed in certain cases of eye-disease, and have been mentioned by various authors, even in iritis, and even by Beer (and we shall immediately quote every word that Beer has said upon the subject), we do not pretend to doubt; but that any author has, of such changes, taken more than an ordinary notice, as of facts which they had seen amongst other facts, much less that such transformations have been made the basis of a classification of the species of iritis, we deny. It is one thing to mention a fact which one has observed, in a recital of numerous other facts pertaining to the same subject; and it is another thing to select a fact from among numerous other facts, and put it prominently forward as the groundwork of a classification. To serve the latter purpose, the fact selected would require to be one of paramount importance and uniform occurrence; one casually mentioned may very probably be of a trivial nature, and its occurrence liable to frequent exceptions.

In Beer's account of idiopathic or rheumatic iritis, he mentions, as symptoms, immobility and contraction of the pupil, along with swelling of the iris, and projection of that membrane towards the cornea; but not one word of the pupil becoming perpendicularly oval. We know of no author who mentions this as a symptom. It seems to be a mere gratuitous assumption on the part of Dr. Desmarres, or of those he has copied, that any author ever stated such a thing.

In his description of syphilitic iritis, Beer, having delineated, with his usual accuracy and minuteness, the state of the sclerotica and conjunctiva, the cornea and the aqueous humour, comes to the changes which take place in the iris itself. "The iris," he says, "becomes every moment less moveable, the pupil grows small, retires from the middle of the eye, and places itself gradually towards the inner angle of the eye, and upwards towards the root of the nose, visibly losing at the same time its circular form, and becoming very angular, while first the little and at length the great ring of the iris grows discoloured, as in every other iritis."* The reader will observe, that Beer assigns no extraordinary diagnostic importance to a fact, which he had undoubtedly observed in certain cases of syphilitic iritis, namely, a displacement of the pupil inwards and upwards. Of its being oblique, he says nothing. That the pupil is often displaced in syphilitic iritis agrees entirely with our experience; but we should not, any more than Beer, insist upon this fact as one of much importance with regard to diagnosis. Mr. Lawrence, in a work which he announces as *drawn up entirely from his own experience*, thus describes the pupil in syphilitic iritis: "The effusions of lymph and the adhesions change the figure of the opening, rendering it angular, and often extremely irregular. Together with other changes, the pupil sometimes undergoes an alteration in situation, being apparently drawn upwards and inwards, or towards the root of the nose. It may deviate in other directions."†

It is well known to those who have studied German ophthalmology, that Beer distinguishes two varieties of arthritic iritis, the one with contracted and the other with dilated pupil. In the latter variety, which is always accompanied by glaucoma and amaurosis, he states that "the pupil does not dilate equally, but more towards the angles of the eye; the iris becomes at length, towards both angles of the eye, but especially towards the outer, almost invisible; the pupil acquires in this way some resemblance to that of the ruminantia."* That this description is true to nature, drawn exactly from cases which had occurred in Beer's practice, there cannot be, in any candid and observing mind, the shadow of a doubt. Beer would have committed a delinquency, had he omitted to notice the facts as they presented themselves. That he attached any undue importance to them, or made them the basis of any classification, is not true. The same remark applies to his followers.

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* Lehre von den Augenkrankheiten, vol. 1, p. 557. Wien, 1813.

† Treatise on the Venereal Diseases of the Eye, p. 141. London, 1830.

Dr. Desmarres, while he acknowledges that a general affection of the system, of whatever nature, may in its progress modify an ophthalmia, and that syphilis undoubtedly involves the iris, denies that any such affection manifests itself in the eye by appreciable characters; affirming that it is impossible to distinguish one variety of iritis from another, and therefore useless to attempt it. At this belief Dr. Desmarres can have arrived, only from not making the symptoms of iritis, as they are presented in different individuals, a subject of sufficiently careful observation.

In determining the species of iritis, however, it is not merely the anatomical changes presented in the eye that are to be taken into account; the state of the body generally is equally to be appreciated; and the history of the patient's previous health is carefully to be ascertained. Dr. Desmarres styles himself *professeur de clinique ophthalmologique,* but he must be unacquainted even with the *initia clinicarum,* if he does not know that the diagnosis of a disease is drawn, not merely *ex symptomatibus,* but *ex causis, ex toto morbi habitu,* and even *ex proclivitate ægri.* A great deal has been written *de oculo ut signo,* that is to say, on the eye as offering signs of different constitutional and local diseases. In the same way it might be shown, that there is no part of the system, which, in certain cases, does not throw light on the diagnosis of eye-diseases; and of such signs the clinical professor, above all, is bound to take account.

If, therefore, a patient of good general health, and free from all signs of constitutional taint, presents himself with an eye marked by zonular sclerotitis, discoloration of the iris, contraction, irregularity and immobility of the pupil, with lymph in moderate quantity effused into the posterior chamber, and the iris, by the medium of that lymph, glued down at different points to the crystalline capsule, and all this independent of any wound, or mechanical injury of the eye, we should at once pronounce the case to be one of idiopathic or rheumatic iritis.

Such a case as that just now described is styled rheumatic iritis, although the patient may never have suffered from any rheumatic affection before. The disease has been excited, as rheumatism generally is, by exposure to cold, perhaps when the individual was overheated; and the fibrous tissue of the eye, which, it is presumed, is liable to rheumatic inflammation, is evidently involved, as well as the internal muscular apparatus of the eyeball, the iris. We shall not contend for the absolute propriety of the name *rheumatic;* the disease may be styled idiopathic, pure, or simple iritis; or, as the quakers do with the streets in Philadelphia, call it No. 1.

Another patient presents himself, with his eye and his constitution in a different state from those of the former. The cornea and aqueous humour of the former patient were normal; but here it is plain that a thin coating of lymph covers the whole of the lining membrane of the cornea and anterior surface of the iris, while the aqueous humour is so turbid, that with some difficulty we discern a mass of lymph filling up the pupil, and projecting even into the anterior chamber. The patient's joints, especially his knees and ankles, are stiff and swollen; he has evidently been suffering, at no very remote period, from acute synovitis, and this was preceded by gonorrhœa. Perhaps the history he gives of himself reveals a long-continued concatenation and repetition of these three related diseases, gonorrhœa, synovitis, and iritis. The condition of the eye itself is sufficient, however, to proclaim what the disease is; for in no other species of iritis than the gonorrhœal is there so great an effusion of lymph, in the cavity and on the walls, of the aqueous chambers. Here, as in the former case, the name *gonorrhœal* may be objected to. Let the disease, then, be known as No. 2.

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* Op. cit., p. 581.

If, again, a patient is seen to exhibit, in addition to zonular redness of the sclerotica, and the other symptoms enumerated in case No. 1, a rusty red colour of the inner or little ring of the iris, with one or more small tubercles or abscesses, of an orange hue, rising on the surface of the iris, or from the edge of the pupil, and if the copper-coloured pits of a leprous or tubercular eruption are evident on the forehead, trunk, or extremities, shall we hesitate for a moment to pronounce the case, No. 3, to be one of *syphilitic* iritis?

If the patient is a child or young person, the whole eyeball presenting a conical form, the iris but slightly discoloured, its serous covering being chiefly affected, the pupil filled with scattered threads of lymph, the disease perhaps of long standing, and the eye suffering many relapses, the constitution evidently weakly, the flesh soft and flaccid, and the glands under the jaw enlarged, we need no ghost to tell us that the case, No. 4, is one of *scrofulous* iritis.

The patient is far advanced in life, the edges of his eyelids and the angles of his eyes present an accumulation of whitish foam, the sclerotic redness is of a livid hue, and over the eye run twisted varicose vessels; at the junction of the sclerotica and cornea there is a narrow opaque ring of a blueish-white colour, the pupil is irregular, perhaps dilated, the iris thinned, as if from absorption of the uvea, the lens is glaucomatous, the eye hard as a stone, little or no lymphatic effusion, vision almost or quite extinct; the patient has long been tormented with headaches, giddiness, loss of appetite, flatulency,. acid eructations, costiveness, and dejection of mind. The general and local symptoms declare this case, No. 5, to be totally different from any of the former, and to correspond to the account commonly given of *arthritic* iritis.

The utility of these distinctions bears entirely and directly on the prognosis, and the method of cure. In No. 2, for example, however thick may be the lymph that fills the pupil, so far affecting vision that the eye scarcely discerns light and shade, only let the appropriate treatment be actively followed out, and the eye will be as perfect as ever, not a vestige of adhesion or irregularity of the pupil being left, nor the slightest dimness of sight. The same means employed in No. 5 would speedily extinguish the little vision left, and hurry the patient, in all likelihood, to his grave.

Here for the present we think it well to pause, since the subjects of cataract, artificial pupil, and others which remain to be discussed, demand more ample space for their due consideration than we can at present allot to them. We shall return to them, therefore, in our next number; in which we propose to give an analysis of other parts, also, of Dieffenbach's great work. The critical attention which we have bestowed upon Dr. Desmarres' treatise, and the numerous extracts we have made from it, sufficiently show that we attach considerable importance to his production. Many of the extracts bespeak, we think, inferiority—a few superiority—in pathological knowledge to the general run of English authors on the same subject. To answer the purpose intended by such a work, it should contain an accurate description of all the diseases of any importance to which the organ of vision is subject, and an account of the various methods of treatment which have been found most successful, not only in the hands of the author, but of the most eminent practitioners, both ancient and modern, in different parts of the world. If the execution of this task has on this occasion been imperfect, it is from Dr. Desmarres' experience being probably yet but limited, and from his not being acquainted with any of the German and English authors on ophthalmology, except the few whose works have been translated into French. That the work is, on the whole, a failure, we by no means wish to insinuate. On the contrary, we have no hesitation in stating our opinion that it is a production of great labour and great merit; that it indicates an immense advancement in ophthalmological knowledge in the country where it has been produced; and that the French medical practitioner, for whose use the work is no doubt chiefly intended, may derive from the perusal of it a very great deal of valuable information, and of the most practical kind. Dr. Desmarres avoids long historical dissertations and tedious narrations of individual cases, and keeps very closely to plain and useful details.

The work is well printed, in a small, clear type, on good paper, and the woodcuts are, on the whole, well executed; especially those illustrative of the operations on the eyeball. The price, nine francs, is extremely moderate. Such are the heavy, the all but unbearable, imposts on materials and labour in this country, that a similar work could not be produced here under three times that price. With all these advantages, however, we are not surprised, on reviewing this, the most recent, confessedly the best, indeed almost the only French systematic work on diseases of the eye, that translations of Dr. Mackenzie's work should still be the favorite text-book not only in France, but, as we have found it, in Italy, Spain, and Greece. Either in its French or Italian dress, it is sold for about six shillings, with copious notes, and is not only studied but *trusted* and followed by our continental brethren, who know well how to appreciate that difference between the ophthalmic hospitals of this country and those of France, to which we have alluded at the commencement of this article, and the higher *moral* tone of our medical writers. We are happy in having this opportunity of recording our sense of the obligations the profession are under to Dr. Mackenzie, for the manner in which he has extended the European reputation of British surgery.

ART. II.

1. *Principles of Scientific Botany; or Botany as an Inductive Science.* By Dr. J. M. SCHLEIDEN, Extraordinary Professor of Botany in the University of Jena. Translated by EDWIN LANKESTER, M.D., F.R.S., F.L.S., &c., Lecturer on Materia Medica and Botany at the St. George's School of Medicine.—*London*, 1849. 8vo, pp. 616. With five Plates and numerous Wood-engravings.
2. *A Manual of Botany; being an Introduction to the Study of the Structure, Physiology, and Classification of Plants.* By JOHN HUTTON BALFOUR, M.D., F.L.S., F.R.S.E., Professor of Medicine and Botany in the University of Edinburgh.—*London*, 1849. Small 8vo, pp. 642. Illustrated by numerous Woodcuts.
3. *Elements of Botany.* By M. ADRIEN DE JUSSIEU, Membre de l'Institut, Professeur au Muséum d'Histoire Naturelle, &c. Translated by JAMES HEWETSON WILSON, F.L.S., &c.—*London*, 1849. Fcap 8vo, pp. 750. Illustrated by numerous Woodcuts.
4. *Rudiments of Botany: a Familiar Introduction to the Study of Plants.* By ARTHUR HENFREY, F.L.S., Lecturer on Botany at St. George's Hospital, &c.—*London*, 1849. 18mo, pp. 250. With illustrative Woodcuts.
5. *Zur Entwickelungs-geschichte der Farrnkräuter, vom* Grafen LESZCZYC-SUMINSKI.

On the History of the Development of Ferns. By Count LESZCZYC-SUMINSKI.—*Berlin*, 1848. 4to, pp. 26. With Six coloured Lithographic Plates.

EVERY physiologist is acquainted with the name and reputation of Professor Schleiden, who first distinguished himself about twelve years ago by those researches on the origin and development of vegetable cells, which, if they did not originate the researches of Schwann on the development of the animal tissues, furnished him with important aid and guidance;* and who about the same time promulgated certain novel doctrines regarding the process of fecundation in flowering-plants, which have ever since remained the subjects of active controversy. His reputation as a botanist, however, by no means rests on these works alone; for he has published several other excellent monographs, especially one on the Cacteæ, which have contributed to raise and extend it; and the treatise now placed before us in an English dress, is already in its third German edition, which sufficiently indicates the appreciation it has acquired in the country which gave it birth. It differs in its general character and arrangement from all preceding treatises on Botany; and presents the anatomical and physiological departments of the subject in a far more scientific aspect than they have ever before been made to exhibit. It is full of original observations of great value; and has this remarkable merit, that the author has taken very little upon trust, but has examined into almost every point for himself. We need not say, then, that its reproduction in our own language, under the able editorship of Dr. Lankester, (who has had the advantage of Mr. Henfrey's assistance,) is a great boon to the British botanist, who has too often laid himself open to the reproach of ignorance with regard to the progress of his science elsewhere, and especially in Germany. Moreover, the appearance of this work, concurrently with two other botanical treatises of which Vegetable Anatomy and Physiology form a large part, affords a cheering indication that the younger botanists of this country are not any longer content with being mere collectors of species, accumulators of what Professor Schleiden sarcastically calls so much *hay;* but that they are setting themselves to acquire a knowledge of the structure and development of plants, and are likely to take their fair share in the labours whereby that knowledge may be extended and rendered precise. We cannot refrain from quoting the following passage, which amusingly and at the same time forcibly displays the errors of the mode in which too many botanical researches have been carried on. When speaking of the small amount of our knowledge regarding the development of the Algæ, Professor Schleiden remarks:

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* See Brit. and For. Med. Rev., Vol. IX, p. 497.

> "In some degree, certainly, the cause lies in the artificial, senseless methods of research that infect the whole science of Botany, which has hitherto directed its attention far more to herbaria than to living plants; and hence, those who live far inland become Algologists, whilst botanists residing near the seaside busy themselves in describing some little dried Jungermannia brought from Java; and European investigators thus too often devote more attention to tropical plants than to those that are indigenous to their own country, and are in their own neighbourhood." (p. 149.)

It is to be borne in mind, however, that the general term Botany includes two subjects, really as distinct from each other as are Zoology and the Anatomy and Physiology of animals; and although neither in the animal nor the vegetable kingdom can these two departments be advantageously studied in complete isolation from each other, yet it is absolutely necessary for their advancement that there should be a division of labour; and we would not therefore ridicule or undervalue the labours of any, who, having first made themselves acquainted with the general outlines of the whole, choose any single part of the domain as the portion which they will especially cultivate. But that which we do, with Professor Schleiden, deem worthy of condemnation, is the spirit of the mere collector of dried plants, who estimates his botanical acquirements by the number of species of which his cabinet can boast, and is all the while in a state of profound ignorance of everything, save the external characters of his specimens. To such we imagine that Professor Schleiden's exposition of what really constitutes botanical science, would be just as intelligible as a treatise on the differential calculus or the lunar theory. To those, however, who have already studied Botany in the same spirit, or who are prepared to do so by their previous study of Animal development (to which science Professor Schleiden makes continual reference as being far in advance of botany, both as to its method and as to its attained results), this treatise will supply ample materials for the improvement of their knowledge, both by the amount of facts it contains, the excellence of the plan on which these are combined and made to evolve principles, and the suggestions to which almost every part gives rise in the mind of the reader who is competent to appreciate the work. The following brief summary of its contents will serve to indicate the method pursued by the author.

The work is divided into four books; of which the *first* is devoted to an exposition of the chemistry of plants, under the heads of inorganic and organic elements. The *second* treats of the "plant cell" in two chapters; one describing its various modifications of form, the other its vital actions, both as an individual, and in connexion with others. The third book is entitled "Morphology," and embraces a general view of the form and structure of each principal tribe of the vegetable kingdom, commencing with the simplest Algæ, and proceeding upwards to the highest Cryptogamia; this exposition being conducted on the basis of the *history of development*, which is what constitutes its peculiar feature. The fourth book, entitled "Organology," is what we should have rather termed physiology, as it is intended to embrace an account of the "general phenomena in the life of the entire plant," and of the "special phenomena in the life of the entire plant," as well as of the functions of special organs. In the appendix we find a section "on the use of the microscope in botanical investigations," which formed a part of the introduction of the original; another part of the introduction, which consisted of an abstract discussion on methology, founded upon the study of the powers of the percipient mind in their relation to the objective phenomena of nature, has been altogether omitted, not, we think, at all to the detriment of the work as a scientific treatise on botany.

The task of translation has been, on the whole, extremely well performed by Dr. Lankester, especially considering that Professor Schleiden's style is none of the most facile. An apology is offered by Dr. Lankester, in his preface, for the inelegance that may be charged against some passages; which has resulted, he tells us, from his "anxiety rather to give a correct notion of the author's meaning, than, by the use of other language, to diminish the force of the original." We have met with scarcely anything that can be termed obscure; and, on the whole, there is little in the diction to indicate the foreign origin of the book. We are disagreeably reminded of this, however, by continually meeting with measurements, &c., expressed according to foreign standards, which we should have been much better pleased to find "done into English." In fact, we cannot regard any translation as at all complete, in which this has not been accomplished; and if the editor or translator would only reflect that by the neglect of this part of his duty he inflicts the labour which he has shirked on *every-one* of his readers who may desire to make himself acquainted with the author's statements on such points, he would not, we think, shrink from this complete fulfilment of the duty which he has undertaken. We have to note, too, that the references between one paragraph and another are frequently incorrect, altogether failing to guide us to the intended information. The index, however, is an excellent one, and we have found it both copious and accurate.

Having said thus much in regard to the general plan of the work, and of that portion of its execution which devolves upon the translator, we might be expected to proceed with a detailed examination of the author's views. Such an examination, however, would be altogether foreign to the general plan and scope of our Journal; and we must content ourselves with taking up a few subjects which are likely to have the greatest interest for our readers. But before we bring to a close this general sketch of Professor Schleiden's treatise, we feel called upon to animadvert most seriously upon the tone and spirit which are displayed in several parts of it, as being totally unworthy of a man who occupies a high scientific position, and who professes to make truth his sole aim. It would seem as if the author could never express his dissent from the opinions of a brother botanist, without giving vent to a sarcasm or an expression of ridicule, sometimes directed against him personally, sometimes against the method of research he has employed, sometimes against the school of phylosophy to which he belongs. There is scarcely an eminent contemporary botanist to whom he does not thus apply the lash in one part of his work or other; in fact, we believe that the only exception is our own Robert Brown, whose name, so far as we have observed, is never quoted but with respect. Now there is an old proverb that "those who live in glass houses should not throw stones;" and Professor Schleiden would do well to profit by it. For his love of originality and his disposition to dissent from usually-received doctrines, often (it would appear) for the mere sake of dissenting, have led him into no small number of rash statements and assumptions, which fairly expose him,—more, perhaps, than any other botanist of the day,—to the attacks of rivals and critics. In fact it is not a little remarkable, that the inferences he drew from those researches on cell-development and on the process of fertilization, which first made him known to the scientific public, and on which his *general* reputation is still chiefly founded, should be now in great part disproved. And yet it cannot be questioned that those researches have been the indirect means of educing what we now believe to be truth, by stimulating the zeal and sharpening the acumen of other observers; and that in constantly dwelling, as he has done, upon the history of development, as our chief guide in the classification and interpretation of phenomena, Professor Schleiden has given a much truer *direction* to botanical inquiry than it had previously taken, and has thus laid the foundation of an edifice on which others may be proud to labour.

The second work on our list is far less ambitious in its design; the author's object being, not to build up any system of his own, but "to give a comprehensive and, at the same time, condensed view of all departments of the science." The following extract from the preface will sufficiently indicate the scope of the work:

> "Attention is directed, first, to the elementary structure of plants, and the functions of the simplest tissues, and then to the compound organs, and the functions which they perform. In the consideration of these subjects, the works of Jussieu and Henfrey have served as a model. The application of physiology to agriculture, both as regards the cultivation of plants and their diseases, is brought under notice, the works of Liebig, Mulder, and Johnston having been consulted. In the important subject of classification, much aid has been derived from the standard work of Lindley. The system adopted is that of De Candolle, but in the arrangement and definition of the natural orders, Walker Arnott has been chiefly followed. Many important hints have been derived from Henslow's excellent Syllabus, as well as from the systematic work of Endlicher. In detailing the properties of plants, care has been taken to notice all those which are important in a medical and economical point of view; Christison, Royle, Burnett, and Lindley, supplying valuable data. In the chapter on the geographical distribution of plants, a very general view is given of the principal facts brought forward by Meyer, Schouw, Humboldt, Berghaus, Watson and Forbes; and in fossil botany, the labours of Brongniart, Ansted, and Hooker, have been made available. . . . . By combining together in the Manual, information which the student has to acquire by the consultation of several volumes, it is hoped that the work may be useful as a text-book." (pp. ix, x.)

The illustrations are, for the most part, the same with those which were executed for M. Adr. de Jussieu's treatise; but others have been added from various sources. We have great pleasure in giving to this treatise our warm commendation, as being on the whole admirably adapted to its designed

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purpose, presenting the different departments of Botany in their due relation and subordination to each other, and in a manner calculated to interest the student, and to lead him on from the simpler and more elementary, to the higher and more difficult subjects. In particular, considering how deficient was the botanical teaching of the Edinburgh school in our day, in everything like a true appreciation of the fundamental facts and principles of the science, it is refreshing to find the present occupant of the chair fully recognising the importance of physiological investigations, not merely for their own sake, but as affording materials essential to the determination of the true alliances and affinities of plants. We could certainly have wished that Professor Balfour had more completely emancipated himself from the trammels in which the too exclusive study of flowering plants is almost certain to involve the botanist, and had more pointedly directed the attention of his readers to the study of the morphology and physiology of the Cryptogamia; since every fresh discovery affords a new confirmation to the doctrine we have long and perseveringly preached, that the essential phenomena of vegetation can only be rightly comprehended, when they are studied under their simplest conditions. Thus we shall presently show that the history of the process of "cell-fission" has been more truly appreciated from observations on the Confervæ, than from those made on any other tribe of plants or animals; and that from the conjugation of the simplest Palmellæ we may derive our most comprehensive expression of the real nature of the "act of generation." If every botanist would apply himself to the examination of a few of the most common forms of vegetation within his reach, and would follow them through their successive stages of development and reproduction, we should soon be in possession of materials of the highest value in general physiology, as well as in the history of that particular tribe. We have a most remarkable illustration of the truth of our assertion, in the success which has attended the labours of a distinguished amateur, Count Suminski; whose researches on the development of Ferns will take rank, we venture to affirm, with the most remarkable botanical discoveries of the age. We shall give an account of his labours in their proper place; and shall at present only further remark in regard to his work, that the plates which accompany it (lithographed from his own drawings) present the most remarkable combination of artistic beauty with fidelity and distinctness of delineation, that it has ever been our good fortune to see in illustrations of vegetable anatomy.

The 'Elements of Botany' of M. Adrien de Jussieu forms part of the Elementary course of Natural History adopted by the 'Conseil Royal de l'Instruction publique,' for the use of the Colleges in France, being a companion-volume to that of Professor Milne-Edwards on 'Animal Physiology and Zoology,' and to that of M. Beudant on 'Mineralogy and Zoology.' Having been written for the same purpose as Professor Balfour's,—namely, as a text-book for students,—it would be unreasonable to expect in it any original contributions to our knowledge of the subject; and it is in many respects well adapted to its purpose, though it bears the marked impress of the older French school, which, though at one period the most advanced, has by no means kept pace with the progress of modern botanical science. The remarks which we have made as to what we consider the chief imperfection in Professor Balfour's treatise, apply still more strongly to this; the whole system of anatomy and physiology being founded almost exclusively upon the higher plants, and the history of development but little brought into notice. Still it was in many respects an advance upon previous text-books; and the English translation, which is very creditably executed, would have been a useful and acceptable introduction for botanical students in this country, if it had not been superseded by Professor Balfour's treatise, which, though framed upon the same general plan, has the advantage of being much more on a level with the present state of the science.

Mr. Henfrey's little 'Rudiments' are designed simply to furnish the beginner with a simple and practical introduction to the study of plants; and they are admirably adapted for their purpose. They possess this great advantage over most other elementary books,—that being the composition of a. botanist who has given proof of his acquaintance with the highest departments of the science, the student who follows its guidance may be assured that he will have nothing hereafter to unlearn; which is the case with too many of the introductions to science written by mere compilers having no practical knowledge of their subject.

Having thus endeavoured to place before our readers a correct idea of the character of each of the works upon our list, we shall proceed to lay before them the results of recent inquiries upon three subjects which may be considered to be of most special interest to the animal physiologist;—namely, the general doctrines of cell-development, the circulation of fluid, and the act of generation.

Formation and multiplication of cells. . The wall of the vegetable cell, which has usually been regarded as a simple membrane, is now well known to be made up, in most if not in all instances, of two layers of very different composition and properties. The inner of these layers, which has received the name of "primordial utricle," appears to be the one first formed, and most essential to the existence of the cell; it is extremely thin and delicate, so that it escapes attention so long as it remains in contact with the external layer, and is only brought into view when circumstances occasion its separation from this; it seems to consist of an azotized compound, probably of an albuminous nature; and it appears to participate actively in the vital operations of the cell. The external layer, on the other hand, though commonly regarded as the proper "cell-wall," seems to be generated on the external surface of the primordial utricle, after the latter has completely inclosed the cavity and its contents, so that it cannot be regarded as essential to the cell; it is usually thick and strong in comparison with the other, but it may possess various degrees of consolidation, from mere mucus to a firm and tenacious substance; it is composed of cellulose, a substance nearly identical with starch; and it does not appear to take any active share in the vital operations of the cell, its principal object being to protect, locate, and isolate the matter it contains. This external layer may consist of many laminæ, the results of successive deposits from the surface of the primordial utricle; and the continuation of this process of deposition of concentric layers may occasion the gradual contraction of the cavity of the cell. After the outer layer has been completely formed, however, the additional matter is usually so disposed as not entirely to line it; sometimes taking the form of a spiral fibre, sometimes being deposited in somewhat irregular patches, and sometimes forming layers which are interrupted by perforations, so that the contracted cavity still sends radiating prolongations through the secondary deposit as far as the external cell-wall. These various modes of deposit are concerned in the production of the numerous forms of tissue, of which the vegetable histologist has to take note, but of which it is not requisite here to give any detailed account.

The granular matter contained in the interior of the cell, being usually coloured, has received the name of "endochrome," which is a convenient designation of the aggregate contents of the vegetable cell; and all the vital phenomena which the cell exhibits have their seat in it. Amongst these phenomena, there is one which, although formerly recognised only in a small number of aquatic plants, had been surmised to be present in every cell at a certain stage of its growth,—a surmise which derives strong confirmation from the recent observations of Schleiden and others. The phenomenon to which we allude is the movement of rotation of a portion of the cell-contents,—chiefly, as it would appear, of a viscid layer of albuminous matter, carrying along with it granules of starch and chlorophyll, by whose progress the movement is recognised,—over the inner surface of the primordial utricle. In most plants of the families of Characeæ, Nayadaceæ, and Hydrocharaceæ, in which the movement has been long known, we may observe a single broad current, sweeping up one side of the cell and down the other, and returning continuously into itself; the ascending and descending currents do not come into absolute contact, being separated by a clear space in which no motion can be detected; but we have sometimes observed a large granule project so far from the border of one current, as to entangle and carry with it a granule projecting in like manner beyond the edge of the other stream. If the cell be carefully tied across, the current is in a short time reestablished in each segment, as if the cell had undergone a natural subdivision.

The vigour of the movement increases and diminishes in accordance with the general vital activity of the cell; as appears, not merely from observation of its ordinary growth, but also from the exciting or depressing effects of physical agents. In the cells which exhibit this form of circulation, there is no fixed nucleus; but one of the moving aggregations of granules is usually much larger than the rest, and seems to have a nuclear character.—A movement essentially similar, but differing in some of its conditions, was long since observed in the hairs of several flowering-plants; and it has been especially studied by Schleiden, who has now collected several hundred examples from the most various families of Cryptogamia and Phanerogamia, and has obtained these from different parts of the fabric. The current, in these cases, is never so broad as in the previous form, and usually presents itself as a thread-like stream passing through a stratum of motionless matter. This stream may be single, passing up one side of the cell, and down the other, as is usually the case in the long and tubiform cells of hairs; but several distinct currents may exist in the same cell; and these are observed to have a common point of departure and return,—namely, that collection of granular matter into one mass attached to the wall of the cell, which is known as the "nucleus" or "cytoblast." Here, too, it would appear that the currents are connected with the general activity of the life of the cell; and that their cessation indicates the cessation of its formative powers.—The young cells of the common "snowberry" are spoken of by Schleiden as exhibiting this movement very characteristically.

The "nucleus," in the latter form of this phenomenon, might seem to exhibit itself as the centre of the vital activity of the cell; but the still more active circulation first described, in cells which have no distinct nucleus, or in which the nucleus, being carried along in the current, cannot be regarded as having anything to do with the production of that current, shows that the force which creates it must be diffused through the entire cell. And this we believe to be the truth with respect to the vital powers of the cell in general. The nucleus is certainly not essential to its production, and frequently does not make its appearance until an advanced stage of development; and yet the cells in which it is absent exhibit no inferiority of endowment to those in which it is present from the first. Perhaps we should be correct in regarding it as concentrating in itself, where it exists, a portion of the forces which are elsewhere diffused through the "endochrome" and the "primordial utricle;" rather than as having any powers peculiar to itself; and this idea is confirmed by the facts next to be stated in regard to the multiplication and development of cells.

It may be stated with certainty as a general fact, that every cell owes its origin in some way or other to a pre-existing cell; but the mode in which new individuals are generated is by no means constant, and there are some forms of the process which are not yet clearly understood. That which is already known, however, leaves no room for doubt that in every instance a portion of the "endochrome" of the parent-cell is the starting-point of its successor, and that although the "primordial utricle" may be also concerned, the outer cell-wall is entirely passive in the matter. The most common method of multiplication in plants, is the subdivision of the original cell into two halves, such as is seen very characteristically in many of the simplest forms of cryptogamic vegetation, in which every cell is a distinct individual. The process has been most completely studied, however, in the Confervæ; and the observations of Mohl,* confirmed by Henfrey and Thwaites, and we may add by ourselves, give the following as the chief stages of the process. The endochrome first appears slightly constricted in the middle of the length of the cell, and the primordial utricle is inflected around it; and by the increase of this construction, there is gradually formed a sort of hour-glass contraction across the cavity of the parent-cell, by which it is divided into two equal halves. The two surfaces of the infolded utricle produce a double layer of permanent cell-membrane between them; and the formation of this may be observed to commence, even before the complete separation of the cavities of the twin-cells. This deposition is not confined, however, to the contiguous surfaces of the young cells, but takes place over the whole exterior of the primordial utricle; so that the new septum is continuous with new layers which are formed throughout the interior of the original cell-wall, and which constitute the proper walls of its offspring. It does not appear that in this process the nucleus performs any essential part; for we find it taking place where no nucleus can be distinguished, in virtue, it may be surmised, of a sort of mutual repulsion between the two halves of the endochrome, leading to their spontaneous separation. Where a nucleus is present, this also undergoes subdivision, so that half of it is appropriated by each of the twin-cells; but the constriction of the endochrome may frequently be observed to commence, before there is any indication of that of the nucleus.

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* Vermischte Schriften, p. 362.

The foregoing is the method according to which the extension of vegetable structures from cells already in existence appears most commonly to take place. Among the lower Algæ, for example, we find the single cell giving rise to an amorphous cluster composed of aggregations of distinct individuals imbedded in a gelatinous "matrix;" to a prolonged filament composed of a single file of cells; or to a flattened leaf-like expansion; according to the mode and direction in which the subdivision takes place, and to the degree in which the new cells continue to be held together by the cell-wall of the parent. It is in this manner, again, that the cell-multiplication takes place in that germinal mass in which not merely the higher plants, but the highest animals also take their origin; and the process continues to take place on the same plan, until certain parts of that mass begin to undergo transformation into heterogeneous structures, in which case we commonly find it superseded by some other method. But we still meet with it in the multiplication of the cells of certain tissues, especially cartilage; in which the phenomena just described appear, from Dr. Leidy's observations,* to be precisely repeated.

But we sometimes observe that new cells originate in little bud-like prominences on the surface of the parent-cell, which are developed in continuity with that from which they arise. This is well seen in the *Conferva glomerata,* a common species, which increases (as pointed out by Mr. Thwaites), not merely by the repeated subdivision of the cells at the extremity of its filaments, but by the origination of new cells from every part of their surface. A certain portion of the primordial utricle seems to undergo increased nutrition; for it is seen to project, carrying the outer cell-wall before it, so as to form a protuberance, which sometimes attains a considerable length before any separation of its cavity from that of the parent-cell begins to take place. This separation is gradually effected, however, by the infolding of the primordial utricle, just as in the preceding case; and thus the endochrome of the young cell is completely severed from that of its parent, and its separate individuality may be said to commence from that time. We may consider this process of "budding" as differing from that of "subdivision" only in this,—that whilst, in the latter case, the individuality of the parent-cell is lost by the *equal* subdivision of its cavity into two similar parts, it is retained in the former through the *unequal* division of the cell, of which only a small portion is pinched off (so to speak) to form the new cell, while the greater part remains unaltered.† Of the extent to which the multiplication of cells by this budding process takes place among plants, we have as yet no certain knowledge. It is obviously the regular mode of growth in the Characeæ; in which the long tubiform cells that form the axis give off, at their points of junction with each other, circular rows of buds, from each of which is developed a whorl of lateral branches. And the same mode of increase is seen in the "ferment-cells" of yeast, which, whilst rapidly multiplying under favorable circumstances, shoot forth little buds from one or even both extremities, from each of which a new cell is developed. There is no reason to believe that, in this process, the "nucleus" takes any direct share: since the evolution of buds may take place from cells destitute of nuclei; and even where nuclei exist in budding cells, they do not seem to be in any way connected with the process, since the buds are not observed to originate in or near them.

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* See Amer. Journ. of Med. Sciences, April, 1849; and p. 277 of the present volume.

† We have here, in fact, in their very simplest forms, the two methods of *fissiparous* and *gemmiparous* reproduction, which are carried out, not merely as regards the individual components, but in the entire fabric, of many animals, as well as of plants;—Polygastric animalcules of every kind affording us an illustration of the one, whilst not only Zoophytes, but also the inferior Mollusca and Articulata, present examples of the other.

In cases where a very rapid production of cells is requisite, it would seem to be affected by the primary separation of the endochrome into numerous parts, each of which acquires for itself a covering of cell-membrane; so that a whole brood of young cells may thus be at once generated in the cavity of the parent-cell, which subsequently bursts and sets them free. Of this plan we have the most characteristic examples in the bodies termed "zoospores," set free by many of the inferior Algæ; these were formerly regarded as the proper generative products, but are now considered simply as detached *gemmæ*. Thus, in *Achlya prolifera*, a plant composed of tubiform cells, which grows parasitically upon fish, the end of the filament dilates into a large cell, the cavity of which is cut off from the rest by the formation of a partition; and within this dilated cell, an irregular circulation of granular particles can for a time be distinguished. Very speedily, however, it appears that the endochrome is being broken up into a large number of distinct masses, which are at first in close contact with one another and with the walls of the parent-cell, but which gradually become more isolated, each seeming to acquire a proper cell-wall; they then begin to move about within the parent-cell, and, when quite mature, they are set free by the rupture of its wall, to go forth and form new attachments, and give origin to new filaments. The whole process is sometimes begun and completed in no more than an hour. —Although this method of endogenous cell-production is chiefly met with among the lower cellular plants, yet there appear to be some cases among the higher tribes, in which parent-cells give origin to a new brood in their interior, by a process somewhat similar, without the successive duplication which is certainly their more usual method of cell-multiplication. This seems the case, for example, in the "embryo-sac" of Phanerogamia; which at one time contains only a mixture of albumen and starchy matter, but which is afterwards filled up by a mass of cells that have incorporated these materials into their own substance. It is by no means impossible that the nucleus may here subdivide, and that each separate particle may constitute a centre around which a certain amount of endochrome may become aggregated. If this view be correct, the process would be strikingly analogous to one of the modes of endogenous cell-development in animals. The following, however, according to Schleiden, is the mode in which the new cells are produced within the embryo-sac: "The nitrogenous substance, or *protoplasma*, collects into a more or less perfectly spherical body, at length sharply defined, the nucleus of the cell (cytoblast); upon this is deposited a layer of protoplasm, which expands as a vesicle, and forms the subsequent lining of the cell; at a very early period, the whole becomes inclosed in a wall of cellulose, and the cell is completed." (p. 568.) Other observers, however, altogether dissent from the statement that a nucleus or "cytoblast" is first formed, and that the cell-membrane springs from it.

The first *visible* stages of the development of new cells do not always take place, however, in the interior of a pre-existing generation; for cells sometimes appear to originate *de novo* in that mixture of starchy and albuminous fluids, which, being the appropriate pabulum for vegetable cells, has been denominated *protoplasma*. Thus it is affirmed by Schleiden that "a globule of nitrogenous substance originates; in this a cavity is formed, it grows, and the complete cell has a delicate coat of cellulose, without our being able to determine the epoch of its production." (p. 568.) This he considers to occur in fluids capable of fermentation, and to be the origin of the "ferment-cells." There is no sufficient proof, however, that this "globule of nitrogenous substance" is anything else than a cell-germ, set free by the parent-cell that elaborated the protoplasma, or by some other; since the minuteness of its size, previously to the commencement of its evolution into a cell, quite sufficiently accounts for its escaping observation, excepting when the protoplasm is carefully examined with very high magnifying powers. We have been informed by Mr. Thwaites that he has frequently seen parent-cells emit cell-germs, or incipient cells, of such minuteness, as to require a power of from 700 to 1000 diameters to render them distinctly visible; and as it is obvious, from Schleiden's Appendix, 'On the Use of the Microscope in Botanical Investigations,' that he sets no value on any power higher than 200 diameters, it is not difficult to account for his persistence in his original statements as to the formation of cells from spontaneously-aggregating cytoblasts. Several parts of this Appendix contain some very curious statements, which show the author's ignorance of the present state of microscopes and of microscopic knowledge in this country. Thus, after praising the instruments of Schiek and of Plössl as the best that are made at the present time, and ranking next to these the microscopes of Pistor, Oberhauser, and Chevalier, he coolly continues:

> "The more recent English instruments seem to be so much inferior to those we have mentioned, that they bear no comparison. I likewise have not seen any of these, but there certainly is no lack of clever observers in England; and since, therefore, no microscopical botanical researches of any importance have been very recently furnished by England, excepting those of Robert Brown, and since what observations the English have made can frequently be easily refuted by a cursory glance through our glasses, this deficiency can only be attributed to the defectiveness of their instruments." (p. 577.)

His translator very properly corrects this statement, and pithily remarks that "we have plenty of microscopes, and those the best in the world; but we have had but few observers (in botanical inquiry). Our microscopes have been used rather as playthings, than as the instruments of profound physiological research." We trust that, by this time, Professor Schleiden has made himself acquainted with what has been recently done in this department of investigation by Thwaites, Henfrey, Ralfs, and others; whose researches furnish the best reply to his sarcasm, and are of merit sufficient to remove the stigma under which, with a few honorable exceptions, British botanists have long been content to lie.

Circulation of sap. We are called upon by Professor Schleiden to give up the opinions which botanists have been accustomed to entertain upon this subject, and which, he assures us, are utterly untenable and absurd. "There are," he assures us (p. 107), "absolutely no vessels for the distribution of the nutritive fluid in the body of plants; and no person would take the trouble of looking for them, or would imagine he saw them anywhere, but one who goes to the investigation of plants labouring under the false and pernicious prejudice in favour of the supposed unhappy analogy between them and animals."—The doctrine current amongst vegetable physiologists we believe to be as follows. The fluid taken up by the roots contains little solid matter, and no large proportion of carbonic acid in solution. In its ascent through the roots and stem of Exogens, it passes through the newer woody tissue and the porous vessels, without affording to them in any great degree the materials of growth. Being conducted through the leaf-stalks to the leaves, it undergoes in these organs a most important change; a large proportion of its water is exhaled, so that its solid matter is concentrated; and a considerable quantity of carbon is introduced in addition from the air, so that many new compounds are formed, and the sap acquires the power of affording nourishment to growing tissues, and of imparting the materials of the secretions to the groups of cells in various parts of the fabric whose office it is to separate them and store them up. This elaborated sap is generally believed to return from the leaves into the bark, and to descend through its inner layers; affording the materials to the cambium-layer, for the production of new woody and cellular tissue; and also, by passing inwards through the medullary rays, conveying to the older layers of wood the materials requisite for their consolidation. With respect to the mode in which the elaborated sap thus descends, there has been a difference of opinion; some maintaining that it is conveyed by distinct anastomosing vessels, termed "vessels of the latex;" whilst others regard these vessels as of special occurrence in a few plants, and consider the latex to be something different from the elaborated sap, which they believe to find its way through the cellular tissue of the bark.—In the stem of Endogens, its channel has not been so distinctly assigned; nor, if it be true that no additions take place to its lower portion, would it be necessary to suppose that a descent of sap takes place for any other purpose than the extension of the roots. Still it has been generally held that the ascent of the sap takes place through the large open vessels and woody bundles of these stems; whilst in its descent, the elaborated sap has been supposed to pass more especially through the cellular substance in which these are imbedded.

According to Professor Schleiden, however, nearly every one of these positions is incorrect: the only fact admitted by him being the movement of fluid from the roots towards the leaves as the chief evaporating surfaces. He asserts in the first place, that, save as a comparatively rare and exceptional phenomenon, the porous vessels do not convey fluid, but contain air only; secondly, that the movement of the sap is from cell to cell, thoughout the entire stem, each cell taking from it that which it needs for its own formative processes; and thirdly, that the idea of a return of elaborated sap or nutritive fluid from the leaves is altogether fallacious. He justifies the first of these statements by reference to the numerous tribes of Cryptogamia which have no trace of vessels, and to the absence of vessels in many important organs in which the formative processes are going on with great activity, as, for example, the stamens and ovules; and remarks that, as nothing is more certain than that the nutritive fluid which the single cells need must be taken up from other cells, it is superfluous to imagine another mode of transit of the sap for less frequent and less important cases. Now we cannot but be struck with the very limited comprehension of physiological truths which these arguments display. Every one admits that fluids pass with considerable readiness from cell to cell; and that this is quite sufficient in cases where there is no occasion for a very rapid movement. It was long since demonstrated by experiment, that in the Algæ every part of the surface absorbs for itself alone, there being no transmission of fluid, even by passage from cell to cell, from one part of the plant to another; how absurd, then, to hold up the absence of vessels in such plants as a proof that they cannot be subservient to the conveyance of fluids in plants, in which there *is* a constant and rapid transmission of fluids from the extremities of the roots to those of the branches. In none of the Cryptogamia, save in the Ferns, do we perceive that the absorbing and exhaling surfaces are so far separated from each other, as to indicate the need of such a provision; and in the Ferns we find vessels abundantly present in the roots, stem, and leaf-stalks. Where the motion of fluid is simply for the supply of what is needed for nutrition, as in the growing parts alluded to by Professor Schleiden, we can well understand that vessels are not required; for we have abundant examples to the same effect in the non-vascular tissues of animals. But to deny that the vessels convey blood into muscle or skin, because there are no vessels in growing cartilage or in the germinal mass, in both of which active formative processes are being carried on, would be an argument of which our readers must see the absurdity.

Nor can we attach any great weight to the direct observations to which Professor Schleiden refers, as proving his position that the vessels of plants convey air only. We have ourselves frequently dissected the vascular bundles under water, in the manner adverted to by him; and have found that whilst the true *tracheæ*, or elongated spiral cells, contain air so long as they retain their original character, the continuous *ducts* or vessels were filled with fluid, if the plant was in a state of active vegetation. Moreover, it rests on good authority, although we cannot say that we have ourselves repeated the observation, that when a vine-stem through which the sap is ascending is cut across, the fluid may be seen with a magnifying-glass to exude from the mouths of the cut vessels. The presence of vessels in nearly all cases in which a determinate current exists in a particular direction, which *we* should have regarded as a pretty clear indication that the vessels serve for the conveyance of the fluid, is considered by Professor Schleiden as a *result* of the flow, which has forced its way through the partitions of the cells, and, having thus created channels, has then abandoned them. We cannot but observe a relation so constant between the amount of leafy surface, and the rapidity of exhalation, on the one hand, and the proportion which the area of the vessels bears to that of the stem, on the other, as to induce us to be very unwilling to give up the notions hitherto entertained on this point, without some stronger disproof than Professor Schleiden has afforded. The greater the extent and activity of leaf-surface in proportion to the diameter of the stem, the greater will be the necessity for a very facile channel for the transmission of fluid from the roots; and thus it is that in the vine, the clematis, or the cane, we find the ducts so large and so freely pervious. The vessels, too, are usually larger in proportion to the closeness and density of the woody structure; thus in the Coniferæ, to which we are surprised that

Professor Schleiden did not advert in support of his argument, there are no vessels; but the woody cells are of unusual size and softness; and neither the leaf-surface nor the activity of exhalation are considerable in proportion to the diameter of the stem. On the other hand, in the oak and mahogany, whose wood-cells are very narrow and firm, and whose leaf-surface and activity of exhalation (being deciduous trees) are much greater, we find large and very continuous ducts. We by no means deny that the sap ascends through the woody as well as through the vascular tissue; but the latter, where it exists, appears to be its special channel, at any rate during the period of most active vegetation,—as to which, indeed, even Professor Schleiden is obliged to make a partial admission in favour of the vine and a few other trees, which he regards as exceptional cases.

We do not regard this first question, however, as one of equal importance to the others; since Professor Schleiden fully recognises the fact that a steady flow does exist between the roots and the leaves, regulated in amount by the exhalation from the latter; confining himself to a denial of the speciality of its channel. The point we have now to discuss is, whether the nutritive materials are drawn by the various growing tissues from the ascending sap, or whether they are derived from that which is supposed to have been elaborated in the leaves, and to have been transmitted by them, in some mode or other, to the parts where the most active formative processes are taking place. It seems to us that this question may be better decided by a reference to the great general phenomena of vegetation, than by observations or experiments directly contrived for its determination, which are open to various sources of fallacy, and may be differently interpreted according to the preconceived views of the inquirer. —There is, we believe, no difference of opinion as to the fact, that the essential nutrient materials supplied by the soil to the roots of plants are (independently of mineral matters) water, carbonic acid, and ammonia. All experiments hitherto made concur to prove, that neither the roots nor any other dark surfaces of plants can decompose carbonic acid; and we must therefore regard it as absorbed by them in a state of solution in water. Now to form gum, starch, sugar, cellulose, or any such ternary compounds, it is requisite that the carbonic acid should be decomposed, its carbon united with the oxygen and hydrogen of the water, and its oxygen set free; and all our most exact knowledge of the processes of vegetation concurs to indicate, that this cannot be accomplished except under the direct influence of light. We cannot conceive, then, how any such changes can be effected by the cells buried in the interior of the stem, as can generate these products from the materials taken in by the roots; and their presence in the ascending sap, in larger and larger amount as it rises, seems to us to be sufficiently accounted for by the opportunity which the current will have, of dissolving the substances previously contained within the tissues through which it passes.—But further, all physiologists have until recently accorded in the opinion, that the leaves or leafy surfaces are the principal channels for the introduction of carbon into the plant; deriving this, under the influence of light, from the carbonic acid of the atmosphere, and combining it with the elements of water supplied by the ascending sap, to form a far larger quantity of solid matter, than could have been generated from the materials which were taken in by the roots. And we cannot find that Professor Schleiden has furnished any adequate reasons for the abandonment of this opinion, or has given any sufficient reply to the inquiry,—what becomes of all the carbon which is separated by the leaves from the atmosphere, and which is unquestionably much more than they can appropriate to their own nutrition?

We must continue, therefore, to believe—until more positive evidence has been adduced to the contrary,—that the leaves do perform the functions which have been assigned to them; not merely getting rid of the superfluous water of the ascending sap, which would seem to be their chief office in Professor Schleiden's estimation; but also effecting such a change in that which remains, as shall render it more fit for the nutrition of the various components of the solid fabric. And it follows as a corollary from this proposition, that the materials applied by these several components to their own formative processes, must be derived chiefly, if not entirely, from the fluid which has been exposed to the sun and air in the leaves, and but little in comparison from that which is in course of ascent towards them. We may freely admit, however, that the use of the term "descending current," to express the movement of the elaborated sap, is inappropriate. It will frequently pass in a horizontal, or even in an ascending direction; and it is doubtful whether the flow can be said to take place in a stream within tubes or passages, or whether the dispersion of the liquid is not solely accomplished by transmission from cell to cell. The former opinion has been held by many physiologists, who have regarded the "latex" in the light of elaborated sap; and who have considered the system of anastomosing tubes found in the leaves and bark, and known as *cinenchyma* or "laticiferous tissue," in the light of a capillary system, destined to convey the nutritive fluid from the organs in which it is prepared to those in which it is to be appropriated; whilst the peculiar movement of fluid which may be seen in these vessels has been regarded in the light of a capillary circulation. By Professor Schleiden, however, and by several other physiologists, it has been affirmed that the presence of this latex, and of the laticiferous vessels is an exceptional fact;—that the latex (or milky juice) cannot be regarded as the same with the nutritious sap, because it contains matters which are poisonous to the plant;—that it must therefore be considered rather in the light of a special secretion, whereby these poisonous matters are withdrawn from the nutritive fluids;—and lastly, that the supposed circulation of fluid in the milk-vessels is an illusion, the movement being merely the result of the discharge of their contents by their own contraction, when the part which contains them is detached before examination. We must quote the passage with which Professor Schleiden introduces the discussion, as a choice specimen of his mode of treating the doctrines from which he feels called upon to dissent.

> "On the subject of the milk-vessels, I am rather afraid of saying too much than too little, for, owing to the total neglect of a correct scientific method, and the puerile sporting with hypotheses, without any foundation or guiding principle, the question regarding them is loaded with such a heap of nonsense, that the best way in beginning upon it is, in the first place, to throw overboard all that has been hitherto done, and commence entirely *de novo*, instead of undertaking the thankless task of cleansing this true Augean stable." (p. 114.)

Now, in the first place, it is by no means proved that the laticiferous tissue is restricted to plants with milky juices, although it has been peculiarly examined in such; on the contrary, we have seen it in plants which do not "milk;" and there is no reason, therefore, to regard their existence as an exceptional fact. So again, if the presence of a fluid contained in them be general, instead of being limited to the plants in which that fluid is milky, we have the more reason to regard it in the light of nutritive or elaborated sap. The argument derived from its containing poisonous matters seems to us a very insufficient disproof of such a doctrine; for, though these matters may be poisonous to the plant when separated and concentrated, their want of any poisonous action on the tissues which surround the thin-walled cinenchyma seems to prove that, when blended with other substances in a diluted state, they can have no such injurious character. With regard to the movement of the fluid in these vessels, it is obvious that it can only be distinguishable in cases in which the current is rendered evident by the opaquer particles which it carries along; and hence, we believe, has arisen the notion, that it is confined to plants with milky juices. But if the existence of such a current be admitted in the latter, we can scarcely deny its existence in other plants which possess laticiferous vessels, although the transparency of the contents of these vessels may prevent us from distinguishing it; and even where the fluid may be, on the whole, transparent, a floating particle here and there may indicate the course of the stream. We have distinctly seen it, for example, in a detached portion of the leaf of Vallisneria spiralis, which shows the movement of fluid within the individual cells so beautifully; and, although we admit that the injury which the leaf has received renders such an observation of little value as *establishing* the fact of a circulation of latex, yet if the fact *be* established by observations on uninjured plants with milky juices, it serves to extend it to others. We quote the following statement from Professor Balfour's treatise, as a valuable confirmation to those assertions of other observers, which Professor Schleiden, in our opinion, very unfairly calls in question; and Professor Balfour's testimony is the more important, as being given with a full knowledge of the objections which have been raised on the ground of injury to the part.

"From observations made last summer, I am disposed to agree with Schultz's statements. It is true, as Mohl remarks, that any injury done to the part examined causes peculiar oscillatory movements, which speedily cease. Thus if the young unexpanded sepal of the Celandine is removed from the plant and put under the microscope, or if the inner lining of the young stipule of Ficus elastica be treated in a similar manner, very obvious motion is seen in the granular contents of the vessels, and this motion is affected by pricking the vessels or by pressure. In order to avoid fallacy, however, I applied the microscope to the stipules of Ficus elastica, while still attached to the plant and uninjured; and I remarked that, while pressure with any blunt object on the stipule caused a marked oscillation in the vessels showing their continuity, there could, nevertheless, be observed a regular movement from the apex towards the base, independent of external influences, when the stipule was simply allowed to lie on the field of the microscope, without any pressure or injury whatever. This movement continued for at least twenty minutes during one of the experiments, and I have no doubt might have been observed longer. It is of importance to distinguish between those molecular movements which are caused by injury and pressure, and those which depend on processes going on in the interior of the living plant. My experiments are by no means complete, but they lead at present to the adoption of Schultz's opinion relative to the existence of cyclosis." (p. 128.)

We are far from regarding the question, whether or not this movement of the latex is equivalent to a circulation or diffusion of elaborated sap by distinct vessels, as satisfactorily determined; but we think that there is at any rate sufficient evidence in its favour, to induce us to look with some suspicion at the dogmatic denial of Professor Schleiden; still more, to prevent us from admitting the justice of his assertions regarding the folly and ignorance of those who hold the affirmative doctrine. We no more regard these vessels as *necessary* to the diffusion of the elaborated sap from the leaves, than we consider the straight porous tubes as *necessary* for the ascent of the crude fluid; but when they are present, we are inclined to regard them as the principal agents in this function, performing more rapidly and directly that which would otherwise be more gradually and indirectly effected by the transmission from one cell to another. That the latter, however, may be, and frequently is, the only mode of diffusion, we have little doubt; and what we chiefly contend for, is the fact of such diffusion of nutritive fluid from the leaves, taking place in Exogens through the bark and medullary rays, and in Endogens probably through the whole cellular matrix of the stem. And, in addition to the evidence which we have already adduced in favour of this generally-received doctrine, we may notice the following facts, which are all strong indications, in our apprehension, of its truth.—In the first place, the peculiar principles which the plant may generate are seldom found in the ascending sap, in any larger amount than would be accounted for on the idea that they have been dissolved during its upward flow; and, if these principles have not been deposited in the wood, we believe that they are never found in the fluid of the ascending current. On the other hand, the leaves and the bark almost always contain these principles or others nearly allied to them, although frequently in a state of less concentration than that in which they may be found elsewhere; as if the leaves were the laboratory in which they are formed, and the bark the channel through which they are conveyed to the parts where they are to be separated and stored up.—Secondly, the history of parasitic plants appears decidedly favorable to the same view. For the parasites possessing green leaves (belonging, for the most part, to the order Loranthaceæ) connect themselves especially with the *woody* structure, with which their own becomes connected as by a natural graft; and seem to grow indifferently on a great variety of trees, whose special or peculiar products are very unlike; as if they drew their nutriment from the crude or ascending current, which is nearly the same in most trees, and elaborated it for themselves. On the other hand, the leafless parasites attach themselves, commonly by suckers, to the bark of the plants they infest, to which they are obviously very injurious; and they are much more restricted as to the species from which they derive their nutriment; as if they are dependent upon elaborated sap of a particular quality.—Thirdly (but to this we attach less importance than we do to the experiments ready prepared for us by nature), it has been found that, when plants have been made to absorb mineral matters, which can be easily detected by chemical tests, these first make their appearance in the wood, then in the leaves, and lastly in the bark.

On the whole, then, we see no adequate reason for calling in question the usually-received opinion, that the elaborated sap is diffused from the leaves, where it is prepared, to the several parts of the plant which are to appropriate it; and we think it probable that this diffusion takes place more particularly through the cellular tissue of the leaf-stalks, which is directly continuous with that of the bark; whilst the fibro-vascular bundles which occupy the centre, are the channels of the ascending sap, transmitted to them from the wood for elaboration. +

True generative process. The tendency of late researches on this subject has been to demonstrate the universality of the principle for which we contended on a former occasion (Vol. II, p. 371), with regard to the concurrence of two sets of organs,—equivalent on the one hand to the conjugating cells of the inferior Algæ, and on the other to the pollen-grain and ovule of Phanerogamia,—in every true "act of generation." For the remarkable discovery which has been very recently made by Count Suminski, in regard to the generation of Ferns, affords the key to the facts which had been previously in course of progressive accumulation, in regard to the existence, in the various tribes of Cryptogamia, of *antheridia*, containing bodies analogous to the *spermatozoa** of animals, which had been surmised by many botanists to be male organs, but without any proof. The very idea of any such function, indeed, being totally opposed to Schleiden's doctrine of vegetable generation, he is unsparing of his ridicule of those who have entertained it; not hesitating, for example, to affirm that "it appears fully certain that neither in the history of their development, nor in their structure, nor in their physiological relation, do they show the most remote analogy with the anthers of the Phanerogamia; and, consequently, that the application of this designation, together with all the visionary matter (the pretended theories) based upon it, are altogether devoid of all foundation, and therefore do not belong to science." (p. 185.) Now unless Professor Schleiden had positive proof that these curious organs perform some other function, and that they do not take any part whatever in the generative process, he most certainly had no right to speak in this tone of the surmises of botanists of equal ability with himself, supported as they were by many very remarkable analogies, although not entitled to take rank as demonstrated facts. And the consequence is, that having committed himself by this violent declaration, he may find himself compelled either to make a very humiliating retractation, or to uphold his own doctrine in spite of evidence to the contrary, sufficient, we feel assured, to make an impression on any unprejudiced observer. We do not feel much doubt as to which course he will adopt; and venture to prophesy that, in a few years' time, he will find himself standing alone amongst scientific botanists, in his assertion of the non-sexuality of the Cryptogamia, of the utter meaninglessness of the "spiral filaments" (as he terms the spermatozoa), and of the entire absence of analogy between the reputed "sexual" organs of flowering plants, and the true sexual organs of animals.

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* We use this term simply to express the exact similarity of these bodies to the corresponding bodies in the seminal fluid of animals, but without any idea of imputing to them, in *either* case, the attribute of distinct *animality*.

We shall now describe the results of Count Suminski's researches, and point out their bearing on the general doctrines of vegetable generation.—It has long been well known that the first product of the spore of the Fern is a flattened leafy expansion, spreading itself over the surface on which it grows, and imbibing its nourishment through radical filaments developed from its under surface. This "primary frond," as it was termed, seemed to perform the functions of a cotyledon; obtaining and preparing nutriment for the production of the true leaves, stem, and roots, which seemed to shoot forth from near its centre; and decaying away, when these more permanent parts were fully evolved. In 1846 the discovery was made by Nægeli that this "primary frond" contains *antheridia*, that is, collections of cells containing moving filaments, analogous to those which had been previously observed in Mosses, Hepaticæ, Characeæ, and Fucaceæ. Their existence, however, in such a situation could not be accounted for, and seemed to render the relation of these bodies to the generative process yet more improbable than before; and alluding to the search which had been previously made for antheridia in the adult Fern, Schleiden remarks in his usual style, when speaking of any who do not implicitly adopt his opinions—

> "For my part, I am surprised that no one has as yet insisted on the presence of the organs of sense, as eyes and ears, in plants, since they are possessed by animals; such an assumption would not be a whit more absurd than the mania of insisting upon having anthers in the Cryptogamia, simply because they are found in the Phanerogamia." (p. 196.)

He has the grace, however, to acknowledge the correctness of Nægeli's observation, though without attempting to solve the problem of the purpose of these curious bodies.

The conformity between the history of the development of the animal spermatozoa, as described by Kölliker, and that of the spermatozoa in the primary frond of the Fern, as described by Nægeli and Suminski, is so extraordinary, that the representations given by the one would equally serve as delineations of the other. In the interior of a prominence that forms on certain of the large cells of which the primary frond of the Fern is composed, a new cell of peculiar aspect is seen, which is distinguished by its not containing chlorophyll, but a collection of granular particles, which after a time present somewhat of a definite arrangement; this cell enlarges, and its contents are gradually developed into a collection of secondary cells, which completely occupy its cavity, and are adpressed against each other. Within every one of these secondary cells is developed a single spermatozoon, having an ovoidal body, which possesses (according to Count Suminski) three cilia on each side, whereby its movements are effected, and a filiform tail, which dilates into a smaller ovoidal body at its extremity. The parent-cell ruptures and sets free the contained sperm-cells; and the spermatozoa, which had previously lain coiled up in their interior, at last make their escape, and begin to move actively over the surface of the primary frond, through the layer of mucilaginous fluid which has been ejected with them, and the moisture with which the inferior surface of the frond is always bedewed.

But besides the *antheridia*, or parent-cells of these spermatozoa, which are usually very numerous, the "primary frond" of the Fern contains a small number of other bodies, which were remarked by Nægeli, who considered them as antheridia in a different stage of development; but whose true nature has been detected by the acuteness and industry of Count Suminski. Of these bodies, which may be termed *pistillidia*, every one contains a single "germ-cell," which appears to be analogous to the "embryo-sac" of the ovule of Phanerogamia, but which lies naked at the bottom of the cavity of the pistillidium, instead of being surrounded (as in them) by the cellular parenchyma of the "nucleus." It is thus exposed to immediate contact with the spermatozoon, which finds its way down through the chimney-like passage of the pistillidium, and applies the dilated extremity of its filiform tail against the germ-cell; at the same time itself undergoing a considerable increase of size, apparently by the absorption of the fluid through which it has moved. Two, three, or more spermatozoa have been usually seen by Count Suminski in each pistillidium; as if to ensure the fertilization of the germ-cell. Soon after this contact has taken place, a new cell is seen within the embryo-sac, which appears to originate in the union of the contents of the "sperm-cell" and of the "germ-cell," being thus the result of a true "act of generation" taking place in the "primary frond" of the Fern; and it is from this parent-cell, as Count Suminski has beautifully shown, that the young Fern originates, by the process of subdivision and multiplication, just as the embryo of the flowering plant originates from its "embryonic vesicle." But there is this remarkable difference between them; that whereas the latter is nourished by the store of food previously laid up in the seed, the former continues to imbibe its nutriment directly from the leaf-life expansion from the midst of which it was produced; so that, physiologically speaking, the primary frond still acts as a cotyledon to the growing embryo, although its homological resemblance would rather be to the receptacle of a head of flowers.

The young Fern, thus assisted in the early stages of its development, gradually puts forth its leaves and roots; and finally evolves what has been hitherto considered as its fructification. The *spores* of the Fern, however, must now be regarded, like the "zoospores" of the Algæ which are not the product of conjugation, in the light of detached gemmæ or buds; their peculiarity being that they are "flower-buds" rather than "leaf-buds," having for their office to develope the true apparatus of fructification, in which we have male and female organs so distinctly present, that all question of the analogy of the animal and vegetable sexes seems to be now at an end. We may state, in confirmation of Count Suminski's statements, that we have ourselves been enabled, through his kindness, to verify some of the most important parts of his observations, in company with two of the most eminent botanical observers of our time; and that he clearly demonstrated to us the existence of the pistillidia, and the origin of the embryo-fern from the bodies they contain. The actual contact of the spermatozoon with the embryo-sac is, of course, not likely to be seen, except after long and careful watching; but Professors Ehrenberg and Müller have satisfied themselves of the truth of the Count's statements on this point; and we must say for ourselves, that we found his delineations of what he *was* able to show us, to be so entirely borne out by the actual specimens, that we could scarcely refuse to credit the statements of so accurate an observer as to the rest. We may remark, however, that a magnifying power of 300 or 400 diameters, and some experience in the interpretation of microscopic appearances, are requisite in the verification of these remarkable observations.—Since writing the preceding account of them, we have perused a Memoir by M. Albert Wigand (Ann. des Sci. Nat., Mars, 1849), who, while confirming the statements of Count Suminski in all essential particulars relative to the structure and contents of the antheridia and pistillidia, does not hesitate to deny the penetration of the spermatozoa into the pistillidium, simply because he has not witnessed it, and thinks it *à priori* improbable. It is evident to us from his drawings, that this author has not employed sufficiently high magnifying powers or an adequate illumination.

It can scarcely be doubted that wherever cells developing spermatozoa are present in cryptogamic plants, they are to be regarded as the *male* portion of the reproductive apparatus, to which an equivalent *female* portion must somewhere exist; for now that the true function of these curious bodies has been demonstrated, their *solitary* existence would be as meaningless, and we may say as impossible, as their presence in any tribe of animals without corresponding female organs. Hence as antheridia have been discovered in a large portion of the Cryptogamia, we can scarcely doubt that the real "act of generation" takes place in them all; since, as we formerly pointed out, the conjugation of cells in the simplest Algæ, and the fertilization of the ovule in the Phanerogamia, are phenomena so essentially analogous, that a process of the same character might be expected to take place in all the intermediate groups. That neither antheridia nor pistillidia have yet been found in the Fungi, nor with absolute certainty in Lichens, cannot be accepted as a valid objection to this view; since it is obvious from the history of the process in Ferns, that we must scrutinize the entire structure of these tribes, in every stage of their evolution, before we can be entitled to declare the absence of sexual organs,—which no one would have even dreamed of finding in the "primary frond" of Ferns.

The fructification of Mosses consists of thecæ or urns containing spores, which are known in their early state as pistillidia; the antheridia have long been recognised in this tribe, whose sexuality has hence been suspected by many bryologists. The careful examination of the structure and development of Mosses, however, instituted some years ago by Mr. Valentine, led him to the conclusion that the antheridia could have no share in the reproductive function, seeing that *their* period of maturity is long anterior to that of the theca and spores. But it was suggested by Mr. Thwaites not long since, that the process of fertilization might take place in the young pistillidium, and that the entire theca with its contained spores might be its product; and the discovery of Count Suminski renders this supposition highly probable. For if in the Ferns the fertilizing process is antedated (so to speak), so that not only the thecæ, but the plant which bears them, should be the product of an act of generation in the "primary frond," it cannot be regarded as improbable, that in the Mosses it should be so far antedated that the thecæ alone should be thus produced. The probability appears still stronger when we descend to the Hepaticæ, in many of which the resemblance of the whole plant to the "primary frond" of the Fern is so strong, that we have heard the latter designated as the "Marchantia-like expansion." In these plants, antheridia as well as pistillidia have long been known; and it can scarcely be doubted that, as in the "primary frond" of the Fern, a true act of generation is accomplished between their products, the germ-cells of the latter being fertilized from the germ-cells of the former; the result being the development, not (as in the Ferns) of a plant with a woody stem, true roots, leaves, and thecæ or spore-cases, but simply of the thecæ themselves. Thus, the "spores" of the Mosses and Hepaticæ must be regarded, like those of the Ferns, in the light of *gemmæ*, destined to evolve the organs by which the true generative process is effected. It may seem strange to affirm that the theca is not a part of the plant which bears it, but is really its offspring; but this is not more strange than that the Fern should be the true offspring of its primary frond, instead of being evolved from it, as hitherto supposed, by a process of continued development.

The early history of the development of the Fungi has not yet been studied sufficiently, to enable us to give even a probable guess as to the period at which the true generative process takes place. This much, however, may be remarked; that as mycologists have for some time been coming to regard the *mycelium*, or filamentous expansion from which the fructification is developed, in the light of the true plant, and to look upon the reproductive body which constitutes what is popularly considered the fungus as really a subordinate part of it, we may expect to find the "act of generation" performed in the mycelium, perhaps by a conjugation of the filaments, after the fashion of that of the inferior Algæ; and this supposition derives support from the fact, that the reproductive powers of the mycelium, which forms the "spawn" of mushrooms, are not inferior to those of the fructification. The sporules of Fungi, if this view be correct, must be considered as gemmæ, like those of the higher Cryptogamia.

With regard to Lichens, we shall at present confine ourselves to the remark, that their near alliance, on the one hand to Algæ, and on the other to the Hepaticæ, leaves little room to doubt that what is true of those two groups must be true also of them. It has been stated by Link that he has found antheridia in Lichens; but we do not know that he has discovered spermatozoa in these.

In the class of Algæ we find the generative process performed under two forms which are apparently very distinct, yet which are seen on a closer examination to be but variations of the same fundamental type. We have already alluded to the "conjugation" which has been observed in the simplest cellular tribes, and in some of the filamentous, as being an "act of generation" in its most elementary form. In the Fucaceæ, on the other hand, which rank among the highest of the Algæ, and in the Characeæ, which are now usually referred to the same general division, we find distinct "sperm-cells," which have long been known in the Characeæ, but the discovery of which in the Fucaceæ is comparatively recent. In the latter tribe, the "sperm-cells" are sometimes developed in the same conceptacles with the "germ-cells," sometimes in distinct conceptacles on the same plant; whilst sometimes (as in the common Fucus vesiculosus) the two sets of conceptacles are seated on different individuals. The number of "sperm-cells" is beyond all comparison greater than that of "germ-cells;" and this we find to be the general rule, in plants as well as in animals, as if to secure the fertilization of the latter, in spite of the accidents that may prevent the spermatozoa from meeting with them. The true "spores" of the Algæ are really the "germ-cells," which seem to be commonly fertilized, like the ova of fishes, after they have been set free from their conceptacles. But almost all Algæ propagate also by detached gemmæ, which often constitute what is known as a second form of fructification, and which, when they are carried about by ciliary action, are known as "zoospores;" and this is the only method of propagation at present known to occur in the Ulvæ and some other tribes. There can be little doubt, however, from the analogy of the tribes to which these are most closely allied, that an act of conjugation or impregnation takes place at some period or other of their development.

We may sum up our present knowledge of the true "act of generation" in plants as follows:—Its essential character everywhere consists in the union or mixture of the contents of two cells, the "sperm-cells" and the "germ-cells;" but this may take place after three modes. 1. In the simplest cellular plants, where every cell appears to possess the same endowments, so that there is no kind of specialization of function, the generative act consists in the "conjugation" of two cells, between which no difference whatever can be traced. In what may be considered the lowest forms of this process, *both* cells discharge their contents, and the new body, or sporangium, is formed between them by the mixture of their endochromes; so that neither cell of the pair can be considered in the light of a "sperm-cell" or a "germ-cell." We have here the *exact converse* of the process of multiplication by fission, already described; for as, in the latter, one cell divides itself into two equal halves, between which no difference can be traced, so in the act of conjugation, two cells, apparently similar, reunite, and form but a single new cell between them. That union, however, seems to develope a new force, in virtue of which the process of subdivision may continue until that force be expended. In the higher forms of this process there is still the same mixture of endochromes by the actual rupture of the cell-walls, and the escape of their contents; but this mixture takes place, and the sporangium is formed (as in many of the Desmideæ and Zygnemata), within one of the conjugating cells, which must thus be regarded as the "germ-cell." We have here an indication of the specialization that is apparent in those more complex fabrics, in which certain cells are set apart for the performance of the act of generation, and in which the "sperm-cells" and "germ-cells" are quite distinct in their endowments. 2. In the higher Algæ, and in most, if not all, other Cryptogamia, the process is effected by the agency of moving filaments, resembling the spermatozoa of animals, which bring the essential part of the contents of the "sperm-cells" into such a relation with the "germ-cells," that a transudation may take place from the former to the latter, without any rupture of the walls of the latter. Of this process, the true germ is the result. This germ may be cast at once upon its own resources, as in the Algæ; or it may be nourished by its parent structure, as in the Ferns. It may at once proceed to the development of the new plant, similar to that from which it sprang, as it seems to do in the Algæ; or it may develope itself into a body containing a mass of gemmæ, each of which may produce a new plant, as in the Hepaticæ and Mosses; or it may spring from the temporary portion of the plant, and evolve the whole permanent fabric, as in Ferns, the latter producing gemmæ from which a new generative apparatus arises.* In none of these cases does the parent appear to furnish that accumulation of nutritive matter for the development of the product of the germ-cell, which, when included in a common envelope with it, would constitute a true "seed." 3. In the Phanerogamia, the "sperm-cell," or pollen-grain, does not evolve self-moving filaments, as in the Cryptogamia, but puts forth long tubes, which convey its fertilizing contents to the "germ-cell" after a different fashion, still, however, fulfilling the same essential purpose. We adverted, on a former occasion, to the concurrence of nearly all the most recent and trustworthy observers, in the denial of Schleiden's assertion that the embryo is formed within the extremity of the pollen-tube; the fact appearing to be rather, that the embryonic vesicle is formed within the embryo-sac without any actual penetration of the pollen-tube, and that the mixture of the contents of the "sperm-cell" and "germ-cell" is accomplished by transudation through the membrane of the pollen-tube and that of the embryonic vesicle. The germ-cell is here imbedded in a mass of nutritious matter, which, with the embryo, constitutes the "seed;" and it is upon this store that the young plant subsists during the early stages of its development. The first leafy parts which are put forth,—the cotyledons,—have a merely temporary function, being evidently destined to obtain and elaborate the materials required for the evolution of the more permanent organs; and we might regard the subsequently developed parts as originating from them by gemmation.†

We have thus endeavoured, so far as our limited space would allow, to place before our readers some of the most interesting results of those recent investigations in physiological botany, which bear most closely upon animal physiology; and we cannot but think that it must be as apparent to them as it is to ourselves, that these two departments of science are so closely related, that they will be most successfully pursued in concert, and with a mutual good understanding between their respective votaries. We feel ourselves called upon to protest most strongly against the denunciations which Professor Schleiden has thought it proper to utter against all those, who labour at drawing closer the bonds of union between these two great kingdoms of organized nature. We are no friends to the vague and superficial analogies, which men, who had no true insight into their relation, have amused themselves with drawing; but we do contend that real and fundamental analogies exist, which are clearly to be recognised by those who possess a sufficiently comprehensive knowledge of the phenomena, and a sufficient amount of philosophic sagacity, to be able to distinguish the essential from the accidental, the real from the formal. The simple fact that the animal, like the plant, is evolved from a single cell, and that even in the most perfect condition of each, all its functions are but manifestations of different kinds of cell-life, should at once indicate that the most comprehensive principles of vital action must be common to both, and that the differences in their structure must have relation to the difference of the conditions under which they are to exist, and the difference of the results they are to evolve. The "idea" of a plant is that of a being possessed of the power of combining certain inorganic elements into organic compounds, and of building up, at the expense of these, an organized fabric, which shall afford materials for the nutrition of animals; but this being is not endowed with sentient attributes or locomotive powers; it is usually fixed to one spot from the time its germ first begins to develope itself, and its whole life consists in formation and reproduction. On the other hand, the "idea" of an animal is that of a being possessed of sentient existence, but that existence is (in our present state at least) dependent upon, or inseparably connected with, an organized structure: powers of formation there must be, as in plants; but these are exercised upon organic compounds, supplied directly or indirectly by the vegetable kingdom; and the animal must be endowed with locomotive faculties to go in search of these, or with some means of bringing them within its reach: and the whole apparatus of nutrition seems destined simply to form and to maintain that of animal life, by which the conscious existence is brought into relation with the world around, so as to receive impressions from it, and in its turn to act upon it; whilst that of reproduction secures its renewal. Considering that the plant and the animal have the same starting-point,—the single cell,—we might naturally expect that the "act of generation," of which that cell is the result, should be the one whose performance in the two kingdoms might display the greatest resemblance; and we think that no one who appreciates the details we have given can fail to perceive how remarkably this is the case. It is curious, however, to observe, that in *no single* tribe of plants is the reproductive process performed *on both sides* after the pattern of that of animals; for whilst in the Cryptogamia the "sperm-cells" bear so strong a resemblance to those of animals, that they might be mistaken for them, the "germ-cell" is naked, instead of being inclosed within a yolk and yolk-bag; whilst in the Phanerogamia we find ovules resembling the ova of animals in all essential particulars, but fertilized by a different development of the sperm-cell.

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* This last would be reckoned by the disciples of Steenstrup as a case of "alternation of generations;" the true Fern, originating from the fertilised germ, being one generation; and the primary frond, originating from a gemma, being in their apprehension a second generation. We shall return hereafter (Art. VIII) to the discussion whether the term "generation" can be rightly applied in this or any similar case of gemmiparous reproduction, which comes more properly, in our apprehension, under the term "development;" and shall at present only remark, that the case fully confirms our general doctrine (Vol. I, p. 205), that in every instance of such alternation, it exists really between the products of sexual and non-sexual reproduction, and that it never occurs between the products of the true generative process, which invariably repeat the same type.

† Professor E. Forbes carries his idea of the succession of generations in flowering-plants to such an extent, as to regard the cotyledons as forming one generation, the first true leaf a second generation, and so on. According to this doctrine, the germinal membrane of the animal embryo, which is the distinct analogue of the cotyledons, and the chick which is developed through its means, constitute two distinct generations, instead of being the successive phases of the evolution of one and the same individual.

Notwithstanding that we have felt called upon, in our discussion of every one of the subjects upon which we have touched, to express our dissent from Professor Schleiden's views, and to condemn the spirit and temper in which he has put them forth, we should not be doing justice to his labours if we did not also repeat our statement, that by his elucidation of a vast number of other topics, which it does not fall within our province to discuss, he has contributed most successfully as well as diligently to the advancement of botanical science. We regard the plan of his work, and the method of investigation on which it is based, as far superior to that of any other systematic treatise on the subject; and no one can rightly comprehend the present aspect of Botanical science, without a careful study of it.

ART. III.

De l'Infection Purulente, ou Pyoèmie. Par le Docteur C. SEDILLOT, Chirurgien principal des Armées, Membre correspondant de l'Institut de France, &c. &c. Avec Trois Planches coloriées.—*Paris,* 1849. 8vo, pp. 520.

On Purulent Infection, or Pyæmia. By Dr. C. SEDILLOT.

"PURULENT Infection," or, as it perhaps might be better rendered in our vernacular, "Poisoning by Pus," is the term adopted to indicate a train of symptoms which seem to be of much commoner occurrence in the Continental hospitals than in our own, or at all events to have attracted more attention there than here, having been somewhat vaguely set down among us as the general phenomena attendant upon some forms of phlebitis. It is true that some of our writers have suggested that the highly irritative character of the symptoms is probably due to the presence of pus in the blood, and that even the capillary phlebitis to which they have in some cases been referred, may be sometimes itself produced by a preliminary contamination of the blood by pus, although, in other instances, it is itself the starting point, and in either case keeps up a continued supply of the toxical agent. M. Sedillot lays down the proposition absolutely, that the symptoms described under the various names of purulent infection, purulent absorption, purulent diathesis, suppurative phlebitis, &c., are always produced by the admission of the pus-globule into the blood. That such entry of pus into the circulation is the usual cause of these, although not operating as M. Sedillot represents, may be admitted; but that it alone can give rise to them, we do not believe: for not only is such admission, under some of the circumstances mentioned by the author, somewhat problematical, and its exclusiveness waived by him in favour of putrid matters; but assemblages of symptoms having the greatest analogy to those described under the term purulent infection, are met with in cases wherein pus has had nothing to do with their production. Thus, for example, typhus fever, some forms of puerperal fever, erysipelas, &c., attended with the severest constitutional irritation, may be generated in the wards of hospitals under the influence of over-crowding and consequent vitiation of the air, the *encombrement* of the French, the *ochlesis* of Dr. Gregory. There can be little doubt that in these cases a subtile poisonous matter enters and ferments in the blood; and such explanation will apply to very many of the cases set down by Mr. Travers and his followers as originating in a primary affection of the nervous system. Farther, a practitioner will go to puerperal patients, one after another, all perhaps favorably situated as regards hygienic circumstances, conveying to them at his fingers' ends, or even in his habiliments, a poison far more subtile than the pus-globule, and yet capable of inducing a train of symptoms just as frightful as those set forth by our author. It is quite recently that a remarkable mortality amongst the puerperal women at the Vienna Hospital was traced to the fact of their being attended by students in the intervals of their dissections, the deaths being far less frequent among those attended by the midwives. The publication of this fact forcibly called to our mind the large proportion of puerperal fevers we met with during our pupillage, compared to the experience of after life. But we are here perhaps combating a shadow; for the author in point of fact does not regard pus as a poison, but as a local irritant. Admitted into the blood, he says, it exerts no direct action on that fluid whatever, and only becomes mischievous when it becomes detained in the capillary vessels of the lungs or other organs, exciting the irritative symptoms characteristic of the affection, in consequence of the impediment it offers to the due performance of their functions, and to the attempts made at its elimination. We may reserve any remarks we may have to make upon this doctrine until we have allowed him to state his views more fully; and as the detection of the pus-globule in the blood is considered by him to be highly important in prognosis, as evidencing the formation of secondary abscesses, we may first refer to the conclusions upon this point to which he has been led by attentive and prolonged examinations, made under a great variety of circumstances. According to these, the pus-globules are two or three times larger than those of the blood, their contour is more regular, their granular or nipple-like projections are more distinct, their transparency is less, their specific gravity is greater, and the tendency they have to adhere to bodies prevents their being carried along in the current as are the blood-globules. The *white globules* of the blood, however, have a certain resemblance to those of pus, but exist in far fewer numbers, and are detected with much greater difficulty. In all doubtful cases the author has, as a means of comparison, made contemporary examinations of healthy blood and of blood to which pus has been added. M. Lebert has furnished him with a note upon these distinctions, in which he observes that, however easy it may be to confound the two by a superficial examination with a low power, the differences become obvious if a power of from 500 to 800 diameters be employed.

> "The dimensions are different, that of the pus-globule varying from $\frac{1}{100}$ to $\frac{1}{80}$ millimeter, that of the white globule from $\frac{1}{120}$ to $\frac{1}{100}$, a sixth or a fifth smaller. The colour of the pus-globule is slightly yellowish, that of the white globule a dead white, without any trace of yellow. The form of the pus-globule is spherical, while that of the white one is much more flattened, being at most lenticular, and never spherical. The surface of the pus-globule is roughened compared with the smooth surface of the white globule. A marked difference is generally seen in the nuclei as exhibited in both by means of acetic acid. Those of the pus-globules are much larger, varying from $\frac{1}{300}$ to $\frac{1}{200}$ millimeter, and exhibit in their interior a regular well defined cavity, while the white globule furnishes scarcely other than very small puniliform nuclei of from $\frac{1}{500}$ to $\frac{1}{400}$ millimeter, rather resembling nucleoli or fatty granules." (p. 269.)

In regard to any effect produced upon the appearance of the blood-globule by the introduction of pus, Dr. Sedillot is very sceptical; and he entirely derides the idea of the conversion of the one into the other, having always found the two remain distinct after commingling them experimentally. A multitude of examinations have proved to him that changes in the shape and appearance of the blood-globule are quite as frequent in healthy blood as in blood containing pus; while in cases in which the circulation has abounded in pus-globules, the shape of the blood-globule has been found quite normal. Some of the supposed changes are due to the imperfect manner in which the microscopic observations have been conducted.

Some of the discrepancies of authors in respect to the detection of pus in the blood are explained by the circumstances under which the search for it has been undertaken. It is not always to be found even when purposely introduced by injections.

> "In almost all our experiments we have examined the blood by the aid of the microscope. Generally we have entirely failed in detecting the pus-globule after a *single* injection of pus; for the instances in which we or our assistants believed we had recognised such in the left cavities of the heart, were very inconclusive. The same remark applies in all cases in which the secondary abscesses have only commenced forming in the lungs, the stage of complete softening not yet being attained. But in those cases in which large and numerous collections of pus existed, we have always found pus in the blood, at whatever part of the system this has been taken from...........In cases of pyæmia in which the metastatic abscesses were not numerous and matured, we met with no traces of pus in the veins which did not communicate directly with the vessels affected with suppuration. The same remark applies to our experiments on living animals. Whatever quantity of pus was injected into the jugular veins, we detected no traces of it in the femorals. We were obliged to admit, therefore, that the globules of the fluid had become arrested in the parenchyma of organs, and we found them in the tissue of the lungs. This was no longer the case when large metastatic collections had formed, for then the blood of every vessel of the body contained pus. It is on these facts that our diagnosis was based in this case, and we have related another in which we were also enabled to announce during life, from a microscopic inspection of the blood, that the anatomico-pathologic changes of pyæmia, with a large collection of matter, would be found at the post-mortem,—an opinion, the exactitude of which was confirmed." (p. 270.)

M. Sedillot prefaces his work with an interesting summary of the opinions which have heretofore prevailed upon the subject. Boerhaave (Aph. 406) and his commentator, Van Swieten, recognised the injurious effects due to the admission of pus into the blood. Morgagni, after quoting a great number of instances of wounds of the head followed by visceral abscesses, opposes the idea of a mechanical transport of the pus thither, and points out the fact, which has required reassertion in our day, that such abscesses are not confined to the liver, and that they may follow wounds and ulcers of other parts besides the head. He attributes their occurrence to particles of pus, which, having become mixed with the blood and disseminated, "are arrested in some of the narrow passages, and, by obstructing and irritating these, offer an obstacle to the circulation of the humours, and become the cause of the production of a much larger quantity of pus than that which was primarily introduced—a suppuration indicated by shivering and trembling." It is to J. Hunter (1784) we owe the first correct account of the influence of phlebitis; and M. Sedillot attributes to his want of erudition his not generalising the observations derivable from this newly-discovered source of pus with those which had already been made upon the influence of this fluid in the production of consecutive abscess. Hodgson is the first author who (1815) suggested the connexion of the two classes of phenomena. Ribes pointed out (1816) the agency of phlebitis in the production of the fatal train of symptoms in puerperal women; Velpeau put forth various untenable hypotheses of the metastasis or transport of pus, and the conversion of the metastatic abscesses into scirrhous or tubercular masses. Rose and Arnott, in England, put observers on the right track of inquiry, but we can scarcely say that their conclusions had gone beyond those of Boerhaave and Morgagni, the introduction of pus into the blood being rendered highly probable without being substantively demonstrated. These and similar ideas were regarded at the time but as ingenious hypotheses. By the researches of Dance a great impulse was given, and what had been hypothesis now became demonstration, so that henceforth phlebitis and purulent infection became synonymous terms; and to such an extent was this the case that the formation of secondary abscesses began to be attributed, not to the agency of pus, but to that of capillary phlebitis. Marechal, writing at the same period as Dance (1828), furnishes a most accurate account of the process of formation of metastatic abscesses, due, according to his views, to the absorption of pus by the veins. Cruveilhier, admitting the doctrine of the formation of secondary abscess being due to capillary phlebitis, further laid down as an axiom, since proved untenable, (see Brit. and For. Med.-Chir. Rev., vol. II, p. 386) that any foreign body introduced into the veins, and whose elimination by the emunctories is impossible, will produce exactly similar visceral abscesses by the agency of the capillary phlebitis they give rise to. From this time forward a great number of observers added to our stores of knowledge upon the subject, the invaluable aid of the microscope being called into frequent requisition. Among the most determined opponents to the reception of the new doctrines was Dupuytren, "the most prominent character of whose mind was his power of procuring the triumph of error in spite of evidence, and his desire of confiscating the interests of his art to his own profit," but his opposition was based upon too flimsy grounds to be of long avail. Tessier, who has long defended the ancient doctrine of a purulent diathesis, has endeavoured to show, that even in suppurative phlebitis, pus never mingles with the blood.

> "We have thus seen the doctrines of Boerhaave, &c., prevail for a while, and then, neglected by the *Académie de Chirurgie,* continued only in occasional works, to fall into entire oblivion during the prevalence of an exclusive solidism. The spirit of observation then awaking with its conscientious imperviability, tore away the veil stretched by prevailing doctrines over a portion of pathology. The effects of the mixture of pus with the blood were studied transitively under the name of phlebitis, for which that of purulent infection and pyæmia was soon substituted. But once arrived at the apparent conditions of this affection, and of the material disorders it gives rise to, observers abandoned themselves to the most contradictory and confused explanations, invoking all kinds of hypotheses without arriving at any certainty, until at last they seemed ready to abandon researches so surrounded by obscurities and obstacles.
>
> "Is pyæmia the result of a diathesis, or is it caused by the introduction of pus into the blood? What is the mechanism of such introduction? Does the pus act as pus, or by means of the products it gives rise to? If it acts as pus, to which of its elements are we to attribute the poisonous properties it seems endowed with? Is it the globules, the granules, or the serosity, separated or combined, altered or unaltered in properties, that give rise to the pyæmic changes, and to death? Is the cure possible; and what are the true indications of treatment?" (p. 70.)

The object of the present work is to furnish a demonstrative answer to the above questions; and this is effected by the minute detail of forty-five experiments performed on dogs, and the narration of a large number of cases of the disease met with in the author's wards in the Strasburg hospitals. Having so recently noticed MM. Castelnau and Ducrest's researches upon the production of multiple abscesses by the introduction of pus into the blood, the first that imitated the workings of Nature in disease by the repeated injections of the fluid, we may pass over those of M. Sedillot, which only confirm the conclusions of these authors, and advert only to some additional points he has brought forward prominently.

After a *single injection* of a moderate quantity (4 grammes) of healthy pus into the veins, the animal recovered, having sustained little inconvenience. So, too, recovery usually took place after the injection of larger quantities (15 to 20 grammes) even when this was of a thickened consistency, containing many globules; the symptoms in these cases being, however, much more severe. When the animal did die from the effects of a single injection, the metastatic abscesses were found to be only in early stages of formation. Even after an injection of fetid pus a recovery takes place, the fatal effects of this fluid evidently depending upon other causes than its fetidity. Yet in other cases a smaller quantity of fetid than of healthy pus proved fatal, and a small quantity of gangrenous pus did so speedily. The injection of diluted chyme produces very analogous symptoms to those proceeding from pus, and, in equal weights, perhaps even more serious ones. Injections of pus were made by the mesenteric veins, in order to discover whether, by influencing the liver more directly than by injections of the jugular, it rendered that organ proportionally more frequently the seat of abscess as compared with the lungs. It did not so.

"There can be no doubt but that there exist special causes for the development of metastatic abscesses in the lungs rather than the liver. For twenty cases of suppuration in the lungs, we scarcely find one of the liver; and, to the present time, no one has indicated the cause of the difference. Is it that the capillaries of the liver are more easily traversed by the pus-globules than those of the lungs? Is suppurative inflammation more easily set up in the latter than in the former? Again, must pus, to cause abscesses of the liver, be possessed of peculiar characters? We should incline to his latter opinion, in recalling to mind that we had particularly met with metastatic abscess of the liver in cases in which the first proceeded from suppurative osteitis; but we cannot venture to affirm this. The sole prominent and remarkable fact upon which we have to insist at present is, that pus injected into the mesenteric veins gave rise to exactly the same appreciable changes as if it had been injected by the jugular, the liver seeming to offer no obstacle to its passage and arrival at the lungs." (p. 104.)

As, in man, the course of the symptoms observed depends not upon the passage of a certain quantity of pus into the blood at one time, but upon the persistence of the source of pus; so if we wish to imitate the effects in animals, we must produce analogous morbid conditions, by a repetition of the injection of pus at short intervals. When this is done, we are enabled to produce completely similar symptomatic effects, and especially anatomo-pathological effects. In this way we may examine the various stages from engorgement to complete abscess. The *globules*, or, at all events, the globules with the granules, are the toxical elements; as when these are isolated by filtration, and then mixed with water and injected, they induce the abscesses, which the bad character of the pus may not. Even the serum of pus that has undergone the *putrefactive fermentation*, as well as various other putrid substances, will, however, produce death, and that often rapidly, before time has elapsed sufficient to allow of the occurrence of local lesions; and even in the cases where these do occur, they are different from those caused by the globules of healthy pus, being essentially of a *gangrenous* character. The abscesses, in this last case, depend upon the eliminatory inflammation induced by the presence of dead or gangrenous matters. "There are, therefore, two distinct diseases, though connected by some appearances in common. The one is determined by the solid elements of the pus, the other by the putridity of some animalized substance." An interesting case is also given of "*pyæmia complicated with putrid infection,*" confirmatory of the deductions drawn from the experimental injection of putrid matters. In this, the pus proceeding from an osteitis was of an exceedingly fetid character; the symptoms of infection came on more suddenly, progressed more rapidly, and were accompanied by a more utter prostration, than is the case in simple pyæmia. In the latter, death comes on gradually, and seems to bear relation to the amount of visceral changes; but here the lungs, having gangrenous patches on their surface, and but few traces of suppuration, still continued abundantly crepitant, and capable of receiving large masses of air, though the respirations amounted to forty-five per minute, and the lips and extremities were cyanosed.

Besides his experiments upon animals, M. Sedillot's observations are founded upon twenty-nine cases of pyæmia which have come under his care at the civil and military hospitals at Strasburg, during 1845-7. These are narrated in considerable detail by his assistants, and, with the clinical observations he has appended to them, form a valuable body of information upon the subject. The space we have at command forbids the transcription of any of these; but we are desirous of furnishing the author's forcible description of the *symptoms* of the disease in his own words:

> "A patient is attacked by suppuration, when suddenly, without any other premonitory symptom, or some days after, a hemorrhage, a diarrhœa, a diffuse inflammation, a phlebitis, an erysipelas, or a painful engorgement of a wound, a more or less violent shivering fit comes on. Frequently there is observed a general trembling, chattering of the teeth, a drawing in of the limbs towards the trunk, and a morbid diminution of temperature of the skin; speech is difficult, the words uttered being short and interrupted; the eyes are hollow, and the features contracted; the countenance is of a leaden or yellowish colour; the respiration frequent; the pulse small, soft, and rapid, and an instinctive sense of great peril is presented. The shivering ceases after a period varying from ten to forty-five minutes; the warmth of surface returns, and a slight transpiration is established. Erratic shiverings, however, return, and not infrequently at the same hours as in the first instance; the wound dries up, or the suppuration becomes grayish and fetid; the surfaces of wounds assume a withered, flabby aspect; the bones become denuded, and ill-conditioned ulcers arise or extend. The patient seems as if exhausted by fatigue, and plunged into a kind of *coma vigile*, with occasional delirium, or into a deep stupor; the inspirations are made laboriously, and become more and more accelerated, so that thirty, forty, and fifty per minute are counted; the breath exhales a purulent odour; subcrepitating *râles* are heard in the chest, the air also not seeming to attain the minuter bronchial ramifications; the skin becomes daily more earthy, yellowish, generally as if jaundiced; articular pains, with swelling and intra-synovial effusion, manifest themselves successively in the various joints; one or both of the calves may become the seat of inconsiderable swelling, attended with great suffering; and sometimes severe stiches in the side of the chest force cries from the patient. The tongue becomes dry; the lips and teeth are covered with a fuliginous paste;

the belly is tender, the pulse tremulent and rapid, subsultus agitates the limbs, the eye looks dull, the cornea has lost its polish, the bladder is no longer emptied, partial paralyses may manifest themselves, the voice is lost, and the patient dies from the fourth to the eighth day in a state of extreme emaciation, and after a prolonged agony. These are the most common traits of purulent infection, but it is seldom that we find them all present." (p. 436.)

Examining some of these symptoms in detail, M. Sedillot observes that he has never met with a case of pyæmia in which there was not *shivering* or alternation of cold and heat; though this offers every variety of degree. The number of the *inspirations* is generally proportioned to the severity of the disease, here arising to thirty, forty, or even fifty; and this is an especially important symptom in the absence of signs of extensive pulmonic lesion. Many shades of the *leaden* or *icteric colour of the skin* are observed in different cases, the yellow generally predominating, though not always appearing at an early period. The rapidity with which *emaciation* occurs is very remarkable; so that in but a day or two the patient is scarcely recognisable. The skin of the face is, as it were, plated on to the subjacent parts, the eyes are hollowed, the nose sharpened, and the lips thinned. The *pulse* varies according to the nature of the disease that has preceded the pyæmia; but it is in general soft, moderately full, compressible and tremulant, becoming at last irregular and very rapid. The above-mentioned symptoms are almost constantly present in pyæmia; the others being of less frequent occurrence, yet when united facilitating diagnosis. Among these are the *diminution or suppression of the discharges of wounds, or the conversion of these into fetid or sanious pus.* These consequences do not necessarily follow the development of pyæmia, but they are usually present when it occurs after amputation, especially for gun-shot wounds, comminuted fractures, &c. Neither are the *dry tongue* and *intense thirst* necessarily present, especially at an early period. The knee is generally the joint first affected with *pain* and *effusion*, next the instep, the shoulder, the wrist, and the hip. The most common seat of the *muscular abscess* is in the thickness of the muscles of the calf, where though causing great pain, it may produce but little redness or swelling. It is not a common but a very characteristic lesion when it occurs, and is much more rarely met with in other muscles. The vast purulent collections occasionally occurring under the deltoid and pectoral muscles are sometimes only discoverable by attentive exploration, so little do the patients complain of them. In the advanced degree of pyæmia the *cornea* becomes dull and loses its polish, as if superficially eroded. The author has never met with the purulent degeneration of the globe, said by M. Velpeau to be of common occurrence. Pyæmia having no one pathognomonic sign, we have then to seek the means of its detection in the frequency, conditions of occurrence, succession and simultaneousness of the various symptoms, these being grouped together differently in different cases, owing to the variety of the preceding or accompanying circumstances of the case, and in spite of the constant presence of the one toxical agent.

"Any wounded person having a suppurating wound, in whom irregular shiverings, difficulty and frequency of respiration, a leaden or icteric colouring of the integuments, great prostration of strength, and sudden wasting manifest themselves, is, in our eyes, the subject of pyæmia. We would deliver the same opinion, if, in the absence of shiverings, the above-named symptoms were present, together with a drying of the wound, or a changed character in its discharges. The existence of an ascertained phlebitis allows of our pronouncing upon the invasion of pyæmia, the moment the local symptoms become complicated with shivering, prostration, yellow colouring of the integuments and altered respiration. Arthritic pains and effusions, disorders of the nervous system, the typhoid appearance, induration or abscess of the calves, &c. &c., add but additional degrees of certainty to our diagnosis. Not unfrequently we only suspect the existence of pyæmia upon the discovery of already advanced changes, which we cannot explain upon the supposition of the presence of any other disease." (p. 448.)

In discussing the *etiology* of the disease, M. Sedillot again refers to the facts deducible from his experiments upon animals, and confirmed by hospital cases, that the products of the *putrefactive fermentation of pus* do not induce true pyæmia but *gangrenous* affections; admitting, however, that it is rare to find the putrid elements acting uncombined with unaltered pus, and that the resulting phenomena are then of a complex character (septico-pyæmia). Believing, with Andral, that the serum of pus is identical with that of the blood, he cannot agree to the opinion of Lebert and others, that it is competent to the production of pyæmia; and the fatal results which followed its injection, in his own experiments, were only produced when it had, and in proportion as it had, become putrid. As to the *modes* in which *pus obtains admission into the blood,* the commonest is in consequence of a *phlebitis.* If the phlebitis is obliterating and without suppuration, or the abscesses it gives rise to are circumscribed and open externally, no symptom of pyæmia is induced; but when it is suppurative, and the pus obtains admission into the circulation, we may have all the symptoms at once set up. While, however, on the other hand, some observers, as M. Tessier, maintain the universal presence of obturating coagula, and cannot see the pus in the blood visible to other eyes; other observers take the opposite extreme, and maintain that phlebitis is always present in, and is the sole cause of, pyæmia. But there are many cases in which no phlebitis, capillary or otherwise, exists, and such are recorded in the present work. The introduction of pus in these cases is of difficult explanation, for if we suppose venous erosions too small to be visible allowing it to pass, these imply the existence of a phlebitis. Other channels for the admission of pus, besides phlebitis, are the *openings made into veins by ulceration or by traumatic injuries,* numerous cases of pyæmia following operations. Under the influence of ill-conditioned inflammation, adhesions partly formed may become destroyed, and the orifices of the veins may be entirely freed, or only partially obstructed by the soft sanious matters covering the surface of the wound. If the pus or sanies have not, under these circumstances, a free external issue, they may easily enter the veins and poison the blood; and indeed the aspiration of pus by open veins, has been regarded by Marechal as one of the efficient causes of such admission. The admission of pus in consequence of *lymphangitis* must be a very rare occurrence, as the author has met with no example. M. Fleury gives a case in which the only explanation of the occurrence of multiple abscesses in the lungs, was the detection of pus in the lymphatics of the arm and the axillary glands. MM. Breschet and Lauth have demonstrated the permeability of inflamed lymphatics, and have several times found pus in the thoracic ducts.

M. Fleury is, however, too hasty in drawing a conclusion from a single case, seeing that the pus was not followed into the blood; for as we may not unfrequently meet with pyæmia without any changes in the veins or lymphatics, the existence of pus in this case might be a mere coincidence. Pyæmia has been said to be produced by the *absorption of pus;* but, as the author believes, only by an abuse of terms. By absorption is meant the passage of a substance through tissues that are exempt from solution of continuity; and as the globules and granules of pus cannot so pass, and its serum is not competent to induce the disease, this mode of admission must be rejected. Even as regards putrid serosity this is not habitually absorbed, as believed by M. Berard, but it may be so exceptionally, and then give rise to gangrenous symptoms. The disappearance of abscesses does not offer an objection to these views, as the serum is alone absorbed, or the globules are dissolved before they are so, and so cannot produce purulent infection. The doctrine of the *spontaneous generation of pus,* under the influence of what is termed a purulent diathesis or fever, is untenable.

Among the *predisposing causes* of this affection may be mentioned those which give rise to other epidemic visitations in hospital wards, as the absence of proper attention to hygienic rules, and especially to the prevention of *encombrement,* and its consequent atmospheric vitiation. *Detention of pus in wounds* by fruitless attempts at effecting union by the first intention, and by the strangulating compression exerted by the dressings, is the most fertile cause of pyæmia after amputations; and the author believes, that since he has adopted a means of forming and dressing the stump, which favours the easy egress of the pus, he has succeeded in obtaining a great immunity from the affection. He avoids the necessity of dressings by cutting an anterior flap in such a way that it covers the wound by its own weight, and requires only to be secured at its angles by means of pins, the sharp edge of the bone having been previously cut off, and a small pledget about two fingers broad having been placed in the centre of the wound, which, on being removed on the third or fourth day, leaves a canal through the stump in which the ligatures can be gathered together, and through which the pus obtains a free exit. If the limb is convulsed a handkerchief or band is passed round it, and affixed to the bed or the cradle which protects it from the bed-clothes. If the nature of the operation prevents this procedure being exactly put into force, at all events the free issue of the fluid is always provided for as far as possible. So, too, certain simple rules in regard to the position of wounded parts should be observed. At first they should be elevated, to prevent passive congestions, and facilitate the return of venous blood; but as soon as pus has begun to form, they should be gently lowered to facilitate its issue. *Loss of blood,* whether this proceed from accident or from phlebotomy, seems to singularly predispose to pyæmia, probably by diminishing the plasticity of the blood. The adhesions closing the veins are destroyed, suppuration becomes more abundant and less laudable, and ulcerative inflammation may be set up. The moral and physical causes which give rise to *debility,* frequently induce pyæmia after operations. Despondency, anxiety, insufficient or bad food, diarrhœa, chronic inflammation are among these. "Persons whose flesh is soft and lymphatic, their respiration short, and their organic activity but little marked, are very subject to this affection, while examples of it are both more rare and less severe among the inhabitants of mountainous countries, whose constitutional condition is so different."

Diagnosis. Pyæmia is often mistaken for intermittent fever, bronchitis, pleurisy, rheumatism, meningitis, typhoid fever, metro-peritonitis, &c.; and the error is one of vast importance, for it withdraws attention from the local disorders which exist in the majority of cases, and from the important indications of treatment which these furnish. M. Sedillot first of all endeavours to distinguish what he calls *septico-pyæmia*, that is, where a putrid matter has obtained admission to the blood, from *simple pyæmia* induced by the fresh pus-globule. The putrid infection is, however, probably never perfectly distinct and uncomplicated with the purulent, and it may be looked upon as a sub-genus or variety of the latter. "An altered condition of the pus, giving rise to putridity, gives rise to especially grave symptoms which pursue a rapid course. The adynamia, stupor, delirium, fuliginosity of the gums, teeth, and tongue, are much more marked. The alvine evacuations are fetid, and passed unconsciously, the countenance undergoes more change, the eyes discharge, and the cornea loses its polish. We have to do with a gangrenous infection joined to a purulent infection, and the symptoms are necessarily modified. It is a most serious complication, but yet is a curable one." We must confess the distinction attempted to be drawn between these two affections is not a very perceptible one, the two varying in degree only, and that not always; for some of M. Sedillot's cases, in which no such putrid complication existed, manifested symptoms of just as great intensity.

It is not uncommon to mistake the shivering announcing pyæmia for *intermittent fever;* and if the existence of suppuration pass unobserved, as well as the irregular erratic character of the shivering fit, quinine is given, which indeed sometimes suspends this symptom. Observation of the case shows that the fever is always a quotidian, and never a tertian or quartan, and if in some cases it does come on at the same hours, other irregular intercurrent paroxysms are repeated two or three times in the twenty-four hours. *Bronchitis* or *broncho-pneumonia* are only supposed to exist when the examination is made imperfectly; for although each metastatic abscess commences with a partial pneumonia, the symptoms of this are too indistinct for appreciation. The respiration is so peculiar that practitioners ought not to be deceived. The inspirations are deeper, more energetic, and more frequent than natural, and yet the air penetrates less easily and to a less distance in the bronchial ramifications. The *pleurisies* in this affection are generally latent, the post-mortem examination revealing vast collections of matter, whose existence had not been suspected during life. In other cases, however, the symptoms are very marked, so that the disease is in danger of being mistaken for a simple pleuritis, independent of purulent infection. So, too, *pyæmic arthritis* is not unfrequently confounded with rheumatism, many of the symptoms of the two diseases being in common. In most cases these arthritic pains follow the other symptoms of pyæmia, and help indeed to a better interpretation of these. The fourth or fifth day is generally the latest at which they appear, and they are of more common occurrence in pyæmia that tends to become chronic. When cerebral symptoms of an ataxic form, cephalalgia, restlessness, loquacity, delirium, carpology, &c., become conjoined with an apparently rheumatic arthritis, and the source of pus remains hidden, the diagnosis then becomes truly difficult. Of all affections, however, liable to be confounded with pyæmia, *typhoid fever* is the most so, and especially when typhoid prevails in the wards or localities wherein are wounded persons. If the source of suppuration has been overlooked, the difficulty of diagnosis becomes greater; but the most difficult case of all is presented by a typhoid fever becoming complicated with a pyæmia.

Pathological Anatomy. The general characters of the organic alterations produced by purulent infection, are a dissemination of numerous small centres of inflammation, and the great tendency to the rapid formation of pus, this being indicated only obscurely by the concomitant functional disturbances. Purulence is the definite result of the pyæmic alterations, and is met with under two forms, that of abscess and of purulent effusion. The *lungs* are most frequently the seat of these multiple points of inflammation and abscess. The globules of pus are arrested in their tissue, and accumulating there have to be eliminated as foreign bodies. If additional portions do not continue to arrive to maintain and increase the inflammatory action produced by these, resolution of the inflammation is brought about by the solution and resorption of these morbid elements. When this is not the case from mere little spots of ecchymosis, the tissue becomes gradually more and more hepatized and deep-coloured, and the abscess it encloses increases in size until at last it seems to be in contact with the healthy tissue. Observers who have failed to detect the marks of inflammation, or only have examined the abscesses after such have disappeared, have denied its existence, and have explained the presence of the abscess by the doctrines of metastasis. Pulmonary tissue apparently healthy, bathed in pus, exhibits under the microscope, however, inflammation granules and epithelial lamellæ; but even supposing that from the arrest of the inflammation the parenchyma of the walls of the abscess exhibited no traces of it, that is quite in accordance with the laws of pathology. "The whole error has arisen from the confusion existing between the point of departure and the terminations of suppurative inflammations. The completed abscess has been regarded as a primary alteration, and the prior periods of evolution through which it has passed are overlooked. The same observation applies to the purulent effusions into the various serous and articular cavities." The pus-globules not only induce ecchymosis by impeding the circulation, but also partial emphysematous spots by obstructing the passage of the air. In the *liver*, the *spleen*, and the *muscles*, in like manner as the lungs, are exhibited all the signs of pre-existing inflammatory action in its various stages of induration, gray hepatization, and purulent infiltration, prior to the formation of complete abscess. The great disposition to the formation of matter has struck all observers; the various organs named, as also the cellular tissue, brain, heart, and kidneys, may abound in hundreds or thousands of these abscesses, and the joints, pleuræ, and bursæ, may be filled with pus. Few patients resist long enough to offer examples of this union, but when the changes are confined to but a few organs, still suppuration is the marked character.

"Can we explain this suppurative tendency thus so plainly exhibited? In repeating, with the ancients, that pus engenders pus, we but state the fact; and we may go further. The globules of pus act as foreign bodies, and by these we always find suppuration induced. Pieces of clothing, ligatures, fragments of wood, sequestra, projectiles, &c., have no other means of elimination. Why, then, should it be otherwise with the pus-globules if Nature does not accomplish their solution? Again, the first effects of the penetration of pus into the blood is the distress of the respiratory organs; and we all know to what an extent persons having an incomplete hæmatosis are liable to suppurations. Compare the mountaineer to the inhabitant of low and humid places, and you can entertain no doubt upon this head. Here, then, we have two powerful causes of suppurative inflammation; and to these we may add the functional disturbances of the liver (and, perhaps, of other glandular organs); and we have in the mixture of the elements of the bile with the blood a new condition of purulence. Are we to accord a great value to the fact of the presence of pus in the blood? Yes, certainly, in an etiological relation, but in nowise as a general cause, and one acting independently of the irritative and suppurative action of the pus-globules upon the tissues they come into contact with. The blood does not seem to us to be directly altered by the pus." (p. 467.)

We may notice a few other of the numerous particulars the author has collected respecting *multiple* or *metastatic* abscesses. They may be found in any part of the economy; but as we have seen especially in the *lungs,* which organ is seldom exempt when others are attacked. As the results of M. Sedillot's observations, in 100 cases of pyæmia we should find the lungs affected in 99 of these; the liver and spleen in 1 of 12 cases; the muscles, 1 in 15; the heart, 1 in 20; and the peripheric cellular tissue about the same; the brain and kidneys being thus affected the seldomest. The pulmonary abscesses usually occupy the surfaces and free edges of the organ, the middle and lower lobes especially containing them. Sometimes the intervening structure remains normal and crepitant, and at other times is dense, being filled with a reddish-brown sero-sanguinolent spuma. In some cases, difficulty exists in distinguishing the early stages of these abscesses from crude tubercles; but in general, their different locality, and the use of the microscope, prevent this error. In respect to the *liver,* sometimes only small purulent spots are found on its surface; and at others, vast collections occupy its substance. Numerous small disseminated abscesses are not so often met with as in the lungs. The *spleen* may be crowded by minute abscesses no bigger than pins' heads, or, in smaller number, they may acquire the size of an almond, nut, or larger body. In some cases, the pus was in a state of infiltration, constituting more or less considerable whitish masses. Frequently (as also was the liver), except in respect to the inflammation surrounding the abscesses, the organ was quite normal in texture. Of *muscular abscesses,* the calf is the favorite seat; these commencing in multiple and distinct points, which, at first hard and circumscribed, gradually unite into a soft fluctuating tumour. Abscesses of the *cellular tissue* are frequently found scattered over nearly the entire body, under the skin and the muscles, and in the vicinity of joints; often containing large quantities of pus, which no sign revealed during life. Little abscesses have been often observed in the thickness of the muscular substance of the *heart,* or under the lining membrane of its cavities. In the *kidney* it is especially in the cortical part the abscess is found. Abscesses of the *brain,* seldom surpassing a pea in size, are found disseminated in both the white and gray substance, and generally seem as if hollowed out of healthy tissue, though attentive examination exhibits the marks of inflammation. In some exceptional cases it would be impossible to count the thousands of these abscesses scattered over the body; and the author has met with hundreds of them in a single lung. Multiplicity is one of their characteristics; but in some cases a dozen, or even less, have been found in the lungs. There is no absolute rule; but in general, when they are very numerous, they are also very small. In respect to the agency of *capillary phlebitis* in the production of these abscesses, M. Sedillot observes:

> "We must have no playing upon words here; for it is evident that no parenchyma can be attacked with phlogosis without the capillaries participating. But if it is understood, by this phlebitis, an isolated inflammation of the small vessels amidst intact cellular texture; and if it is supposed that pus is formed and recognisable in the venous ramuscules, observation does not confirm this hypothesis, and we may fearlessly deny the existence of the phlebitis." (p. 470.)

Abscesses, when fully formed, may evacuate their contents into neighbouring cavities, or externally like other purulent collections; but at other times these contents are resorbed, the pus being at first replaced by a clear serous fluid ("the liquefaction of pus"), and all traces of this afterwards disappearing.

It is to *purulent effusion into the pleura* that attention has been chiefly directed, this being far more common than effusions into the peritoneum or arachnoid, which, indeed, are rare. The author has never observed such pleuritic effusion without coexisting pulmonary alterations, which, indeed, he regards as a necessary correlation. Sometimes there are only partial pleurisies; and at others, pseudo-membranes in various stages of organization. The most remarkable cases are those of *consecutive pleurisy*, consequent upon an abscess of the lung having made its way into the pleura. Purulent effusions into the *joints* are of frequent occurrence.

> "When a bone has been sawn across in amputation, and a fatal purulent infection has resulted, it is common enough to find pus in the joint above. Here the cause is evident; the inflammation being propagated by the medullary canal, in which we may generally discover sanies and pus. But, in many cases, joints far removed from the source of primary suppuration become the seats of such effusion; and in explaining this, we must invoke not only the presence of the globules of the pus, but also the facility with which the synovial membranes reflect all general perturbatory causes. We can offer only doubtful explanation of the sudden disappearance of articular swelling and effusion, and we confine ourselves to indicating them." (p. 478.)

As the author has endeavoured to point out the symptomatic distinctions between purulent infection, and its complication with a putrid element (*septico pyæmia*); so he also believes that their pathological characters are sufficiently distinctive. As purulence is the distinguishing trait of pyæmia, so is gangrene of putrid infection; and hence we find in purulent infection, complicated with putridity, "blackish, brown, or gray projections on the lungs, having soft walls, and being true gangrenous phlyctenæ filled by an altered and spumous blood. The affected tissues are easily torn through, offering irregular, eroded, grayish surfaces. Suppuration is tedious, and takes place at the circumference of the mortified parts, and petechiæ are observed in different parts of the body."

Prognosis. Although ordinarily the progress of this disease be from worse to worse, and that with rapidity, in other cases it is of slower procedure—the mean duration of the disease being from four to eight days, according, at least, to M. Sedillot's own experience. The disease has generally been regarded as incurable, but the author long since published proofs of the incorrectness of this opinion, and the present work contains both in the experiments and cases detailed, its ample refutation. Still we believe the author speaks too sanguinely upon this point, or he must include cases not usually comprehended under this term. Thus he says:

> "MM. Vidal, Blandin, Velpeau, Nelaton, and others have published some successful cures. We may reassure practitioners by telling them that such ought to be the ordinary result of well-directed treatment. Pyæmia is of infinitely more frequent occurrence than is generally admitted, and for the most part terminates successfully. Incurability is the exception; and far from proclaiming the powerlessness of art, we believe it possible to throw farther back the limits of its applicability, and yet extend its resources." (p. 452.)

The danger in pyæmia is in direct proportion to the *amount of pus poured into the circulation.* Where large quantities rapidly obtain admission, the progress of the disease may be so rapid (*pyæmie foudroyante*) that the formation of the metastatic abscesses has been even prevented, it being an error to suppose that the existence of such abscesses is essential to constitute a case of pyæmia. Emphysematous patches and numerous points of ecchymosis are found on the lungs, and the signs of asphyxia in other parts of the body; and the patients succumb by reason of the defective hæmatosis. In such a case the danger is extreme, and death almost inevitable. In other cases the quantity of pus mingled with the blood is less at a given time, but is continually augmented by the arrival of new parcels, presenting a very dangerous form of *acute pyæmia,* which must become incurable if the source of the pus cannot be arrested. If this is accomplished by amputation, or other means, the patient will then be saved or lost, according to his condition of vitality, and to the degree of alterations that have been already produced. When pus is introduced in small quantities and with intermissions, a state of *chronic pyæmia* is induced. The consequent organic changes are inconsiderable, of slow development, and of easy resolution if the source of pus be dried up. The *qualities of the pus* also influence the result. Thus a thick pus, abounding in globules, is more dangerous than a serous pus; and an inodorous, laudable pus causes fewer accidents than does an altered and fetid pus. The *conditions* in which the patients may be placed are highly important in the consideration of the prognosis. Thus, as regards *constitution,* a person of a lymphatic temperament, with soft flesh and hurried breathing, whose vessels are of small calibre, and in whom moderate exertion induces dyspnœa and palpitation, ill resists the invasion of a pyæmia. As regards *age,* the author's observations lead him to believe that youthfulness is favorable to the cure. The great cause of the fatal termination of surgical cases being *faulty hygienic circumstances,* the existence of such materially influences the prognosis. *Hemorrhages* not only predispose to pyæmia, but render its cure more difficult, owing probably to the depressed *morale* of the patient, and the greater tendency to suppuration in enfeebled constitutions. Upon this point M. Sedillot makes the following incidental observation:

> "I have formerly heard men of undoubted authority declare that tying the principal artery of a limb would be a very efficacious means of arresting the progress of an inflammation by a suspension of the supply of arterial blood; but experience has shown me the error of this supposition. I have observed many examples of abundant and inexhaustible suppurations occurring in organs in which the circulation of red blood had been in part suspended by the ligature of the arteries. The

most vigorous men are the least liable to suppurations. Facts like this should greatly modify ill-founded opinions." (p. 488.)

Treatment. After adverting to the vague, empirical, and desponding views which have prevailed upon this subject, M. Sedillot lays down the following plan of procedure: 1. *The prevention of the causes* (prophylaxis). This would be best effected by the suspension of the formation of pus, but although this is often impossible, yet, as all suppurating wounds do not induce pyæmia, we should investigate the additional circumstances upon which it especially depends, and these seem especially to be *faulty hygienic conditions* and *retention of pus in the wounds*. The author mentions the fact that formerly a moat filled with stagnant water existed near the Strasburg Hospital, and that then few patients were saved after operations, whereas since it has been filled up, success has been as great. Still he regards as a chief cause of pyæmia, the *retention of pus in the wounds*, which induces inflammations and erosions, and consequent phlebitis. If union by the first intention could certainly be obtained, it would operate as our best preventive; but unfortunately this is not the case, and he reprehends the attempts as deceptive.

> "It is principally in reference to amputations that so much controversy has prevailed upon this subject, and for my part I have a strong feeling against attempting union by the first intention. I have performed or seen performed hundreds of amputations at Paris, Metz, Strasburg, and in Poland, Germany, and Africa, and I have never yet been witness of one case of the complete success of this mode of dressing the stump. I know, however, such do exist; but how often have not patients been considered as cured whose partially cicatrized stumps yet presented fistulous apertures, from which daily escaped a certain quantity of pus? I have found such patients in a yet more precarious position, one, two, or three, and even six months later, their health having become deteriorated, and a fatal pyæmia being set up. In union by the first intention, the integuments adhere together easily enough, but the blood, lymph, and pus remain accumulated beneath around the extremity of the bone, ferment there, and acquire fetidity; the deeper textures become altered, the bone is denuded, and pyæmia is imminent." (p. 496.)

All inflammatory strangulation of the soft parts concerned in wounds must be guarded against, whether by local bleeding, the cautery, or *debridement*. The traumatic surfaces should be maintained in their vitality by moderately stimulating applications. M. Sedillot refers to the injudicious preference given to the bistoury in recent times, in several operative procedures, where *caustics* would act much better, as exciting obturative or obliterating inflammation.

The second indication is, *in the case of pyæmia becoming declared*, to arrest the penetration of pus into the blood. This is endeavoured to be accomplished in five modes: 1 The opening of a free issue for the pus. 2. The conversion of a pyogenic membrane into an eschar, and thus suppressing suppuration from its surface. M. Bonnet passes the actual cautery over the whole of the suppurating surface for this purpose; but M. Sedillot prefers proceeding more cautiously unless the surface is very limited, fearing otherwise that by acting only upon the superficial layers, pus might be imprisoned by the eschar, and strangulation produced. 3. Obliteration of the veins by intercurrent applications of the actual cautery effectually impedes the conveyance of pus by substituting an obturative to a suppurative phlebitis. 4. The actual cautery is the most valuable means we have of also modifying an unhealthy, fetid condition of the wound, conditions which may produce septico-pyæmia. Various other astringent and caustic topical applications are of much use to this end. 5. Ablation of the suppurating parts is of course a means of great efficacy, but requires much consideration before deciding in favour of it.

The third indication relates to the treatment of the local inflammations due to the presence of the pus-globules; but under this head we find nothing worthy of notice.

Having thus brought the principal points of M. Sedillot's work under our readers' notice, we may observe, that although we consider it a valuable contribution to the literature of the subject, its value appears to us to consist rather in the number of facts it contains, than in the reasoning by which these are explained. In the first place we believe, as already stated, that certain miasmatic poisons are capable of producing the train of symptoms he has indicated, and, in fact, we cannot doubt that they did so in some of the cases where he has attributed this agency to pus; and, indeed, when we consider the numberless cases in which pus may obtain access to the blood, and yet does not give rise to purulent infection, we may well suppose that some co-operating cause is essential to its production. His theory is excellent for the explanation of the production of the metastatic abscesses; but we cannot regard these as other than a mere part of the affection; for if sufficient pus is thrown into the circulation, death results long before these can become developed. Then again his acknowledgment of the toxical effects of putrid matters introduced into the blood, even separated from the obstructing globules, seems fatal to his theory; especially since the fatal effects of such agents proved far more rapid than those caused by mere pus, and are often exhibited before any of the gangrenous results on which he bases the distinction could be developed.

It can scarcely be questioned that purulent fluids have a peculiar tendency to undergo decomposition; and we believe that the facts of the case will be better met by supposing that in this condition pus may act as a "ferment," in common with other putrescent substances, than by considering that it has any special agency, otherwise than possibly that of mechanically obstructing the capillary circulation.

Art. IV.

A Short Sketch of the Life and Writings of the late Joseph Clarke, Esq., M.D., V.P.R.I.A., *and formerly Master of the Dublin Lying-in Hospital, &c. &c.; containing Minute Results of his Private Practice, extending over a Series of Forty-four Years, including Three Thousand Eight Hundred and Seventy-eight Births.* By Robert Collins, M.D., President of the King's and Queen's College of Physicians in Ireland, &c. &c.—*London,* 1849. 8vo, pp. 88.

This is an interesting and important publication. It contains an obstetric record which, in some respects, is without a parallel. It is one perfectly to be depended on, and comprises no less than 3847 cases of labour, representing 3878 births, personally attended and registered by one of the most distinguished obstetricians of his age, and occurring amongst the higher ranks of society. It is the rich legacy of an active professional life, extending over forty-four years, which has been made an available addition to the science of midwifery by Dr. Collins. It will be received by all whom experience has taught to appreciate the records of an honest and discriminating mind with interest and respect.

A short sketch of the principal events of Dr. Joseph Clarke's life, written by himself, and extended by a more detailed account of his history, writings, and private practice, by Dr. Collins, forms another feature of this publication. In this part, however, we have been disappointed. The life of a practising physician is almost necessarily scanty in incident; but we hardly could anticipate such a meagreness of personal history in one who had filled so conspicuous a position in a large capital, and for so long a time, as Dr. Clarke. Those who, like ourselves, did not know Dr. Clarke, can learn but little of him from this biography. And yet the large expressions which Dr. Collins, with such a warmth of feeling and veneration, employs in reference to him, excite a strong desire to learn more of his individual character, and of what he said and did. The outline is broad, but there is no filling up. We feel, too, that Dr. Collins's long and intimate acquaintance with Dr. Clarke might have supplied a most useful and instructive account of his midwifery consultations; a kind of knowledge which is much wanted, and which might well be gleaned from such a life as Dr. Clarke's.

It appears that, after a good general education, Dr. Clarke studied medicine at Edinburgh, where he graduated with ease to himself and some reputation. He afterwards travelled on the continent with a patient; and before returning to Dublin, spent some time in London, which enabled him to attend Dr. William Hunter's course of lectures, including those on the gravid uterus. He had previously determined to practise as a physician in Derry; but by the advice of his friends, and particularly Dr. Cleghorn, he resolved to settle as an accoucheur in Dublin, a fair prospect of success being opened by the death of his grand-uncle, Dr. Maconchy. He then entered as a pupil of the Lying-in hospital, with something short of £300 of money, and in about eighteen months afterwards he was appointed assistant to the master, for each of which appointments he paid 100 guineas. He subsequently travelled with an invalid gentleman, from which, he says, he "acquired much valuable information from visiting the London and continental hospitals, gratified a great deal of curiosity, diminished somewhat his youth (which is adverse to professsional success), and saved of his allowance £240." He married, in 1786, Isabella Cleghorn, niece to Dr. George Cleghorn, with whom he received a fortune of £1500; and on the 3d of November of the same year he became master of the Lying-in hospital. Dr. Cleghorn was the founder of the Anatomical School in Trinity College; and in this school Dr. Clarke was called upon to assist him as a lecturer for about two years.

During the time that Dr. Clarke was pupil and assistant-physician to the Lying-in hospital, his attention was directed to the frightful mortality amongst the infants, from what the women called nine-day fits, which destroyed no less than one in six. Dr. Clarke referred them to a defect in the ventilation of the hospital, which, at his urgent suggestion, was remedied. There was constantly provided an easy and free passage of fresh air through the wards; the number of beds was reduced; and the wards kept more clean. The mortality from this time diminished; and in six years it amounted to 1 in 19⅓, instead of 1 in 6. At the present time this disease is nearly extinct. Dr. Collins has rather a questionable way of computing by statistics the value of these suggestions. He speaks of 20,000 lives having (humanly speaking) been saved; "as, had the mortality of infants continued *one* in six to this day, of the 158,436 (which is the total number born alive up to the end of 1847), we should have had 26,406 deaths, instead of 5,704, as the hospital registry now shows." Of course, any one writing twenty-five or fifty years hence may carry on this arithmetical sum of lives humanly saved, until at last the number would appear preposterously great. We are far from undervaluing the sagacity which detected and rectified this evil; but it appears to us that Dr. Collins, by this method of calculation, makes rather too much of it.

When Master of the Lying-in Hospital, Dr. Clarke adopted the system of lecturing; which was the means of establishing the School of Practical Midwifery, which has since acquired high and deserved reputation. Dr. Collins says that he was a most lucid and agreeable lecturer, and had the happy facility of making his class clearly comprehend what he wished to convey.

Several Essays were published by Dr. Clarke, which are mentioned and commented on in a laudatory manner by Dr. Collins. They relate particularly to the diseases of infancy, and are sufficiently known to the profession. Two letters on some of the causes of the excess of mortality of males above that of females, in which Dr. Clarke shows that at the time of birth the size of the male head and the weight of the body are greater than that of the female, are referred to. These observations, with those bearing on the cause of death, have been abundantly confirmed by the experience of other authors since his time.

> "He found that there was a difference of *nine ounces*, or nearly a twelfth part of the entire, between the weight of the male and female; and that in the size of their heads there was a difference of nearly half an inch, or about a 28th or 30th part. The weight of the majority of male children at birth he observed to run thus—7lbs., 8lbs., 6lbs., 5lbs., whilst that of the females was 7lbs., 6lbs., 5lbs., 8lbs. In 120 instances, only five children exceeded 8½lbs. in weight; and in the size of their heads, only six measured above 14½ inches in circumference, and these all of the male sex; five measured 14¾ inches, and one, 15 inches. It is obvious, from the facts stated, that, in the birth, male children are more exposed to injury than female." (pp. 33-4.)

The last essay which is noticed is entitled "Observations on Puerperal Fever, more especially as it has of late occurred in the Lying-in Hospital of Dublin."* Dr. Collins has introduced in connexion with this subject some correspondence between Dr. Clarke and some distinguished London physicians, which is interesting as showing the different type of disease which occurred in their respective experience. Dr. Joseph Clarke, in his private practice, lost only three cases from puerperal peritonitis. In writing to Dr. John Clarke, he says:

> "One great difference between the puerperal fever in London and Dublin is this, that with you it extends very much from the hospitals into the city, and even adjacent country; here, I can speak positively for the last twelve years, it hardly ever appears in the city. I have never lost a patient by it in private practice, and

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* Edinburgh Med. Commentaries, 1791, vol. xv.

for the last five years I have had as many patients as any physician in Dublin; even in consultation I meet it very rarely. When an accidental case does occur, in private practice, I have never failed to subdue it in a day or two." (p. 37.)

This was in 1794; and up to October 1829, out of the entire number of cases, namely 3867, there were only twenty-two deaths, and three of these had symptoms of peritonitis. It is perfectly obvious that Dr. Clarke had not met with that infectious form of puerperal fever, accompanied with low typhoid symptoms, which we now know to be associated with uterine phlebitis and a poisoned circulation, and to which Sir Charles M. Clarke thus refers in this correspondence. "Every year's experience convinces me of the intractable nature of this complaint, and of the entire inadequacy of any mode of treatment with which I am acquainted in curing it." We can well understand how so successful and excellent a physician as Dr. Joseph Clarke should, when perusing the fatal records of these cases as occurring in London, declare that "it was impossible for him adequately to express the gratitude he felt to a kind providence for having spared him the pain of witnessing a similar mortality."

Besides these three cases of peritonitis, out of the twenty-two deaths *five* cases died from phthisis, four of whom were confined prematurely; *two* died from hemorrhage, one before the birth of the child when the placenta was found *entirely separated*, and the second after the delivery of a live child by version in a placenta presentation. There were *two* deaths after turning in shoulder presentations; and the remaining ten were of a miscellaneous character not admitting classification. Of these we notice one from a paralytic stroke followed by convulsion, with death on the tenth day after delivery; one from scarlet fever, which began on the third day and was fatal on the fifth; and two from lacerated vagina.

This record of fatal cases is much diminished in value from the absence of the particulars of post-mortem examinations; but the smallness of the number is a striking proof of the skill of the physician, and, we may add, of the comparative exemption from sources of danger in childbirth amongst the rich as compared with the poor.

We cannot follow Dr. Collins into the details of the statistical data which he has so carefully arranged from Dr. Clarke's note-books; but we would particularly refer the reader to them, and to the inferences which flow from a study of the comparative results of private and public practice. We cannot fail, however, to notice that out of eighteen cases of laborious and protracted labour, there were twelve crotchet cases, one vectis case, and only one in which the forceps were applied, but without succeeding in delivery with them. The remaining four were delivered naturally. All the mothers recovered favorably. In the crotchet cases Dr. Clarke had the assistance of professional friends.

> "If we seriously reflect upon the happy results to the *mothers*, from the practice pursued by this distinguished physician, as regards the use of instruments, and then carefully examine the succeeding section upon children *still-born*, and find here equally happy results (as of the 3816 *single* births, there were only *forty-two* children still-born of those that had arrived at the full period of gestation, or in the singularly small proportion of *one* in 91), we cannot fail to discover a number of *astounding* truths, sufficient to warn our *artificial* advocates, and make them pause until *they can supply* their professional brethren with a series of facts equally satisfactory." (p. 57.)

And again with his significant allusion to his recent controversy with Dr. Simpson:

> "How seldom should most practitioners be found to use instruments, if the successful course pursued by Dr. Clarke were universally aimed at. Is it not worthy of our best consideration, with the invaluable statement before us, that, in an extent of practice in the upper ranks of life perhaps unexampled, there is *not one single instance of death* resulting from *laborious* or *protracted labour*. This is a practical *fact*, which ought to be carefully recollected, and seriously weighed, by most of our continental brethren, who use instruments in every fifth, tenth, fifteenth, twentieth, or thirtieth labour under their care, with the object of *expediting* delivery; as also by some of our own countrymen, whose unsound doctrines inculcating mischievous interference to promote *hasty* delivery, the unquestionable truths here recorded clearly demonstrate to be unjustifiable and most uncalled for. Should not this inexpressibly important record for ever silence those who venture to publish crude and fanciful opinions, unsupported by any data from their own experience affording similarly happy results. It affords me infinite satisfaction to supply this truthful registry of facts, for the universal and serious consideration of the profession. These truths speak in language the most convincing, and must, when studied, leave an indelible impression." (pp. 58-9.)

It is quite beside our purpose to enter into the controversy to which Dr. Collins has referred, or to express our own opinion upon the subject of it. We are bound, however, to say, that although we may rejoice at and admire the success of Dr. Clarke's practice, yet we see no reason why midwifery is to remain fixed and stationary where he has left it. We believe from the perusal of this work, and from what we have derived from other sources, that the sound sense and scientific knowledge of Dr. Clarke greatly improved the bad practice of his day; but it appears to us that in restricting the use of the long forceps, so as virtually to all but abrogate them, he ran into just the opposite of the prevailing error. We learn from Dr. Collins that accoucheurs of any practice of Dr. Clarke's day, carried about with them the forceps or the vectis in a pocket expressly made to receive them, and the just inference is that there was a frequent and most unwarrantable employment of them. Still we feel persuaded that Dr. Clarke, perhaps from a desire to err on the safe side, carried his objections to an extreme; and his prejudice (for so we must call it) against this most useful instrument has infected the school which he founded ever since. We doubt whether Dr. Clarke would have retained the same opinion, were he now with us. We feel that had he known the value of obstetric auscultation, could he have listened to the fœtal heart and known when the child was alive, those twelve crotchet cases would probably have been reduced, and a case or two of the long forceps with live children would have been the evidence of their successful employment in protracted labour. At least this is our own opinion. Dr. Collins appears to us to be held in bonds by Dr. Clarke. They may be, and no doubt are, the bonds of veneration and love; but still they are bonds. They fasten down his views to a particular level—they keep him strictly in the track of his beloved predecessor and relative; and the impression which he gives us is, that he looks with jealousy and suspicion on any material deviation from Dr. Clarke's practice. For our own part we should not hesitate in a suitable case to turn the child in a case of contracted conjugate diameter, as recommended by Dr. Simpson, just as

Dr. Denman did in one case, in spite of the heavy weight of Dr. Collins's ponderous statistics or the emphasis of his *italics*.

Dr. Collins has introduced a highly interesting letter from Dr. Sims to Dr. Clarke, respecting the death of the Princess Charlotte; and it leaves on our minds the lamentable conviction that had there been prompt means employed directly to the uterus, to ensure its contraction, this valuable life might have been spared. We feel that this letter cannot be abridged, and we transcribe it in full:

> "Her Royal Highness's labour commenced by the discharge of the liquor amnii about 7 o'clock on Monday evening, and pains followed soon after; they continued through the night and a great part of the next day, sharp, short, but very ineffectual. Towards the evening Sir Richard Croft began to suspect that the labour might not terminate without artificial assistance, and a message was despatched for me. I arrived at two on Wednesday morning. The labour was now advancing more favorably, and both Dr. Baillie and myself concurred in the opinion that it would not be advisable to inform Her Royal Highness of my arrival. From this time, to the end of the labour, the progress was uniform, though very slow; the patient in good spirits, pulse calm, and there never was room to entertain a question about the use of instruments. About six in the afternoon the discharges became of a green colour, which led to a suspicion that the child might be dead; still the giving assistance was quite out of the question, as the pains now became more effectual, and the labour proceeded regularly though slowly. The child was born, without artificial assistance, at 9 o'clock in the evening. Attempts were for a good while made to reanimate it by inflating the lungs, friction, hot bath, &c., but without effect; the heart could not be made to beat even once. Soon after the delivery, Sir Richard Croft discovered that the uterus was contracted in the middle, in the hour-glass form; and, as some hemorrhage commenced, it was agreed that the placenta should be brought away by introducing the hand. This was done about half an hour after the delivery of the child, with more ease and less loss of blood than usual. Her Royal Highness continued well for about two hours; she then complained of being sick at stomach, and of noise in her ears; began to be talkative, and her pulse became frequent; but I understand she was very quiet after this, and her pulse calm. About half-past 12 o'clock she complained of severe pain at her chest, became extremely restless, with a rapid, irregular, and weak pulse. At this time I saw her for the first time, and saw immediately that she must die. It has been said we were all gone to bed, but that is not a fact. Croft did not leave the room, Dr. Baillie retired about eleven, and I went to my bed-chamber and laid down in my clothes at twelve. By dissection, some bloody fluid (two ounces) was found in the pericardium, supposed to be thrown out in articulo mortis. The brain and other organs all sound except the right ovarium, which was distended into a cyst, the size of a hen's egg; the hour-glass contraction of the uterus still visible; a considerable quantity of blood in the cavity of the uterus, but those present differ about the quantity, so much as from twelve ounces to a pound and a half; the uterus extending as high as the navel. The cause of Her Royal Highness's death is certainly somewhat obscure; the symptoms were such as attend death from hemorrhage, but the loss of blood did not appear to be sufficient to account for a fatal issue. It is possible that the effusion into the pericardium took place earlier than what was supposed, and it does not seem to me to be quite certain that this might not be the cause. As far as I can judge, the labour could not have been better managed. That I did not see Her Royal Highness more early was awkward; and it would have been better that I should have been introduced before the labour was expected; and it should have been understood that when the labour came on I should be sent to without waiting to know whether a consultation was necessary or not. I thought so at the time, but I could not propose such an arrangement to Croft. But this is entirely *entre nous*." (pp. 68-70.)

There is an interesting correspondence between Dr. Clarke and Drs.

Denman and Savage, respecting the propriety of the induction of premature labour in a case where the pelvis was defective, which will repay a perusal.

The few particulars of Dr. Clarke's private and professional life, which Dr. Collins has recorded, exhibit a most prosperous career, and a highly estimable character. He made, by practice, £3,000 a year; and, by accumulation, his receipts within the first twenty years reached annually £5,000. His patients were almost entirely of the highest rank, and his midwifery fees were large in proportion; so that altogether, in fees varying from £10 to £150, he received £37,252. In November, 1801, in his fee-book, he notices the change from the gold guinea to a one-pound-note as the "first of these vile productions" whereby he lost 2*s*. 9*d*. on each fee.* This, as Dr. Collins says, was by no means an agreeable innovation to physicians; and we are surprised that a nearer equivalent of the old coin—say, for instance, half-a-crown—should not have been insisted on as a kind of make-weight to the light note. With us, there is now-a-days a little fraudulent tampering with the physician's fee, as though a guinea and a pound were the same thing. We too often get mulcted of our shilling—that useful coin—which, in its annual accumulation, keeps the physician a horse, or covers his early rent, or more than deprives the income-tax of its burthen; or, when he gets to his fullest professional receipts, may even add a carriage to the horse, or pay a tax or two in addition to the rent, or be provident as insurance-money; or, last of all, as we have heard, supply an extravagant wife with pin-money! We abhor changes in the currency, for they are always to our detriment; and we sincerely hope that Dr. Bowring may long be preserved at Hong Kong, or any other place equally remote from the Commons House of Parliament, and that we shall never again hear of any proposition to change shillings into coins of tenpence, and twenty of them into a pound, unless a good honest-looking piece be struck at the same time, of the value of twenty-one shillings sterling, and perhaps a little more, in special remembrance of the medical profession.

In early life, Dr. Clarke was a tall, slightly-made man, hardly equal to the fatigue of his profession.

> "On one occasion of debility, he sent for Dr. Harvey (a well-known physician in Dublin) to advise what he should do; and after detailing to him his inability for so much work, &c. &c., Dr. Harvey asked him abruptly, 'What do you eat for dinner?' and 'what do you take after it?' 'Indeed,' replied the Doctor (whose appetite was but indifferent), 'I sometimes take a little mutton-broth, with a little boiled mutton, and at other times chicken. I take very little wine, or occasionally a little drop of spirit in water.' Dr. Harvey replied, 'Phew—nonsense, man; take your roast mutton and roast beef, with a pint of good port wine daily, and you will be as stout a man as any in the kingdom.' 'I took his advice,' said the Doctor; 'I speedily improved, and enjoyed excellent health afterwards.'" (80-81.)

In 1820, Dr. Clarke had the misfortune to lose, at the early age of 32, his only son; a physician who was much beloved, and who died from fever which he caught when prescribing for a patient. In 1829 and 1833 he was left childless by the death of his two daughters. In September, 1834, he left Dublin for Edinburgh, to attend the meeting of the British Association. His bowels were rather unsettled from the sea-voyage; but be continued cheerful, and intended to dine with the members of the association on the 8th of September; not feeling very well, however, he desisted from doing so. In the evening he was sick, said that he felt differently under this attack from what he had ever experienced before; and spoke of his wish, should the attack prove fatal, that his remains should be taken to Moneymore, where his father was buried. He told Mr. Learmouth that he had no fear of death, as his trust was fixed on his blessed Redeemer. He passed an uncomfortable night, suffering much from griping pain and spasm of the bowels. He gradually became torpid, his voice indistinct, his features greatly changed, and he died on the 10th of September, at the age of seventy-six. Much alarm was excited, as the disease was thought to be cholera; and it was considered prudent at once to remove his remains, in order to abate the fear and excitement which this occasioned.

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* The old guinea was worth £1 2*s*. 9*d*. Irish currency.

"Of him," says Dr. Collins, "it may indeed be said, that as a physician or as a friend he was without guile. Many, both in and out of the profession, when in difficulties, sought his counsel, impressed with the conviction that he could and would give them sound advice to guide them safely through; nor did they ever find themselves disappointed.

"He was a man naturally of a most cheerful disposition; and his fine expressive countenance so beamed with benignity, as to make the most favorable impression, even upon those who were strangers to him." (p. 87.)

ART. V.

1. *Elements of Electro-Biology, or the Voltaic Mechanism of Man; of Electro-Pathology, especially of the Nervous System; and of Electro-Therapeutics.* By ALFRED SMEE, F.R.S., Surgeon to the Bank of England, &c. Illustrated by numerous Engravings on Wood.—*London,* 1848. 8vo, pp. 164.
2. *The Dependence of Animal Motion on the Law of Gravity.* By HENRY WIGLESWORTH, M.B., Scholar in Physiology in the University of London. —*London,* 1849. 8vo, pp. 210.

WE believe that it is Pinel who relates the story of a madman, who laboured under the delusion of believing himself to be the Holy Ghost, but who was cured by meeting with another lunatic possessed with the same belief. "There cannot be *two* Holy Ghosts," he sagaciously reasoned; whereupon he began to consider the validity of his own claim to the title, and ended by renouncing it.

Life, with Mr. Alfred Smee, is all electricity; with Dr. Wiglesworth, animal motion, which is one manifestation of life, is nothing else than gravity. Both cannot be right;—is either?

Each of the authors before us can urge a certain claim to be heard; but the pretensions of the two are by no means of the same order. Mr. Smee first became known as the inventor of the very useful voltaic battery which bears his name, and as the improver of the art of electro-metallurgy, on which he has published a valuable treatise. He next endeavoured, however, to possess the public with the conviction that he had discovered the cause of the potato disease; but the botanists and the entomologists proved stubborn adversaries, and by their united forces the *aphis vastator* was driven out of the field. Nowise discomfited by this failure, but with undiminished confidence in his own resources, Mr. Smee returned to the original enterprise, which, as we now learn, he has been prosecuting amongst many interruptions for the last ten years; and he now comes before us with the matured results of his experiments and reasonings, which are to revolutionize the whole science of physiology, and to establish his title to a place among the masters in philosophy. For some time past, mysterious paragraphs have been seen in the public prints, heralding the disclosure of wonderful discoveries in electro-biology, said to have been made by Mr. Alfred Smee, F.R.S. These announcements have doubtless excited the curiosity of those who knew nothing of Mr. Smee except by the trumpet of newspaper fame, which he has contrived to get blown in his behalf both loudly and continuously. Some curiosity, too, may have been felt by those who were better acquainted with Mr. Smee's true position in the scientific world, as to the plan of his next assault upon public credulity. The production thus heralded will afford an easy means, if we mistake not, for deciding at once and for ever upon the validity of the author's claims to scientific notoriety; being evidently regarded by himself as his *chef-d'œuvre*, and containing a system of doctrine so much at variance with those which are at present in vogue, that their originality cannot be for a moment disputed. Of their other attributes we shall not prejudice our readers by expressing an opinion, until we shall have given them an opportunity of judging for themselves, by placing before them a series of extracts from Mr. Smee's work, sufficient to convey an idea of his system and of his mode of enunciating it. We shall be much mistaken if these extracts are not found to afford most satisfactory proof, that the author is either a wonderful genius, or deserves a precisely opposite character.

Dr. Wiglesworth's claims upon our attention are of a very different order from Mr. Smee's. The honours he has gained as a student in physiology indicate that he cannot be charged with ignorance either of the fundamental principles or of the details of that science; his work has not been forced into public notice by any puff preliminary, but has been left to work its own way; and the author, having satisfied his own mind by the consideration of the body of evidence which he has collected, modestly gives it to the public, "that they may consider and determine on it likewise." We do not couple his work with the preceding, therefore, on account of any similarity in the spirit or style of the two; their association is occasioned simply by the resemblance in their dominant idea—that of accounting for *vital* actions on *physical* principles. This idea, as our readers must be well aware, is not new. It has been revived by the physicists and chemists of successive generations; the progress of science giving it, on each occasion, more or less of novelty of form, but the fundamental conception remaining the same.

In his first chapter, Mr. Smee gives us the following as his general view of Life:

> "In organized beings, therefore, the changes occurring in the organization alone constitute the vital phenomena. Hence all animal beings, and even man himself, is solely constituted of matter, and obedient to physical laws. All phenomena of nutrition, growth, assimilation, excretion, together with the action of the senses, of memory, of thought, of reason, of action by word or deed, are phenomena

produced by virtue of organization, and consequently solely obedient to physical laws.

"Whilst the dissecting-room reveals to us the structures necessary for the organization, the laboratory the composition of the respective parts undergoing change, universal science demonstrates that even proud man himself is not endowed with any imponderable, ether, or any other than a physical property for the purpose of manifesting any of those phenomena which are classed under the head of vital actions.

"Man, however, is immortal. Man at all times, and in all regions, has believed in his immortality. It is probable that this conviction has its origin in his very organization. Now, that which gives to man his immortality, can have no relation with that which is mortal. Life, mind, thought, reason, knowledge, come from organization and cease at death. 'When the breath of man goeth forth, he shall turn again to his earth; and then all his thoughts perish.'" (pp. 3-4.)

If we analyse the first of the foregoing paragraphs, we shall find it to contain two distinct propositions, which have been separately entertained by preceding speculators, but which have for the first time, we believe, been brought into correlation by Mr. Alfred Smee. That the phenomena of organic life, with those properly included under the designation of *animal* life, are to be accounted for on physical principles, is a doctrine, as we have already remarked, by no means novel; but those who have heretofore entertained it have concerned themselves only with phenomena obviously *material,* and have arranged those of *mind* in a category altogether distinct. On the other hand, there have been men in all ages who have thought themselves more enlightened than their fellows, and who have considered themselves as showing a noble superiority to vulgar prejudices, in maintaining that between mind and matter there is no essential difference, and that all the actions of the former are simply and necessarily the results of material organization, and follow its laws; but such reasoners have recognised in life and organization something altogether distinct from the operation of physical and chemical laws. It was reserved for Mr. Smee to combine these two doctrines, and to account not only for nutrition and secretion, but for sensation, memory and thought, upon electrical principles.

We are not going to raise against Mr. Smee the vulgar cry of "materialism" and "atheism," which every one is liable to excite who speculates a little more boldly than his fellows. On the contrary, we think that in these days of "shams" and "conventionalisms," Mr. Smee is entitled to some credit for the honesty with which he has expressed opinions that must be very unpalatable to the thinking public. We wish, however, that he had explained himself a little more fully, in order to avoid misconstruction. How man's individuality can be preserved after death, if not only his life but his "mind, thought, reason, and knowledge," cease with his material organization, we find it difficult to conceive. Nor do we find it easier to comprehend what valid ground there can be for the belief in our immortality, if that belief has its origin in nothing else than our perishable "organization;" since as such, it is merely one of the "thoughts" which are to cease with our bodies. Such matters as these, however, are beyond our province. *We,* at least, will profit by the poet's admonition, and will not be guilty of the folly of intrusion "where angels fear to tread."

The following is the simple mechanism which Mr. Smee deems competent to supply "the requisites for the maintenance of organic life:"

> "Now a central apparatus, supplied with a peculiar fluid, a peripheral apparatus similarly supplied, the whole connected together to form one universal total, is the apparatus desired, and such an apparatus we have in a double voltaic battery.
>
> $$\left\{\begin{array}{c} \mathrm{Z} \text{ ——— } \mathrm{S} \\ \mathrm{S} \text{ ——— } \mathrm{Z} \end{array}\right\}$$
>
> "Now, if we abstract the proper exciting fluid from either end, or substitute any other fluid, or destroy the structure either at one end or the other, or divide the connecting portions or wires, the effects proper to the apparatus will not be manifested, and the battery will be destroyed." (p. 5.)

He next tells us that the ordinary forms of voltaic battery would not answer to the physical mechanism of man; since a human being contains no metallic plates nor wires, but consists solely of animal membranes and fluids. This, however, is not a difficulty of any weight; since, as is well known, the different animal tissues moistened with blood, supply all the necessary conditions for a galvanic pile. That electric disturbance does take place in the living body, as a part of the operations of nutrition, &c. there is adequate evidence, which we wonder that Mr. Smee has passed by. The readers of Professor Matteucci's lectures are well aware that, among other phenomena of this kind, he has described a "muscular current," which passes from the interior to the exterior of every muscle varying in energy, according to the activity of the nutrition of the muscle experimented on, and evidently dependent upon the molecular changes which occur in the muscle itself,—tending to its disorganization when in a state of functional activity, and to its repair during a state of repose. This, we apprehend, affords the explanation of the result obtained by Mr. Smee in the following experiment, provided there be no source of fallacy in the mode in which it was conducted.*

> "The first animal which was honoured was a black rabbit, into the masseter of which I introduced one sewing-needle, whilst the second was placed in the subcutaneous cellular tissue. After leaving them for a few minutes, so that they might be in the same state, they were connected with the galvanometer, without sensible deflection of the needle. After a few moments, the animal not liking its treatment, made an attempt to bite my finger, and the deflection of the galvanometer instantly showed the mechanism of volition. I then gave the creature a piece of wood to bite, upon which it used all its power of mastication, and by catching the oscillation of the needle, a very powerful current was exhibited.
>
> "In this experiment, the deflection of the needle in the electro-voltaic circuit, proved the existence of a voltaic current passing through the parts during the action of biting; and did thus denote the mechanism of the force employed to throw the muscles into operation." (p. 7.)

The inference that "the force employed to throw the muscles into operation" must be electric, because "a voltaic current passes through the parts during the action of biting," is worthy of the logical acumen displayed by Mr. Smee, throughout his treatise. It will probably occur to our readers, that the molecular changes concerned in muscular action, involving (as they seem to do) a chemical change, might give rise to a voltaic current, which should be the *result* instead of the cause of the operation. But this aspect of the phenomenon does not seem to have occurred to Mr. Smee, who builds upon his experiment the hypothesis of a peripheral battery, which "consists of the muscular substance forming one pole, the cutaneous tissues the opposite, the serous fluid, which lubricates the parts, being the electrolyte." It is from this battery, according to our author, that the *muscular* as well as the cutaneous nerves are given off; an idea which will be new to the physiologist, who has been accustomed to consider the muscular nerves as originating in the nervous centres, and as conveying their influence to the muscles. According to Mr. Smee, however, the nerves are merely cords of communication between the peripheral battery; consisting of the muscles and skin, and the central battery, whose existence is proved in the following simple manner:

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* Since the above was written, the question of the development of electricity during muscular contraction has been brought under the consideration of the Institute of France, and various physicists and physiologists, both in France and in this country, have investigated the subject experimentally. The result appears to be decidedly *negative*, when the various sources of fallacy (of which there are many) are excluded or allowed for.

> "If we follow the course of the nerves, we find that they are prolonged to the brain, and end in the gray matter, where they again come in contact with a large quantity of blood-vessels. As the two series of nerves are not immediately connected in the brain, it follows, according to the laws of voltaic action, that another battery exists there, which may be termed the central battery." (p. 9.)

Having thus laid down his fundamental propositions, Mr. Smee proceeds in his second chapter to discuss the electric mechanism of sensation, under the term *Electro-Aisthenics*. The chapter is divided into sections, headed by the euphonious designations, *Opsaisthenics*, *Ousaisthenics*, *Gumaisthenics*, *Rhinaisthenics*, *Cœnaisthenics*, and *Somaisthenics*, which treat of the electric mechanism of sight, hearing, taste, smell, touch, and the bodily feeling, respectively. On the first of these subjects he gives us some new light from actual experiment. He finds that if two platinum poles be immersed in certain mixed chemical solutions, and the apparatus be exposed to a strong light, one of the poles being covered with an opaque vessel (such as the bowl of a tobacco-pipe) so as to keep it in darkness, an electric current will be generated between the two poles. This affords, according to him, a simple solution of the mechanism of vision. That an electric current is actually produced by the operation of light upon the retina, has been ascertained by Mr. Smee in the following manner:

> "To apply this test to the eye, one needle should be thrust into the eye of an animal through the choroid coat; and a second into the muscle in the neighbourhood; when if a sudden transition be made from darkness to strong light, a very slight deflection of the galvanometer declares the presence of a photo-voltaic current.
>
> "There are unquestionably considerable difficulties in the operation; but by careful management, and watching the oscillation of the needle, the current may be made decidedly appreciable. In estimating these effects, two tests are applicable: first, the motion of the motion of the needle in one direction; and secondly, the stopping of the oscillation of the needle when the current is reversed. A very feeble current may be ascertained with certainty by this manœuvre." (pp. 15-6.)

We certainly should like to see this result for ourselves, before believing it. We do not for a moment suspect Mr. Smee of intentional perversion; but we think that there is at least one source of fallacy in the mode in which the experiment was made, that destroys its value. The second pole should not have been inserted into a muscle, but into the other eye;

and the one eye should be shaded from the light, whilst the other is exposed to it. By inserting it into the muscle, we obviously encounter the risk of disturbance from the muscular current. There is considerable *naiveté* in Mr. Smee's account of these inquiries.

> "In detailing the results of these experiments, I think it right to state that I have relied upon a multitude of trials. Sometimes I have utterly failed to produce any effect; at other times, I have been dissatisfied with my results; and it is only occasionally, when I have been fortunate enough to get an animal at his ease, and in so little fear that he would be happy and comfortable during so unpleasant an operation, that I have succeeded in obtaining satisfactory results. It is a curious circumstance, that in no instance have I ever seen any permanent injury inflicted on an animal from this mode of treatment. I have caught strong, healthy cats, and subjected them to similar experiments, but they generally have been in such a terrible fright, from the confinement necessary to keep them quiet, that I could make no use of them for Electro-Biology." (p. 16.)

This passage strongly reminds us of the touching account given by some of the Italian disciples of Haller, of the martyr-like self-devotion which was displayed by the dogs upon which they inflicted the most agonizing tortures, in their zeal to confirm and extend their master's discoveries. For the honour of their country they are delighted to record that these dogs were all of Italian races, except one unhappy victim that howled and struggled whilst he was being dissected alive; and *he*, they gravely narrate, was of Grecian extraction.

Granting, for the moment, that Mr. Smee's experiment gives a correct result, and that a voltaic current *is* excited by the impinging of light upon the retina, what does it prove? Certainly nothing more than that, as in the contraction of muscle, the molecular and chemical changes which take place between the blood and the nervous matter give rise to electric disturbance. That this is the case in the apparatus upon which Mr. Smee builds his explanation of the action of the organ of vision, is obvious enough. If the solutions are not such as to be acted on chemically by light, no electric disturbance will ensue. The current is the *result* of the chemical change, and is not in any way essentially connected with it. The apparatus just described is regarded by our author as presenting a close imitation of the real eye.

> "The essential structure of the eye consists in a definite number of distinct poles of the electro-biological batteries; and according as they are influenced by illuminated images, an action would occur in a sort of pattern, similar to that used by ladies for the designs of worsted work." (p. 18.)

He knows, it appears, everything about it, except the precise number of the batteries in operation.

> "Now, if we could only tell how many points of definite magnitude we could observe at one time, we should be enabled to decide the number of opsaisthenic poles which exist in that part of the eye; but, at present, I have not succeeded in learning the fact." (p. 18.)

We hope that he will persevere in his experiments until he has discovered this; and that he will then be able to construct genuine *seeing* eyes, capable of being supplied at a low price to the unfortunates who stand in need of them. We do not see why, by the proper application of electric circuits, every body who likes it should not have an additional pair of eyes fixed in the back of his head; the great utility of which, as endowing the possessor with a marvellous power of *circumspection*, need not be enlarged upon.

Mr. Smee has not advanced quite so far in his development of *Ousaisthenics;* not having been yet able to bring experiment to bear upon the demonstration of the true and essential nature of the mechanism of sound. His faith, however, in its electric nature, is not the less firm.

> "At first sight there certainly does appear much difficulty in constructing a voltaic circuit, which shall be acted upon by the vibration of sound. Upon consideration, however, that difficulty will not be found to be so great, for as the internal ear is inclosed in a solid unwieldy case, it follows that the part on which a vibration impinges, will have the delicate capillaries emptied of blood. From this result, according to the bio-electrolytic law, the oxygenated corpuscles would not be present at the termination of the nerve, and a voltaic circuit must of necessity be formed." (p. 20.)

The following is his formula for the construction of an *artificial ear:*

> "We may merely, in a remote and imperfect manner, imitate such a state of things, and form an artificial ear, by fixing a piece of vellum over a glass vessel shaped like a funnel, and terminating in an inverted syphon. When the vellum is thrown into action, the water would be displaced in the tube, and as a consequence thereof, a circuit might be made or broken. By labour, I have no doubt but that a perfect acoustic telegraph could be made, which shall be acted upon by sounds, and have the power of transmitting them to any distance." (p. 20.)

The subject of *Gumaisthenics*, also, is very summarily dismissed. Mr. Smee has no doubt that the mechanism of the sense of taste corresponds with that of other departments of electro-aisthenics; and offers us the following as the mode of constructing an *artificial tongue:*

> "We may make a voltaic battery, in which the circuit shall be determined by savours, in very different methods. For instance, if we place a little per-salt of iron, with two platina poles, in a V shaped tube, and then drop a little infusion of meat into one side, a voltaic circuit will instantly be produced. In nature, taste is probably excited by the absorption or contact of savours." (p. 21.)

Does not Mr. Smee know that *any* substance, whatever its taste, if capable of reacting chemically with the fluid in the tube, would produce a voltaic current, so long as the condition of the fluid in the two legs remained different? How this experiment, then, peculiarly illustrates the mechanism of taste, we are quite at a loss to discern.

Passing on to *Rhinaisthenics*, we find that Mr. Smee considers that he has succeeded in determining the existence of a voltaic current during the act of smelling,—after the following fashion:

> "To examine the mechanism of the nose experimentally, a needle should be thrust up the nasal organ,—an operation to which most animals have an extreme repugnance. The other needle should be inserted in the textures about the alæ nasi, and the scent may be excited by a little hydro-sulphuret of ammonia. The result of this experiment is often masked by secretions which interfere with the result; but, in good instances, the galvanic needle will be deflected as though the nose were the positive pole of the battery." (p. 22.)

We certainly should ourselves experience an extreme repugnance to the thrusting of needles up our noses; we cannot, therefore, be surprised that Mr. Smee's dogs and cats should manifest the same. He seems not altogether unconscious in this instance of a source of fallacy which in our apprehension entirely vitiates his results; for as there seems good reason to believe that the act of secretion is itself attended with electric disturbance, anything which excites this process will produce an electric current through its means, rendering it unsafe to affirm that any other cause of such a current must be in operation. Mr. Smee's misgivings, however, do not prevent him from speculating upon the rationale of the current, with as much coolness as if he had demonstrated its existence; and the following is his recipe for an artificial nose:

> "An artificial nose may very easily be formed to act with certain odours, such as ammonia; for, if we place two pieces of iron in a tube divided by a diaphragm to imitate a nose, and place each piece in contact with very dilute muriatic acid, no current will arise. If to one pole, however, the fumes of ammonia be applied, polarity will be produced, and a voltaic circuit generated." (pp. 22-3.)

Here, again, we have direct and positive chemical action, taking place in a manner which must of necessity produce an electric current. Mr. Smee takes no notice of the fact that irritating odours, such as ammonia, act through the nerves of common sensation rather than through the proper olfactive sense; but such trifling physiological matters as these are quite beneath the consideration of a physical philosopher who can so easily reduce every action of the body to a voltaic current.

Under the head of *Cænaisthenics*, Mr. Smee considers the sense of feeling, as brought into action by heat or cold, or by pressure or other mechanical force. Of course he has no difficulty in regard to the first of these modes; since, as he correctly states, "it is a perfectly simple and ordinary physical phenomenon for a voltaic circuit to be excited by temperature." Nor does he find any greater difficulty in the consideration of a voltaic circuit excited by force; "for if by the pressure we prevent the arterial corpuscle from coming in contact with nerve-fibre, action must ensue, inasmuch as the balance would be destroyed, polarity would ensue, and action take place." He states, that he has experimentally demonstrated the existence of such a current, after the following fashion.

> "We have only to introduce one needle into the muscular tissue, and a second under the cutaneous structure, when a distinct current is immediately manifested in the galvanometer, when the animal is pinched or otherwise irritated. From this experiment we learn that the cænaisthetic pole is positive—the muscle, negative. Of course, the electro-voltaic current, by which we render manifest this phenomenon, is in the reverse direction." (p. 25.)

We must confess to such an obtuseness of apprehension, that we cannot see the essential difference between the conditions of this experiment, and those of the one first cited as having afforded to Mr. Smee his proof of the mechanism of muscular action. For Mr. Smee speaks of the movement in the former case as having been sometimes excited by "pinching or otherwise irritating" the animal; and as in the latter he uses the same excitants, we are at loss to discover on what ground he sets down the electric current as in the one case produced by muscular action, and in the other by cutaneous sensation. None of these trifles, however, seem worthy of his attention; and he gravely proceeds to the construction of an *artificial skin*, after the following fashion:

> "We may imitate this kind of circuit by very easy means. For instance, if we take two pieces of iron wire, and insert them in very dilute acid, no action ensues, when tested by the galvanometer. The same result is obtained if the blood-corpuscle be imitated by taking a membrane containing a little nitrate of iron, and

placing one such artificial corpuscule against such iron wire. As soon, however, as the artificial corpuscule is thrust aside from the pole, a very powerful current is generated, which has its origin at that pole where no corpuscule exists. This structure is strictly and perfectly analogous to the natural mechanism of the body." (p. 24.)

Into the mechanism of *Somaisthenics*, which seems to be nothing else than the "muscular sense" of physiologists, Mr. Smee does not enter; contenting himself with remarking that here, as elsewhere, "no difficulty surrounds the consideration of the voltaic mechanism of the somaisthenic pole; for as we can have no motion without force acting upon certain parts, it is certain that this force might prevent the arterial blood from coming in conjunction with the nerve, and thus a circuit would be generated." (p. 27.)

Proceeding now to the central battery, our author unveils to us in the third chapter the "Voltaic mechanism of the brain," under the designation *Electro-Noemics*. The full exemplification of its structure by artificial apparatus, would occupy, he informs us, many acres; and he therefore contents himself with exhibiting examples of its mode of acting in its several departments. From these we must make a further selection; but we shall be much mistaken if the few specimens we shall present are not sufficient to settle the minds of our readers as to the worth of the whole. The first section is entitled "*Memory a voltaic phenomenon;*" the proof of which proposition is conceived by Mr. Smee to be contained in the following extract.

> "When a man receives an impression, it is not evanescent, passing immediately away, but it is retained in the system to regulate future actions. Now, in voltaic constructions, it is not difficult to produce an action which shall influence future motions, and thus exhibit the effects of memory.
>
> "If we take two iron wires, and place them in a solution of argento-cyanide of potassium, and direct a voltaic current through them, silver would be reduced at that wire constituting the negative pole. The two wires would be ever afterwards in different electric relations to each other; one would be positive, the other negative; and thus the effects of memory would be shown, and future actions regulated." (pp. 30-31.)

So, again, we find that "desire is a voltaic phenomenon."

> "The faculty of desiring resolves itself into a tendency to act, and is manifested when the central batteries are in a condition of excitement. Desire is to mental operations similar in all respects to tension in electric arrangements. When the desire is gratified it ceases for a time. This phenomenon is similar to an exhausted battery, in which arrangements exist for replenishing the exciting fluid; as in this case, after a time the battery would again become active, and exhibit tension, which I have, in my sources of physics, described as a desire for action ungratified." (p. 39.)

Pleasure and pain, moreover, are the result of the same kind of voltaic mechanism (p. 41); the ideas of "time," and of "absolute concurrence, or 'all time,'" are accounted for with the same marvellous facility.

The higher operations of the mind are in like manner reduced to voltaic mechanism in the fourth chapter, which is entitled "Electro-Psychology." He does not offer us, however, any attempt at a demonstration of this hypothesis; but bases it upon what he conceives that he has already proved in regard to his "lower batteries." His exposition of the mode in which some of our most important ideas are derived from the combination of simpler elements has at any rate the merit of conciseness:

> "Other combinations doubtless gives us other ideas; thus personality and infinity give us the idea of the soul; pleasure and infinity, of good; pain and infinity, of bad; cause and infinity, of God; time and infinity, of eternity; infinity, pleasure, and time, of heaven; infinity, pain, and time, of hell. Personality, and all the units of sensation, give us the idea of the body; personality, infinity, and time, of immortality. Personality, and other totalities of senses, give us the idea of the mind; thought and infinity, of spirit. Lastly, action, infinity, and pleasure, conjoined, give us the idea of virtue; action, infinity, and pain, of vice." (pp. 45-6.)

The moral character, according to our author, is entirely formed by external influences.

> "Electro-noemics indicate that man, at the beginning, is perfectly free to act in any manner from external impressions; but after he has once received impressions, these also regulate his future proceedings. He is then necessitated to act well, if he has received good impressions,—badly, if he has received evil impressions. In fact, electro-noemics indicate the fundamental principle of education, for they teach that if you 'train up a child in the way he should go, when he is old he will not depart from it.'" (p. 46.)

According to this view, there is no individuality but such as circumstances may have induced; the minds of all being precisely in the same condition before they have been first called into action. We infer from this, that Mr. Smee has never had an opportunity of studying the differences of character and conduct in two children brought up under the same circumstances. We had supposed that the comparison of the infant's mind to a sheet of white paper, on which anything may be impressed, had been long since abandoned as utterly absurd and untenable.

Not only moral philosophy, but also religion, is simply a result of voltaic mechanism.

> "The moral law is infinite, and, being manifested in the pneuma-noemic battery, rules every specific instance; and hence the desire for virtue would prevent any vicious act. In the lower battery, action will be determined entirely from the pleasure or pain of the immediate impressions; but these actions are controlled by the ideas of infinite pleasure in the higher battery." (p. 47.)

So, again, "religions, whether true or false, have the same effect in controlling the actions from the influence of the higher batteries."

In Chapter V our author again carries us back to the peripheral batteries; and first considers muscular motion, under the head of *Bio-Sarco-Dynamics*. His idea of the cause of muscular contraction is certainly original; unfortunately we do not see where the proof of it is to be found.

> "According to this view of the case, muscular contraction ensues from the material existing in the ultimate fibre being increased in bulk by changes taking place in consequence of the voltaic circuit." (p. 51.)

We shall presently see that Dr. Wiglesworth gives a very different explanation of muscular contraction, on the basis of precisely the opposite assumption,—namely, that the bulk of the muscular substance *diminishes* during contraction. Both these assertions are inconsistent with the most carefully conducted experiments; the uniform result of which has been to show that *no change* in bulk takes place during muscular contraction. This, however, is quite immaterial to Mr. Smee, who gives the following simple directions for the construction of an *artificial muscle* upon voltaic principles:

"To effect this object, the sheath of the ultimate fibre should be imitated by a bladder, or perhaps more strictly, by a piece of the gut of any animal. Into the interior of this, a strip of platinised silver and a small quantity of sulphuric acid should be introduced. The gut or bladder should be tied round at both extremities by a piece of strong cord, which would close the apertures, and serve for artificial tendons. The whole must then be immersed in dilute sulphuric acid, containing a positive pole of zinc. When the zinc and silver are connected, gas is evolved within the artificial muscular fibre, when it widens and shortens. This contraction acts upon the artificial tendons, to produce any required motive power." (p. 51.)

We are afterwards enlightened as to the mode of constructing "an artificial electrical fish;" but one thing seems to be at present beyond Mr. Smee's power, namely, to construct an artificial glow-worm, no mode of forming which has hitherto occurred to him.

The four succeeding chapters, entitled *Bio-Statics*, *Bio-Electrolysis*, the *Electro-Biology of Cells*, and the *Electro-Biology of the Spinal Cord and Sympathetic System*, need not detain us; since the views which are developed in them do not differ so widely from those advanced by other electricians and physiologists. The *Aphis vastator* is again brought in; Mr. Smee having a notion that from the same stock may be produced either the Vastator or the Bean Aphis, according to the kind of electricity to which the brood is subjected. He admits, however, the possible fallacy of the experiment; and remarks, that if confirmed, "it would not amount to a great deal, because the Bean Aphis and Vastator do not differ more than a white man and a black man." We do not agree with him in this low estimate of the importance of his supposed discovery; for it would unquestionably change the aspect of the present human population of the globe very considerably, and would afford an easy solution to great social problems of extreme difficulty, if all black races could be converted into white by the simple action of a voltaic current.

We have thus endeavoured to lay before our readers a concise account of Mr. Smee's system, nothing extenuating, nor setting down aught in malice, and using his own language in every instance in which we might be suspected of exaggeration or distortion. Upon his therapeutics and pathology we need not descant, since we have very recently discussed the therapeutic value of electricity; and any one who has made himself acquainted with his biological system must see its applications to the prevention and cure of disease almost as clearly as Mr. Smee himself. We cannot think it necessary to occupy the attention of our readers by any serious discussion of the question, whether we think and feel by electric currents, however strong may be the evidence of their presence as a part of the actions of organic life. To say that memory and desire are nothing else than voltaic phenomena, is at once, it appears to us, to stamp the character of the whole system, and of the mind from which it emanates. We again repeat that he must be either a second Newton, or a shallow pretender; it will be our safer course, however, not to indicate which way the balance hangs, but to leave our readers to suspend and observe it for themselves, giving him the full benefit of the alternative.*

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* There are certain cases in which "the better part of valour is discretion." We do not wish to be called upon to discuss Mr. Alfred Smee's merits in a court of law, whither he appears disposed to drag every critic whose opinion of his 'Electro-Biology' does not happen to please him. (See Medical Times, April 21.) We might, therefore, have passed by his production without notice; but, in doing so, we should have lost the opportunity of placing the author's own estimate of his merits before our readers, who would doubtless be sorry to have missed such an opportunity of becoming acquainted with the wonderful discoveries of Mr. Alfred Smee, F.R.S.

When he shall have succeeded in the construction of a living, moving, feeling, thinking, moral, and religious man, by a combination of voltaic circuits, all doubt will be dissipated. Mr. Alfred Smee will then most assuredly rank as the greatest genius of this or any other age.

We have now to apply ourselves to the discussion of Dr. Wiglesworth's hypothesis; which, as we have already remarked, is advanced with more of the modesty befitting an aspirant for scientific honours, though with the confidence of one who has well considered the subject for whose advancement he is labouring. The two authors are by no means similar in manner or matter. Mr. Smee's *forte* lies in his physics; Dr. Wiglesworth's in his physiology. Mr. Smee passes from one position to another in such breathless haste that we can scarcely follow him, much less comprehend the ground of his assumptions; Dr. Wiglesworth's positions, on the contrary, are comparatively few, and he labours most sedulously at the establishment of each upon a secure basis. In so doing he manifests considerable comprehensiveness in his survey of facts, great ingenuity in their analysis and application, and much argumentative power in bringing his several deductions to bear upon the positions he desires to establish: hence we are disposed to augur very favorably from Dr. Wiglesworth's essay of what he may do for science at some future time, when his aims shall be better directed. The fatal error of his present attempt will be found to be, that he has started with an entirely erroneous conception of the physical facts of the case: however ingenious, therefore, his reasoning upon physiological phenomena may be, he cannot bring the two into the relation which he seeks to establish. We shall endeavour to give a fair account of his theory, and point out what we deem its fundamental errors.

The contraction of a muscle is regarded by Dr. Wiglesworth as the result of atmospheric pressure acting upon its substance; a *vacuous state* being produced in that substance by the chemical changes which are now generally admitted to be essentially involved in muscular action. Of these changes, the formation of urea is one of the resultants; and taking advantage of the well-known fact that urea and water may be resolved into carbonic acid and ammonia, our author represents it as "extremely probable that urea is not directly formed by muscular action, but that the first chemical change is the production of ammonia, which substance is presented to carbonic acid in such a form as to produce urea and water, rather than the usual compound carbonate of ammonia." Now both carbonic acid and ammonia are remarkable for this,—that the vapours of their components, when united, are condensed into half their bulk; two volumes of carbon vapour, *plus* two of oxygen, forming only two volumes of carbonic acid; and six volumes of hydrogen, *plus* two of nitrogen, forming only four of ammonia. Hence, reasons Dr. Wiglesworth, "if these substances are formed during muscular action, as sound evidence proves them to be, a vacuum must necessarily be formed also." Here we join issue with Dr. Wiglesworth. We have no objection to concede to him, in order that we may start from the same point; that the carbonic acid and ammonia which contain the equivalents of urea and water, may be formed as such during muscular action. But he assumes that they are formed from their component *gases;* a position which is altogether untenable. Surely Dr. Wiglesworth must be aware that these compounds do not result from the direct union of carbon-vapour and oxygen, hydrogen and nitrogen, existing previously in the condition of uncombined and *elastic fluids;* but that they result from new arrangements of the elements of *solids* or *liquids* which previously occupied a much less space. Hence if the production of these gases had anything to do with the movement, it would be, as in Mr. Smee's artificial muscle, by their distensile force, not by creating a "vacuous state" by their reduction in volume.

Again, Dr. Wiglesworth is not unaware of the fact to which we have already referred, that the volume of the entire muscle remains the same during its contraction and relaxation; but he endeavours to get over this difficulty by calling to his aid the doctrine current among physiologists, that all the fibres of a muscle are not in contraction at the same time, whilst the muscle is putting forth its power; but that some are relaxing whilst others are shortening. This explanation, however, will obviously not stand a rigid examination. Dr. Wiglesworth must admit that in the extended muscle *all* the fibres are in the state of relaxation, and consequently occupying their fullest dimensions; and that in the contracted state, *some* of these are shortened, and therefore (according to him) diminished in bulk, whilst others remain as they were; there must consequently be a reduction in bulk of the entire muscle in the contracted state, which is contrary to fact.

But further, not only has Dr. Wiglesworth abstained from putting his hypothesis to the test of a very simple experiment, but he has disregarded the known results of experiments already made (though with a different purpose) which are quite adequate to disprove it.—If muscular contraction be due to the pressure of the atmosphere, the removal of that pressure must put a stop to it. Now it is for Dr. Wiglesworth to show us that such is the case. He might very easily place the prepared leg of a frog under the receiver of an air-pump, in such connexion with an electric circuit that when he shall have exhausted the receiver he can send a charge through its nerve. We assert, without the least hesitation, that muscular contraction would be excited under such circumstances as readily as under the pressure of the air; and we do so because we have seen a beetle crawl, and a frog's heart continue its pulsations, within an exhausted receiver.

Having thus, as we think, settled the question of the merits of Dr. Wiglesworth's hypothesis of muscular motion, by proving a direct negative in regard to the assumptions on which it is based, and the conditions it involves, we need not occupy further space in discussing the various arguments adduced by the author in its support. These arguments are all of them of the nature of circumstantial evidence; none of them can lay claim to the character of proof; and, consequently, they are all disposed of by the direct negation with which we have met the doctrine itself.

Not only muscular but nervous force is attributed by our author to simple physical agency. His hypothesis is as follows:—The chemical changes occurring between the oxygen of the blood and the hydrogenous contents of the nerve-cells of the brain, determined by an act of volition, give rise to an expansion which would produce an enlargement of the cells if the brain-case permitted; but as this is impossible, the force is expended on the moveable fluid in the adjacent nerve-tube, which, being thus driven onwards, propagates an impetus to the opposite extremity, where it comes into relation with the muscular fibre; and the mechanical pressure thus exerted on the fibre excites its contraction. The distension of the tube is prevented from acting upon it during its course, by the closeness with which it is packed in the nerve-trunk; and it is only when it is entirely separated from connexion with others, and no longer surrounded by its special sheath—which is just at its termination,—that we shall find a capacity for enlarging under pressure. "As the muscular fibre will thus be affected, simultaneously with the cranial impulse, the promptness of nervous action is as satisfactorily explained as it could be even by electricity." (p. 79.) In like manner, the impressions made upon the peripheral extremities of sensory nerves by mechanical pressure, are supposed to be conveyed by the propulsion of their fluid to the nerve-cells of the brain, producing "an inconceivable amount of yielding in their contents, followed by the formation and condensation of gases; and that the impulse derived therefrom informs the soul of the impression." (p. 83.) This is considered by Dr. Wiglesworth to be a sufficient account of the mode in which tactile, gustative, auditory, and olfactive impressions are originated and conveyed; in regard to the visual, he conceives that the rays of light, which are well known to be extremely active in promoting decomposition, occasion the gaseous combination in the cells of the retina; and that the distension thus produced communicates the required impulse to the fluid in the nerve-tubes.

Now we cannot but admit that this hypothesis has a certain show of probability, inasmuch as mechanical pressure upon the trunk of a nerve will undoubtedly give rise to remote effects—sensorial or motor—through the propagation of a certain influence along its course. But we cannot imagine that any one sufficiently conversant with physical science to understand the laws of the propulsion of fluid, can for a moment admit the possibility of the instantaneous transmission of an impulse from periphery to centre, or from centre to periphery, along canals bounded by walls of such extreme tenuity as the membranous coats of the nerve-tubes. How far any transmission whatever could occur in tubes so thin-walled and flexible, we will not take upon us to affirm; but it is quite certain that no such *instantaneous* transmission could take place without *perfect rigidity* in their walls; and even then, the force requisite to propel any fluid through a capillary tube of such extreme minuteness, so as to move a column of several feet in length, is altogether beyond what any distension of cells at either extremity could be supposed capable of effecting, especially when there is nothing to prevent their own expansion. We cannot assent to the doctrine that the cranial investment of the brain prevents any change in the dimensions of the nervous mass; such an assertion being completely opposed to experience. And even if it did, the same condition does not exist in other ganglionic centres. Again, the transmission of a mechanical impulse along any individual fibre, whose own walls would readily yield to it, would necessarily throw into vibration the fluid contained in all the other tubes bound up with it in the same fasciculus; and thus they would be all made to act in some degree consentaneously with it, there being assuredly no such separation between them by intervening tissues, as would remove them from each others' influence. Moreover, the *indistensible* condition is far from being universal among nerve-trunks; for in the mollusca, the fibres lie so loosely within the neurilemma, and are themselves so soft, that the trunks were mistaken by the older anatomists for absorbents, and were injected with mercury. If anything be required to complete the disproof of Dr. Wiglesworth's hypothesis, it will be found in the effects of an electric current transmitted along a small part of a nerve-trunk, in occasioning the transmission of nervous agency along the remainder, so as to excite motion in the muscle it supplies, or sensation in the central ganglion to which it proceeds. For the electric stimulus, having here no cells to act upon, can scarcely be supposed to occasion any mechanical propulsion of the contents of the nerve-tubes; yet without such distension, no remote effects could be produced.

We shall in the last place briefly notice Dr. Wiglesworth's application of his doctrine to the phenomena of cell-growth, as concerned in the functions of organic life. He bases this upon the explanation he has already given of muscular action. The ultimate cells of the muscular fibril, having been partially emptied by the process already described, remain in that contracted state until the muscle is again extended by the action of its antagonists; when there will necessarily be, according to Dr. Wiglesworth, a passage of nutritive liquid from the blood-vessels towards the interior of the cells, to supply the partial vacuum thus created in their cavity. This is considered by Dr. Wiglesworth to be the type of all cell-growth; the particular fluid which each cell is found to contain having been forced into it by atmospheric pressure, and the selection of the different fluids being accomplished by a sort of filtration through the porous walls of the cells, each being endowed with a molecular structure, adapted to give passage to a certain class of particles, and to them only. In the latter part of this doctrine there is nothing new, it having long since been abandoned as untenable. The former portion, however, certainly possesses the merit of originality; and the reader who shall make his way through the section on secretion will certainly be struck, as we have been, with the ingenious mode in which it is advocated. Thus according to our author, the requisite vacuum is produced in the cavity of the kidney, so as to occasion the filling of the uriniferous cells, by the self-dilating action of the walls of the urinary bladder; in the liver, a similar effect is occasioned by the self-dilatation of the gall-bladder; in the salivary glands, by the movements of the mouth. He forgets to explain to us, however, in what manner this is effected in animals which have no urinary nor biliary receptacle; and he does not attempt to apply his hypothesis to the phenomena of growth in a simple isolated cell, such as a protococcus or palmella, in which are displayed all the essential phenomena of the life of the component parts of other organisms.

On the whole, then, we cannot regard Dr. Wiglesworth's hypothesis as in any degree more satisfactory than that of Mr. Smee. In restricting his application of physical forces, however, to phenomena confessedly material, the former has wisely kept aloof from the dangerous ground on which the latter has so boldly and ambitiously ventured; and has not laid himself open to the accusations which will doubtless be fulminated against one who reduces all mental activity to the play of voltaic currents. We could wish, as we have already said, that Dr. Wiglesworth's ingenuity and industry had been exerted on a better subject. We should recommend him to study books less, and nature more; and more especially, when he next frames an hypothesis, should we counsel him to keep it to himself until he has tested its merits by the best experiments he can devise. His physiological knowledge, though clear and explicit so far as it goes, should receive a considerable extension ere he ventures to generalize upon vital phenomena. But more particularly do we recommend him, before he next addresses himself to the public, to make himself better acquainted with the elements of physics. It will be evident to those who have followed us through our analysis of his views, that the very title of his book is a misnomer. "The law of gravity" has only this to do with muscular motion, that, on Dr. Wiglesworth's hypothesis, muscular contraction is occasioned by the pressure of the air; but this pressure is as much dependent upon its *elasticity* as upon its *weight*, for it continues when the weight of the superincumbent air is prevented from acting—as in a closed vessel with rigid walls. With the transmission of nervous force, gravitation, on Dr. Wiglesworth's own showing, has nothing whatever to do. And in cell-growth, again, it is the pressure of the air, not alone its gravitation, which is the active agent, if we are to believe Dr. Wiglesworth's explanation. This is a sufficient indication that his knowledge of physical science needs great extension.

On the other hand, we would urgently recommend Mr. Smee, either to proceed at once to the construction of a pattern man upon voltaic principles, or to go back to his electro-metallurgy. If he is successful in the former aim, we will be the first to proclaim his title to the highest pinnacle of fame; of his competency to benefit the public, by devoting his attention to the latter, he has already given evidence. One of these pursuits is undoubtedly Mr. Smee's "mission;" let him choose between them.

ART. VI.

Eruptions of the Face, Head, and Hands; with the latest Improvements in the Treatment of Diseases of the Skin. By T. H. BURGESS, M.D.—*London*, 1849. 8vo, pp. 254. With coloured Plates.

THE extreme importance of the Skin as an exhalent, absorbent, and sensitive surface, imparts to its pathology the highest degree of interest. The recent development of the anatomy of tissues, especially the minute examination of the mucous and cutaneous membranes, has done much to elucidate the nature and seat of their maladies; while the advances in pathological medicine tend to throw light on the relation of symptomatology to morbid anatomy, so as to establish far more rational views of those hitherto obscure diseases.

The classifications of former times were almost limited to physiognomical character; the subject of etiology, and especially the dependence of many of these disorders on conditions of the general system, being overlooked or erroneously viewed. Hence, instead of the maladies of the cutaneous tissues being regarded as frequently mere symptoms, signs, or indications, they were usually deemed *substantive* affections; the treatment based on such erroneous pathology being little better than a blind course along the beaten track of empiricism. The result was, of course, the prevalence of *hypotheses*, which, as Sydenham writes, "owe their origin to ostentatious vanity and idle curiosity, whence it is easy to conceive how much they must needs obstruct the improvement of physic, which is a science that depends chiefly on well-conducted experiments and close and faithful observation."

Variola, varicella, scarlatina, rubeola, as well as vaccinia and syphilis, were, of course, known to be the outward signs of a special poison in the blood; and therefore not the eruption but the erethism or fever, not the local but the systemic affection, was the object of pathological study. But eczema, impetigo, erysipelas, acne, &c., were constantly deemed local diseases, and treated accordingly. From this practical error we may readily conjecture not only how often the endeavours of the physician were thwarted, but also the perils and fatality so often following the *paling* of a rash or the healing of a crust.

Of these untoward events we now possess so copious a record, as almost to make us blush for the imperfect ministration of our art to the ills that flesh is heir to.

There has been much error even in the *ordinal* arrangement of cutaneous pathology. Vesicles and pustules have been strangely intermingled by some nosologists, and separated and rearranged by others; while the dilemma has been occasionally evaded by noting down the mere specific varieties. Thus varicella, once a vesicle, is now a pustule; while a doubtful crust will be termed eczema impetiginodes or impetigo eczematodes, according to its incipient ordinal form. The primary and secondary affections of the skin may indeed in many cases remain undiscriminated: if, in inflammatory *branning* of the scalp, vesicles be excited and form crusts, the malady, if seen first in this state, would be termed eczema; but if the *dandriff* was the object of consultation, the disease would probably be set down as pityriasis.

The word impetigo also means nothing but an *invasion* (of the skin), but Celsus has alluded to it as synonymous with psoriasis and lepra; while Cullen, in the construction of his order "impetigines," seems to imply any *sore* affection; Willan confuses it with the contagious *porrigo*, as he was pleased to term it, and the vesicles.

Any annular or circular eruption affecting the scalp, especially the *ring* herpes, has been termed ringworm, and stigmatised with the odious quality of contagion.

But the mere physiognomy of skin diseases, even a perfect diagnosis of the genera, will be abstractedly of little avail. Their combinations and sympathies must be studied with patience and discrimination, in public practice, ere correct and safe modes can be adopted for their removal; for it is often as essential to know *when* as *how* to cure.

Attentive study may often enable us to recognise and prevent the ultimate changes of these diseases; thus we induce exfoliation of the vesicles of herpes, and prevent the maturation and "pitting" of a variolous pustule.

We must not, however, forget that this outward sign may be a safety-valve of the system, on the principle of depuration or counteraction.

Angina, peritonitis, hypochondriasis, mania, have often been relieved by critical eruptions, as the raising of a vesication may transfer disease from an important to a comparatively unimportant tissue, or from a visible to a hidden portion of the body. Intermittents have been checked and cured by an attack of variola, while the sudden subsidence of eruption has often been followed by fatal disorder.

It is also constantly observed that children who have a cutaneous *oozing* or incrustation during dentition, are usually fresh coloured, and are very seldom attacked by diarrhœa. A transient vicarious influence is even observed over syphilitic eruptions; they have been *suspended* by an attack of pneumonia, and have again raged as the inflammation of the lungs subsided.

We may add, that the *successful* (?) removal of rashes, pustules, and crusts has often been speedily followed by acute ophthalmia or meningitis. From Merriman, Morton, Darwin, Copland, and our own records, we might quote many instances in proof.

It is with the impression of these truths on our mind, that we have perused the work of Dr. Burgess, who it is evident has long made the subject of our remarks his patient and attentive study, not only in England, but in France and Italy; and having enjoyed the friendship of Cazenave and the privilege of *entrée* at St. Louis, is well qualified to write on a class of diseases, the nature, causes, and treatment of which, even at the present day, and in the minds of men of much general experience, are jumbled together in very complete disorder.

The association of various eruptions with internal malady are so concisely stated that we will quote the passage on that point:

> "The eruptive fevers and the syphilides, for example, are constitutional diseases, or rather the sequelæ of constitutional diseases, and to view them in the light of special or local affections, would be to mistake their nature altogether. Eruptions of constitutional origin, however, by long standing may sometimes assume a local character. Erysipelas and acne frequently supervene in cases of derangement of the uterine functions. Strophulus is associated with the process of dentition. Urticaria, lichen urticatus, and several varieties of herpes, are often the results of a disordered condition of the digestive organs. Psoriasis and lichen agrius frequently occur during the progress of gout and urinary diseases; and the hereditary nature of certain diseases, as lepra, psoriasis, lichen, is beyond all doubt. Unless we bear in mind, in our treatment of these maladies, their intimate relation with the organic functions, we are constantly liable to serious error. If, for example, we were to look upon those critical eruptions or discharges which occur at certain periods of life as *local* diseases, and were to attempt to suppress them by topical applications, it is unnecessary to add that serious consequences would result; whereas, if they are not interfered with, they will get well as soon as the equilibrium of the system is restored. The impetiginous eczema of infants, and those eruptions that occur at the period of puberty and the turn of life, are examples of this kind. A knowledge of cutaneous diseases, in fact, implies familiarity with the general principles of pathology; and above all with the doctrine of the sympathies, with general infections, idiosyncrasies, the effects of regimen and mode of life." (p. 5.)

Dr. Burgess, as his title imports, has arranged his remarks according to the pathology of regions, and although this mode must of course be somewhat tautological, it renders the study much more simple and practical. The local peculiarities so often tend to modify the course and influences of disease, as to point to a different and often contrasted mode of treatment. So is it especially with regard to the diseases of exposed and covered surfaces, to the former of which Dr. Burgess has chiefly limited his remarks.

The diseases which Dr. Burgess describes are, Lichen, Erythema, Roseola, Herpes, Eczema, Impetigo, Acne, Sycosis, Pityriasis, Psoriasis, Lupus, Syphilis, Favus, Alopæcia, Scabies, and Pellagra; the whole of which, it will be seen, with the exception of the three specific and highly contagious diseases, Syphilis, Scabies, and Favus, may be termed symptomatic maladies.

As this is a limited and, for practical purposes, a very judicious arrangement, we do not pause to make comment on the *general* classification of M. Cazenave, which the author has appended; merely observing, that it is as concise as the subject will admit, although in this, as well as the arrangement of Rayer and in that according to tissue, of Hebra, we might point out many objections. The nosology and nomenclature of cutaneous pathology have yet to be arranged.

On the subject of general diagnosis, Dr. Burgess offers some very pertinent remarks; for it depends very essentially on our discriminating judgment whether our *methodus medendi* be safe or perilous. The difficulty of forming a correct diagnosis from a few isolated cases must be evident to all, and the rare opportunities that offer in private *general* practice of noticing the primary forms, especially of eruptive diseases, increase the dilemma.

> "When we recollect," the author observes, "the slight difference that exists between a vesicle and a pustule, in certain stages of their course, that the bulla of rupia often closely resembles the pustule of ecthyma, and, moreover, that the fluid of a vesicle may dry into a scab or incrustation, the fallacy of such a means of arriving at pathological facts and precise descriptions of disease must be sufficiently obvious. To understand thoroughly the diagnosis of the important classes of disease under consideration, it is necessary that they should pass before the eye of the student in every phase of their existence. . . . . A small herpetic ring, the vesicles of which are slightly exfoliated, situated on a perfectly round and red surface, may often be mistaken for a patch of lepra without scales. It is needless to dwell on the evil results, as regards the treatment of the patient, sure to follow an error of this kind; but happily this mistake may easily be avoided, and cannot occur to any careful observer, for the depression in the centre and the prominent border of the leprous patch may readily be distinguished from the even surface and the débris of vesicles, so characteristic of the ring of herpes. Besides, there are generally several patches of lepra present at the same time, some of which are sufficiently developed to indicate their real nature. Again, herpes circinnatus may be confounded with porrigo scutulata, the name ringworm being erroneously applied to both. However, one is a vesicular, and the other a follicular disease. The latter is of long duration, forms thick scales, is contagious, destroys the hair, when it occurs in that neighbourhood; while the former is a mild affection, lasts but a short time, never destroys the hair, and terminates by desquamation, or by insensible exfoliation. The rings of lichen circumscriptus are not much larger than those of herpes circinnatus, and hence it is sometimes difficult to distinguish one from the other. Here we must depend principally on the elementary lesion for the means of diagnosis, which in the former case is a papule, whilst in the latter it is a vesicle." (p. 9.)

These judicious remarks are followed by rules for the process of elimination adopted by M. Cazenave, for which we refer our readers to the author's Introduction.

There cannot be too much stress laid on the subject of the sympathies of the skin:

> "Thus," writes the author, "we frequently find a certain form of impetigo to be

in reality but the expression of an exaggerated lymphatic temperament; an obstinate variety of eczema, the external manifestation of functional disturbance of the apparatus of secretion, and frequently of the digestive organs; acné is commonly associated, in females, with disorder of the uterine functions; prurigo with an exalted state of the nervous system; and pemphigus with organic disease of the liver." (p. 13.)

From the correct decision, therefore, regarding *causes*, how simple and plain may be our treatment. Our rules of general pathology have already taught us how to cure those maladies of which the eruption is but a symptom; and without this we may be cruelly, ungenerously hammering away for an age on a kind spot that would be a lamp to our path, by pointing to the otherwise latent cause of the evil.

In his first section, the author refers to the frequency of eruptions on the face, and to the explanation of Bichat regarding the sensitive vascularity of the cheeks, as illustrated by blushing,—a combination of the profusion of vessels and extreme sensitiveness of nerve. "Every anatomist," he writes, "has been struck with this phenomenon, in infants particularly, in whom, if the coarse injection of the dissecting-room pass, even imperfectly, the face becomes quite black, whilst very little fluid penetrates the other parts of the cutaneous system."

In his discussion of the subject of erythema, the author very briefly hints at the eruption being "merely the reflection of disorder or irritation of the mucous surface." Of this truth we have long been convinced. Minute examination would prove, not only this *reflection*, but very often a *continuous* extension from the epithelium to the cuticle; we might almost say, from the internal or moist, to the external or dry epithelium. The proximate cause of gastric affections, especially during childhood, often consists of an erythematous or papular condition of the mucous membrane; not only causing nausea and diarrhœa, but setting up a state of erythema, and often decided fever, of a remittent type, which, if not checked and treated *pathologically*, may induce fatal marasmus, or some severe cerebral affection.

So also, with roseola (indeed, the practical distinction between these two exanthemata is often a mere hair-line), the author briefly refers to the synchronous affections of the throat and the skin: a hint quite sufficient to the reflective physician for the establishment of an appropriate practice.

Herpes has always been a prominent subject with pathologists: the term *herpetic*, like that of scorbutic, having been put down for almost every eruption which they did not understand. It has been woefully jumbled with other affections,—the result of incorrect definition and a confusion of terms. Even Underwood defines herpes as "dry, scurfy blotches," and adds, it "is by many distinguished from ringworm, but perhaps needlessly."! We might have thought that the watching of one case of each would have convinced the physician of their dissimilarity in every point—ordinal character, nature, cause, progress, and termination. The tyro might readily believe the annular form to be ringworm, merely because it *is* annular;—he will, however, soon find his error during the treatment. But this strange notion has been revived even at the present hour. Underwood wrote in the infancy of cutaneous pathology, before the classification of Willan. His error may therefore be palliated. But Biett also alludes to a *contagious* form of herpes, and Cazenave has termed ringworm "herpes tonsurans." Rayer also does not mince the matter, but terms herpes circinnatus "vesicular ringworm." But the aphoristic form in which the Dublin physician, Dr. Neligan, has adopted this error, in his 'Essay on Diseases of the Scalp,' cannot fail to astonish those who have once traced the course of these maladies: "Herpes capitis is the true ringworm of the scalp."! We do not see, we confess, why herpes should be non-contagious on the trunk, and highly infectious on the scalp: the mere matting of hair could scarcely effect this.

That papulæ or vesicles may be *excited* at the margin or in the midst of crusts and scales, by irritation or scratching, we have daily proof; but these casualties do not constitute the essence of the disease. Of course, if any physician chooses, with Rayer and Neligan, to call herpes "ringworm," we may not gainsay it: there is nothing in a mere name. But when he decides that it is the *popular* and *contagious* ringworm, we must very decidedly differ and protest, or we do not our duty.* The diagnostic marks are very lucidly laid down by Dr. Burgess.

Our author restricts his notice to one other form,—H. labialis. Indeed, there are but two really distinct forms of the disease. The annular form (circinnatus) stands alone; the other varieties are essentially the same, yet some nosologists have multiplied them very needlessly. We are induced, as a matter of curiosity, to enumerate the items of this prolix catalogue, with which Rayer heads his sections on herpes:—Herpes—Dartre—Tetter—Zona—Zoster—Shingles—Ignis sacer—Phlyctænodes—miliaris—labialis—Exanthema labialis—Hydræa febrile—preputialis—Aphthæ—Ulcuscula preputii, and Iris!

On the subject of Impetigo Dr. Burgess is concise, and his remedial hints are simple and practical. We venture, however, to differ with him and Rayer in limiting the two trivial divisions of the malady to peculiar localities. The scattered form will appear on the face as well as the "figurata." It may be well perhaps for Willan to adopt these trivial distinctions in an elementary work, which in a practical treatise are nugatory, as they are equal in degree, and do not, like the distinct and confluent variola, induce a different prognosis.† After due attention paid to systemic remedies, we agree with the author in his advocacy of the dilute hydrocyanic-acid lotion.

The various species of Acne and Sycosis are treated very judiciously, especially with regard to depletion in the acute forms. The author agrees with Devergie, Biett, and Cazenave, that the existence of a parasite in the follicle is not the essential cause of acne, but, as Simon of Berlin states, a frequent casualty.

We have long doubted the worth of the term Sycosis, and have felt disposed to consider the S. capillitii and S. menti of Willan, as at least most closely allied to Acne, altered in character by the more complex tissue or locality. We believe, however, that the favous sporule may occasionally exist in the beard as well as the scalp; and this is probably the contagious Mentagra of Gruby; as well as the disease communicated in lunatic asylums by shaving with a soiled razor, and perhaps that also which Pliny tells us was so virulent in Italy during the reign of Claudius. In the chronic, protracted, and inveterate form of these diseases, we have known much benefit derived from the arseniate of ammonia, one twenty-fourth of a grain in distilled water once or twice in a day.

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* The above was in type previously to the appearance of Dr. Neligan's somewhat angry reclamation in the Dublin Journal for August. No good purpose could be answered by our prolonging the controversy by a reply; but as Dr. Neligan is so injudicious as to impute to us a national antipathy to Irish medical literature, we may take the opportunity of informing him that some of our most valued *collaborateurs*, including the author of the very critique of which he complains, are countrymen of his own. The reviews of the works of Dr. Walshe and Mr. Rynd in our last Number will show that we have no indisposition to recognise the merits of the contributions of Irish authors to the literature of our profession.

† It is of more importance, however, to divide impetigo into the acute and asthenic. The *granular* form of the disease is almost always an indication of debility, requiring an opposite mode of treatment to the acute.

Dr. Burgess lays much stress on the distinction between the formidable condition of lupus and the syphilitic tubercle. On these remarks we may fully depend, as they are the result of careful observation at St. Louis. We have no doubt that the condition of low vitality marking the strumous diathesis is a predisposing cause of this disease, as we have witnessed cases in which all the signs of scrofula were prominent, and in which the disease has been amended by the most decided course of tonics. The last case which came under our notice was in a *child*, in the Westminster Hospital, which proceeded to a complete cure, (under the penalty, however, of an awful chasm of the mouth from exfoliation,) by the *copious* exhibition of port wine and nutritious diet, the parts being assiduously washed with dilute nitric acid. Donovan's solution of iodine, arsenic, and mercury is the most valuable remedy of a specific nature in those cases attended by great prostration.

The chapter on Syphilides, which contains the essence of Cazenave's treatise on syphilis, should be studied attentively. We must be aware of its high importance, especially with regard to diagnosis, when we contemplate the five plates of Rayer containing nearly eighty forms of the disease. The nature of the malady once decided, the treatment by the iodide of mercury is very simple.

We will not endeavour to abridge this very practical chapter, but merely transcribe its judicious conclusion.

> "The practitioner must always be guided in the selection of his remedies, by the nature and form of the eruption, its duration, the particular constitution of the patient, and the anterior treatment. As a general rule the acids are indicated in the semi-acute varieties, especially in the exanthemata, vesiculæ, and in one or two forms of lichen. Sudorifics are useful in the pustular, but more particularly in the scaly eruptions. Whatever may be the treatment indicated, it should always be preceded for a certain time by preparatory measures, both hygienic and medicinal. Biett prescribed opium in small doses, for a week or two before administering the iodides of mercury, and generally with good effect. During the employment of the mercurial preparations, he prescribed the occasional use of the warm bath and sudorifics.
>
> "It is indispensably necessary that the treatment of the syphilides should be continued for some time after the eruption has disappeared. If the eruption was mild, of short duration, and yielded easily to the treatment, say in the course of a month or six weeks, it ought to be continued for a month longer, at the same time gradually diminishing the dose. It is sometimes necessary to discontinue the medicine three different times before we have done. It is a singular coincidence, and worthy of being remembered, that a patient who may have borne the mercurial preparations well for several months without intermission, will suddenly, and after a discontinuance of the treatment for a few days, show an almost invincible intolerance of these remedies; this is a sure indication that the treatment is complete."

With M. Cazenave the bichloride of mercury, about half a grain per day, is the favorite form. The protoiodide (with liquorice powder) is, we think, more efficacious in inveterate cases, pushed even to four grains per day in extreme conditions.

In papulæ, rashes, and vesicles, the acids only are given, or combined with the mercurials. Before the course, warm bathing, and opium, and free purgation are had recourse to.

Dr. Burgess considers "crusta lactea" a form of impetigo. Underwood termed it a "rash," and Willan, porrigo. Our author is nearly right: but the incipient form is an *opake* vesicle, not a pustule. Rayer says it is both.

We here again approach the subject of the sporulous affections of the scalp.—The author retains the term "ringworm;" indeed it is so conventional a word that we cannot venture to discard it. Yet we really believe that if five very experienced pathologists were called on to define what they meant by the term, no two of them would very closely coincide. The definition of "favus confertus" of our author and of Rayer differ in this, that the one identifies it with the "porrigo scutulata" of Willan, while the other consider it a variety of "porrigo lupinosa" of the same writer, or what is popularly termed "scald-head." Now the physiognomical characters of these are totally different, and require different modes of treatment. That rough papillary state of the skin, with tawny circular patches, to which Dr. Burgess justly confines the term ringworm, is often combined in consecutive stages with pustules and crusts; but if it be treated as a mere pustular disease, the mode will assuredly fail. The term scald-head, as it is popularly received, is equally vague with that of ringworm. Underwood evidently blinks the question: he *mentions* it, but does not *describe* it. He evidently did not know what his tinea or scald-head meant, or he would not have written so childishly about *little* ulcers found in *little* glands, which may be "safely healed up after being thoroughly cleansed."!

Dr. Burgess knows better, and he has come to the conclusion that the most efficacious way to cure ringworm and favus, is by a complete alteration of the cutaneous action: his favorite mode being by the vapour of iodine and sulphur. The variety of systemic condition is, however, a subject of vital importance. Of this we have within a few days been convinced, on our visit to an asylum at Hampstead, to inspect the heads of about a dozen girls. The affections, though from one source, were varied in character according to constitution; in some requiring purgation, in others tonics: in the robust it was comparatively tractable, in the weakly most stubborn and unyielding.

The honeycomb scab is too peculiar a crust to be mistaken. Dr. Burgess's description is more concise than that of Rayer; and we believe, if the decided method which he recommends had been adopted in our large public schools some years ago, the public would have heard little of this *opprobrium medicorum.* Our author differs alike from Willan, Bateman, Plumbe, and Rayer, who term the incipient form a pustule; from Bennett, who calls it a tubercle, and from Gruby and Schönlein, who affirm it to be a vegetable parasite. He calls it an "inflammation producing an *heterologous* secretion within a hair-follicle." This peculiar secretion the author considers to form a soil favorable to the growth of the sporules, as paste or cheese or wet wood is of the cryptogamic parasites. We do not presume in this comment to determine this dilemma of opinions, but merely observe that the inflammatory action must be slight, as an areola is seldom seen around the honeycomb crusts.

The treatment, however, is a point of great interest.—The French empiric, Mahon, first discarded the barbarity of the pitch cap and other cruel depilatories. His nostrum seems to have consisted chiefly of lime and carbonate of potass. At St. Louis, at the present day, the favorite applications consist of subcarbonate or sulphuret of potass, two drachms to a pint of water, and the ointment of ioduret of sulphur, about a scruple to an ounce of lard. Dr. Burgess still prefers the vapour of iodine and sulphur. The author also recounts Professor Hebra's plan of softening and removing the crusts, and the subsequent applications of deuto-chloruret of mercury, nitrate of silver or lime, &c.

In Psoriasis and other Squamæ assuming an asthenic type, Dr. Burgess recommends phosphorus, in the forms of pills and ointment.

The account which the author gives of the Acarus.scabiei is very interesting. The experiments of Renucci and his followers have demonstrated the reality of that which had been *conjectured* even by Aristotle, and firmly believed by Avenzoar in 1179. Dr. Burgess has witnessed experiments by Dr. Bourginon at St. Louis, which confers a high degree of truthfulness on his comments.

He is, also, of the few who have seen the loathsome Pellagra in its own selected region, Lombardy. Since his book appeared, Dr. Burgess has published a more elaborate account of the disease, which will be read with great interest.

At the end of his work the author has appended a very concise and practical pharmacopœia, which will be found amply sufficient to compass the whole catalogue of diseases of the skin; well calculated indeed to refresh and remind the experienced dermatologist. The plates contain sketches of the anatomy of the cutaneous tissues, from Breschet and Roussel's work; and four coloured delineations of prominent diseases, of which three are copied from Cazenave. They are too small, but have the merit of fidelity.

With this slight reservation, we repeat our very favorable opinion of the conciseness and practical merits of Dr. Burgess's book;—the highest eulogium that can be passed on any labour that aims at the alleviation of a troublesome class of maladies.

Art. VII.

A Treatise on the Pathology, Diagnosis, and Treatment of Neuroma. By Robert W. Smith, m.d. t.c.d., m.r.i.a., Fellow of the Royal College of Surgeons in Ireland, Lecturer on Surgery at the Richmond Hospital School of Medicine, Secretary to the Pathological Society of Dublin, Fellow of the Royal Medico-Chirurgical Society of London, &c. &c.—*Dublin*, 1849. Folio, pp. 28. Plates XV.

In these days of Students' Manuals and duodecimo editions, the prospect of a magnificent folio monograph, such a one as Hunter, or Home, or Cooper would have loved, is as cheering to the reviewer's eye as the first glimpse of an oasis to the weary traveller in the desert. When, too, as in the case before us, we recognise in a familiar name on the title-page a guarantee for the quality of the literary banquet, and observe that the work is published by subscription, which banishes our fears for the caterer's pocket, we sit down to the enjoyment of the treat with a whetted appetite, the rarity of whose occurrence only makes it the more keen.

The "getting-up" of this volume is perfect. Printed on the finest elephant paper, in a clear bold type, and illustrated by fifteeen large plates, admirably drawn on stone, it will form an honorable and lasting monument to the taste and skill of all concerned in its production. Its costliness will of course prevent its extensive circulation; and on this account, as well as because the subject to which it relates is little understood, and also because a treatise from the pen of Dr. Smith is sure to deserve and command attention, we propose to submit to our readers a digest of its contents.

The subject of Neuroma is confessedly difficult and obscure; any attempt, therefore, to elucidate it deserves our thanks, and these we cordially tender to Dr. Smith; but at the same time, while giving to him his full meed of praise, we must, even at the risk of appearing ungrateful, confess to a little disappointment. It may seem unreasonable to quarrel with the quantum provided for us, when the difficulty of procuring even that quantum is so great; still, with every disposition to be satisfied, we must, like Mrs. Squeers's hungry scholars, acknowledge that we have only had enough to make us wish for more, and not by any means sufficient to satisfy the craving of our hungry nature. There is a deficiency in the treatise of what we should call "*exact information;*" the literature of the subject certainly seems nearly exhausted; and of this we have no complaint to make, except as regards the absence of any mention of Rokitansky; it is rather to Dr. Smith's own investigations that we refer. He has met with unusual facilities for the study of the subject; and it does not seem to us that he has profited by them as he might.

Very little has been made of that important part of the investigation, the microscopical characters of these growths. With the exception of a loose and meagre account of the appearances under the microscope, of a section of a neuromatous tumour which fell under the author's own observation, nothing whatever is said upon the subject. We the more regret this, because if Dr. Smith felt himself incompetent to the investigation—and it is no reflection upon him to suppose him so—there are plenty of younger men in Dublin who are excellent microscopists, and whose labours would materially have enriched this portion of his work. Again, the "comparative" part of the subject has been left wholly untouched. Are the lower animals altogether exempt from this disease? or could nothing be learned from the study of it in them? With respect to a publication of this kind, we have a right to ask such questions. They are blemishes in a great work that must be permanent ones; for it is not likely that a second edition of so large and expensive a treatise will be called for. Far more agreeable to our own feelings would it be to thank Dr. Smith for his beautiful work, and to commend the matter as worthy of the dress in which it is presented; but conscientiously feeling as we do, we shall not shrink from the discharge of our duty to the readers of this Review, because that duty is in some respects a disagreeable one.

Tumours implicating and connected with nerves have long been recognised by pathologists; and, according to one opinion, were referred to by Hippocrates and Galen under the name of ganglions. This, however, is probably incorrect; and, at all events, it was not until the publication of M. Odier's 'Manuel de Méd. Pratique,' in 1803, that they received the name, by which they are now known, of Neuroma. They may be divided into idiopathic and traumatic: those which are spontaneous, and for whose appearance no external origin can be assigned; and those which follow injuries to nerves, as wounds made in amputation.

Neuromatous tumours are generally ovoidal, and have their long axis corresponding with the direction of the nerve upon which they are seated; they may often be moved from side to side without much inconvenience. Extreme suffering is occasioned when this is attempted in the longitudinal direction. They never contract adhesions to the integuments, unless from continued pressure, nor is the skin over them discoloured; and they are scarcely ever spontaneously cured. They are subject to great variety both in size and number—occasionally no larger than a grain of mustard-seed, they have been met with of the dimensions of a large melon. Frequently single, affecting one nerve only, they are at times to be counted by the hundred, and affect all parts of the nervous system alike.

The solitary neuroma is nearly always the source of acute agony to the patient, and is most often of the traumatic variety. When kept at rest, a tingling or numbing sensation may alone indicate its presence; but, influenced by changes in the state of the atmosphere, disorders of digestion, and so forth, it is liable to exacerbation of pain terrible to witness, as severe as the acutest neuralgia, and comparable only to an electric shock darting through the trunk and branches of the affected nerve. It has been noticed that in dry weather the pain sometimes ceases altogether. Idiopathic neuroma is generally met with as a single painful tumour; yet, occasionally, these growths are numerous on the same nerve, and in such instances are often free from pain. They are relatively much less frequent in the nerves of the ganglionic than in those of the spinal system. Both kinds of neuroma may be solid throughout, or may contain a cyst filled with fluid; and it has been remarked of both of them, that when the trunk of the affected nerve is compressed above the tumour, the pain occasionally ceases entirely.

There is no ground for the assertion of Bayle, Cayol, and others, that neuromatous tumours are of a scirrhous or malignant character. They never involve neighbouring parts, nor affect the lymphatic glands; and do not return when once removed. It is necessary, however, carefully to distinguish those cases in which malignant diseases implicate nerves, and which are not examples of neuroma at all. Of the causes of this disease we know little; mechanical irritation of a nerve by the presence of a foreign body, pressure, blows, the division or wounding of nerves by cutting instruments, and such like injuries, may be assigned as influencing the production of traumatic neuromas; but concerning the origin of the idiopathic variety we are wholly ignorant. It seems to us better to confess this at once, than to gloss over our want of knowledge by such terms as inflammation or the rheumatic diathesis. Dr. Smith has adopted as his motto a statement of Cruveilhier's, which will probably long remain true—"*L'anatomie pathologique des nerfs est encore à faire.*"

However great may be the diversity in the causes of neuromatous growths, their anatomical characters are remarkably uniform. It has been before stated that they are either solid or cystiform; it remains to consider each of these varieties, and for this purpose we transcribe entire Dr. Smith's description of them:

"The solid idiopathic neuroma originates either in the cellular structure which connects the neurilemma or external sheath to the trunk of the nerve, or within one of the smaller sheaths, by which each of the fibres, the aggregate of which constitutes the nervous trunk, is enveloped. In these two cases the disposition of the nervous fibres upon the surface of the tumour, as also the coverings of the latter, are somewhat different. In the former, when the tumour originates in the neurilemma or its connecting cellular structure, we usually find that the nervous fibres, although flattened and increased in breadth, are not separated from one another as widely as in the latter, supposing the tumour to be of the same size in each case: they are more confined to one surface of the tumour, generally its deep aspect; but when the neuroma originates in one of the central sheaths, it presses, as it grows, more equally upon the surrounding fibres which are, in such cases, found more widely dispersed over the surface of the morbid growth. Again, in the latter case, the tumour, when large, is generally invested by a greater number of coverings than in the former; its immediate capsule is the sheath of the fibre where it has originated; external to this we find a thin investment, formed by the condensation of the cellular tissue, which constitutes the general sheath to the trunk of the nerve; this sheath or external neurilemma forms the most superficial covering, with the exception, of course, of the subcutaneous cellular texture. In some cases, the coverings of the tumour are easily separated from one another, and with very little dissection can easily be shown to be distinct membranes; but in others they are intimately united, forming a dense capsule of considerable thickness. The degree of adhesion of the tumour to its immediate investing membrane also varies considerably in different instances, the connexion being sometimes so close, that it is impossible to remove the tumour from its sac without the aid of the scalpel; while in other instances, when the sac is opened, the tumour can without difficulty be detached by the finger from all its connexions.

"I have never seen the capsule undergo any cartilaginous or osseous transformation; it is smooth and shining externally, and its internal surface presents in general a glistening aspect, more especially when it is but loosely connected to the tumour. The oval or oblong form, which so large a proportion of neuromatous tumours acquire as they increase in size, is to be ascribed to the circumstance of their being covered by the neurilemma, which admits more readily of their growth in the direction of the trunk of the nerve. A section of a neuroma usually discloses an exceedingly dense homogeneous texture; the surface is smooth, of a grayish-white colour, and frequently the elasticity of the structure is such that the cut surface becomes convex; it seldom presents the same shade of colour as the nervous tissue; its texture is essentially fibrous, or fibro-cellular, but so exceeding close and dense, that the unassisted eye can scarcely even detect the course or arrangement of the fibres. In the majority of cases, the tumour is solid throughout, and everywhere presents the same appearance, and an equal degree of density; indeed the uniformity of the aspect of the interior of neuromatous tumours is not one of their least remarkable features.

"It has been stated by Dupuytren that one of the characters which distinguish neuroma from the fibro-cellular encysted tumour (or painful subcutaneous tubercle of English authors) is, that in the former there is a cavity containing fluid, the latter being always solid throughout. This I believe to be by far too general an assertion, and my experience would lead me to say that it is only in exceedingly large tumours that cavities are occasionally found; as neuroma may even attain the size of an infant's head, without any cavity being formed in it. The existence of a cavity containing fluid in the smaller neuromatous tumours, I believe to be the exception. When, however, the tumour has attained a very large size, cavities occasionally form in its interior, they vary considerably in their dimensions, do not communicate with one another, and are lined by a distinct shining membrane, which, sometimes villous, is generally smooth. Some contain a serous fluid; in others the contents resemble rather synovia or albumen, or may present the characters of purulent matter; others, again, are filled with fibrine; and finally, some are empty.

"The lining membrane, generally pale, is sometimes very vascular; but I have never seen in any of the cavities of neuromatous tumours fluid or coagulated blood, lardaceous or medullary matter, or any of the substances which are found in malignant tumours; nor have I ever known the neuroma itself, no matter how long it may have existed, or to whatever size it may have attained, assume the external character or acquire the internal structure of cancerous or encephaloid growths. The trunk of the nerve, immediately above and below the tumour, is normal in appearance, and the intermediate portion is merely altered in form, and thrown out of its usual course; it either passes as a single trunk along the posterior or lateral surface of the tumour, or else its separated fibres are dispersed over the surface of the neuroma. In a few instances I have seen some of the nervous filaments enter the superior extremity of the tumour, although I am unable to trace them through it; but I have not yet met with any example of the passage of the entire trunk of the nerve through the neuroma, as is stated to happen occasionally: 'Mox nervus cum toto trunco suo in uno latere tumorem intrat, altero autem ex eo rursum egreditur, mox complures rami et intrant et exeunt.' (Knoblauch.) When the neuroma is of large size, it is exceedingly difficult to trace all the separated fibrillæ joining again into a trunk below the tumour, and in some instances it is quite impossible to follow them with the scalpel; but if the tumour and the portion of the nerve connected with it be left for some time in nitric acid, it will become manifest that there is no interruption of continuity in the fibrillæ. It should, however, be mentioned that in the first case recorded by Aronssohn, the author states that the continuity of the nervous fibres was destroyed; perhaps if the preparation had been macerated in nitric acid, it would have been found that this opinion had been hastily formed." (pp. 5 et seq.)

Dr. Smith relates several instances of the cystic variety of neuroma.

Owen Nolan, an old soldier, who died of fever in the Hardwicke Hospital, had, consequent upon an injury to the elbow, a swelling a little above the internal condyle of the humerus, which had existed for a considerable period. In general it was not painful, becoming exceedingly so, however, when touched. After death, the growth was found to be a neuroma of the ulnar nerve, an inch and a half long, exhibiting upon section a cyst filled with a transparent albuminous fluid. It had originated within the neurilemmatous sheath, and the filaments of the nerve were separated from each other, spread out over the surface of the cyst, and reunited below the tumour to form a trunk.

Occasionally, however, the cystic neuroma is not painful, as the following case related by M. Bertrand will prove.

An old man had a tumour as large as a hen's egg on the inner side of the right arm. It presented a sensation of obscure fluctuation, occasioned no inconvenience whatever, and was not particularly noticed until after death. A section of it looked very like an aneurism, for out of it flowed bloody serum mixed with clots of blood; but careful dissection demonstrated that it was a cyst developed among the filaments of the brachial nerve.

It recently occurred to us to witness the removal of a neuroma from the arm of a female, to whom it had long been the cause of extreme suffering.

The growth, not larger than a common pea, presented itself between the heads of the triceps muscle, and was situated on the musculo-spiral nerve, the fibres of which it separated and pushed aside. It was of a reddish-brown colour; of compact structure externally, but softer in the centre, which was composed of a soft light-coloured mass. The microscope showed it to consist of cytoblasts in various stages of development towards fibrous structure, interspersed with oil-globules and some granular matter.

This appears to be the most appropriate place to append some observations which we have extracted from Rokitansky, to whom, as we have before observed, Dr. Smith makes no reference.

Rokitansky, in his pithy way, remarks that cysts are sometimes found in the trunks of very large nerves, and from the condition in which they are developed, it is clear they perform the part of a bursa. With the exception of cancer, neuroma is the most frequent morbid growth which occurs in the peripheral nerves, and cancer is very often only a secondary affection supervening on the contiguity of the nerve to other organs. The tumours are interspersed amongst the fasciculi of the nerve, and interwoven with their neurilemmatous sheath. It is important and worthy of observation, in reference to the symptoms to which it gives rise, and to those which may be set up by experiments upon it, that, as a general rule, neuroma is never developed in the *centre of a nerve*, but *on its side*, so that only a small portion of the nervous fasciculi are displaced, spread abroad, and stretched over the neuroma; while the greater mass of the nerve remains on the side uninjured, and with its fibres in connexion with each other.

The diagnosis of neuroma is generally sufficiently easy: "The extraordinary sensibility of the tumour, the peculiar electric character of the pain, its terrible severity, paroxysmal type, and extension along the trunk and branches of the nerve," are symptoms alone sufficiently characteristic of the disease.

With respect to the treatment, it may be observed that every measure, short of excising the disease, has been wholly unsuccessful. This may be accomplished in three ways: in the first, the tumour is cut away, together with the piece of nerve to which it is attached; in the second, the tumour is dissected out of the mass of nerve-fibres amongst which it has grown; and there still remains the severe measure of amputation, when the disease is situated in either of the extremities. The first-named operation, that of removing a portion of the nerve, together with the morbid growth, is in general a safe and practicable proceeding. No fear of any evil result need be entertained; for there is ample evidence to prove that several inches of large nerves may be removed without danger; and frequently both sensation and the power of voluntary motion return after the lapse of a few months, a sensation of remarkable coldness only remaining in all the parts supplied by the affected nerve. Reich removed four inches of the ulnar nerve; a slight degree of insensibility in the little finger being the only permanent effect of the operation. A similar favorable result has followed the removal of portions of the median and tibial nerves. Malagodi cut away a portion of the sciatic nerve in a case of neuralgia, and at the end of a year the functions of the limb were perfectly restored.

Indeed the restoration of the functions of a divided nerve is a fact painfully evident in cases where the division has been performed for the cure of neuralgia; and to the veterinary surgeons, who frequently practise neurotomy, it is often a source of great annoyance.

From a number of cases in which this operation has been performed, we select one or two related by Dr. Smith.

Mr. Hunter and Sir Everard Home removed a neuroma which had existed for several years in a young lady. It was as large as a pullet's egg, and situate about the middle and on the outer side of the right arm. Three inches of the musculo-cutaneous nerve were taken away along with the tumour, and produced the effect of paralysing the thumb and forefinger, and causing desquamation of the cuticle. The narrative is silent as to the final result of the case.—The case operated on by Reich, to which allusion has been already made, occurred in a gentleman who had been troubled with it for upwards of thirty years, having followed a severe blow with a stone upon the right elbow. At the time of the operation it was two inches long and one broad, slightly elevated about the surface, and exquisitely painful to the touch. The patient's sufferings during the operation were dreadful, but ceased immediately afterwards, never to return; and in less than a year the condition of the limb was as perfect as it had ever been.

The second operation alluded to, that of dissecting out the tumour from amongst the nervous filaments in which it is imbedded, has been rarely practised, and without encouraging results. Sir Everard Home extirpated one of these tumours from the axilla of a Frenchman, which was as large as two pullet's eggs. He relates that upon laying bare the growth, he found it to terminate in a white nervous cord, and to be covered by a membrane, which on division permitted it to be dissected out.

> "The tumour, of a yellowish-white colour, was about three inches and a half in length, and two in thickness; it was of an oval form, and when cut through, was found to consist of a fibrous structure of great density. Towards the surface the texture presented a radiated appearance." (p. 9.)

The patient died from the effects of the operation, and the post-mortem examination is thus described:

> "Upon examining the part after death, the tumour was found to have been encased in one of the large nerves of the axillary plexus; the principal portion of the nerve passed along the posterior surface of the tumour. The cyst which had surrounded it was contracted, and more than four times thicker than at the time of the operation. In consequence of having been inflamed, the cavity was lined with lymph, and almost filled with coagulated blood, as suppuration had not completely taken place. The inflammation and swelling had extended some way into the surrounding parts, which were consolidated into one mass. The other parts of the body were found in a natural state; so that Sir Everard remarks, there was no evident cause of death but what arose from a considerable degree of inflammation upon the substance of a large nerve for three inches in length, which also affected the other nerves of the plexus. A similar tumour had formed in one of the other nerves, but was much smaller in size. It lay directly between the axillary artery and the tumour which had been removed." (p. 9.)

A case of neuroma of the median nerve, in which the extirpation of the tumour was followed by a fatal inflammation of the nerve, has also been recorded by Aronssohn. It happened, however, to Velpeau successfully to extirpate a tumour developed in the substance of the sciatic nerve of a lady thirty years old, which was as large as the head of a newborn child, and having originated without apparent cause, had existed for several years.

The severe measure of amputation has not unfrequently been resorted to in the treatment of this disease. Dr. Smith relates at length the particulars of a case in which Chelius amputated the leg of a female for a tumour in the ham which was of a neuromatous character. It extended from the popliteal space nearly half way up the back part of the thigh, was the seat of intolerable pain, and had resisted every variety of treatment which had been employed for several years in the endeavour to disperse it.

> "The tumour, which was of about the size of a pine-apple, was found to be a neuroma connected with the sciatic nerve, which, along with the femoral artery and vein, corresponded to its anterior or deep surface; and from its lower extremity proceeded the posterior tibial nerve and several small branches, destined for the gastrocnemius muscle and the integuments, but the peroneal and communicating or external saphenus arose above the tumour, and ran along its external surface, the fibres of the peroneal being separated from each other where the nerve crossed the middle third of the tumour, to which it was connected by a loose cellular structure. The tumour, though everywhere solid, was not of the same consistence throughout; in some parts, especially below, the feeling of elasticity was so great, that it might readily have been supposed that in these situations fluid existed; other portions possessed nearly the firmness of cartilage. The investment of the tumour consisted of an external capsule, seemingly formed by the condensation of the surrounding cellular tissue, and beneath this of a membranous expansion continuous with and apparently composed of the distended sheath of the sciatic nerve. A section of the morbid growth exhibited a lobulated structure, of different degrees of consistence, but nowhere presenting any of the characters of malignant disease." (p. 3.)

Several similar cases are recorded; among others, one in which M. Louis amputated the arm for neuroma of the median nerve.

From the examination of these and similar facts, Dr. Smith concludes that the first and the last of these operations, viz. excision of a portion of the nervous trunk and amputation, can be performed in almost every instance with success; and with respect to the remaining one, he remarks, "that there may be cases in which it may be practicable, and perhaps prudent, without sacrificing the nervous trunk by division, to dissect out the tumour from the nervous fibrillæ which surround it."

Having thus described the general characters of neuromatous tumours, their varieties, pathology, and treatment, our author proceeds to consider each class rather more in detail.

He commences with giving an account of cases in which these morbid growths have existed in considerable numbers in the same individual, including two remarkable examples which fell under his own observation.

Occasionally throughout the nervous system, alike upon the largest trunk and the smallest peripheral nerve, implicating both the par vagum and the phrenic, and not excepting the sympathetic system, there are developed tumours in vast numbers, which occasion neither pain, lesion of innervation, nor any other inconvenience. These growths were considered by Knoblauch as accessory ganglia, indicating an original vice of conformation—"Vitia primæ conformationis habendi, et vera ganglia sunt;" but this opinion is almost certainly an erroneous one. Two very remarkable examples of this form of neuroma are related by Schiffner, in the fourth and sixth volumes of the 'Oestreich. Med. Jahrbücher.' They occurred in brothers, cretins afflicted with the worst form of that disease. A vast number of the principal cerebro-spinal nerves all over the body were enlarged, and had tumours developed in their course. In one of the two cases, the superior cervical ganglion of the right side, and the inferior ganglion on the left, had not escaped the disease; and in the other case the sympathetic nerve was also involved. There is another remarkable example of the affection related by Knoblauch, as occurring in the practice of Bischoff, which we pass over to arrive at the cases which came under Dr. Smith's notice, and to which most of the lithograph plates refer.

We extract entire the first of these cases.

"John M'Cann, 35 years of age, was admitted into the Richmond Hospital, under the care of Dr. Hutton, in 1840, having a large tumour on the right side of the neck, of a globular form, and equal to a moderate-sized cocoa-nut in magnitude; it extended from the mastoid process to within a short distance of the sterno-clavicular articulation. It presented an uniform surface, and admitted of being moved freely in the transverse direction, but could neither be pushed upwards nor drawn downwards; the external jugular vein grooved its surface, the integuments did not adhere to it, nor (although it obviously extended deep into the neck) did it appear to have contracted a close adhesion to any important part; the larynx was not displaced, nor was there any interference with the functions of respiration or deglutition. It was solid throughout, and had existed for upwards of fifteen years, but had never been painful, nor was it now (although of so large a size) the source of much inconvenience to the patient, whose general health was not impaired.

"A second tumour, about as large as a walnut, existed underneath the left side of the tongue; it likewise was solid, and free from pain, but its presence was the source of much annoyance to the patient when eating. He could not, with any degree of accuracy, state at what period the tumour commenced to form, being unaware of its existence until it had attained such a size as to interfere with the motions of the tongue. In consequence of the size of the tumour in the neck, and its relations to the larynx and great vessels and nerves, as well as from an apprehension of its being of a malignant character (notwithstanding its slow growth and freedom from pain), it was not deemed advisable to attempt its removal, more especially as the existence of the sublingual tumour appeared to confirm the opinion which had been formed respecting the nature of the disease. The patient, therefore, left the hospital shortly after he had been admitted, and resumed his business in the country,—that of a cattle-driver, an occupation which exposed him to the inclemency of the weather, not only during the day, but frequently also in the night-time.

"In 1843 he was again taken into the hospital, having, upon the day previous to his admission, been found by Dr. Adrien, lying upon the side of the road, and complaining of pain in the left hip. Upon examining the part, a large solid tumour was discovered upon the back of the thigh, which the man stated had been growing for nearly two years. It extended from the fold of the nates to the inferior third of the thigh, and exceeded in magnitude the head of the patient; it admitted of being moved from side to side, and, although more prominent in some parts than in others, its general surface was smooth. Several large veins ramified beneath the integuments, which were not adherent to the tumour. The patient suffered no acute pain, the chief inconvenience of which he complained being referable to the existence of so large a tumour in such a situation.

"His general health had, however, undergone a material alteration since the period of his first admission into the hospital; he was now pale and greatly emaciated. He had no relish for his food, nor did he sleep at night; yet he made no complaint of any particular distress, nor could he assign the cause of his restlessness; he had neither diarrhœa nor perspiration; his pulse was quick and weak, but respiration was undisturbed. In this condition he lingered for a few months, generally pining and wasting away, and, towards the end of the year, died with hectic symptoms and without pain, but emaciated in the last degree. The tumour in the neck had neither enlarged nor undergone any alteration since 1840, the date of his first admission, nor had the inconvenience arising from the sublingual tumour increased. It was the opinion of all who saw the case, that this immense tumour on the back of the thigh was of a malignant character, and its appearance was looked upon as establishing the correctness of the diagnosis which had been originally formed regarding the nature of the cervical tumour.

"Upon the day succeeding that upon which the patient died, I made a careful examination of the body. Independently of the tumours that have been already mentioned, several others were now discovered, which had escaped notice during life, although most probably some of them had existed when the man was first admitted into the hospital. The largest was situated upon the posterior part of the right thigh, immediately beneath the lower margin of the glutæus maximus muscle. It was of an oblong form, and considerably larger than a lemon. There was one upon the outer side of the right arm, near its centre, of the size of a pigeon's egg, and another upon the point of the right fore-arm, immediately above the carpus, nearly as large as a hen's egg; they all admitted of being moved in a lateral direction. The intercostal spaces upon each side, as well as the abdominal parietes and right inguinal region, presented numerous tumours about the size of large peas.

"Upon laying open the cavity of the abdomen, the first object which attracted my attention was a white, solid, oblong tumour, situated in the right iliac fossa, between the iliacus internus muscle and the outer margin of the psoas magnus. It was three inches and a half in length, and one in breadth.

"It was connected with the anterior crural nerve, several filaments of which, separated from one another, were opened out upon its anterior surface; these filaments were themselves enlarged, and presented numerous smaller tumours.

"Upon tracing the nerve upwards, it was found that all the branches entering into the composition of the lumbar plexus were greatly increased in size, some of them being nearly three quarters of an inch in diameter; this enlargement commenced where the nerve issued from the intervertebral foramina. The anterior crural nerve measured three quarters of an inch in diameter from the lower extremity of the tumour down to its point of exit from the pelvis. Three tumours, each as large as an almond, existed upon the pelvic portion of the musculo-cutaneous or external cutaneous branch of the plexus, and the trunk of the nerve in the interspace was as large as that of the ulnar in its normal state at the elbow. The anterior division of the nerve was hypertrophied, and studded with tumours throughout the whole of its course along the anterior and external region of the thigh. The branches of the anterior crural presented similar appearances; the long saphenous nerve in particular being crowded with tumours from its origin to the foot. There existed upon the anterior and lateral regions of the right lower extremity upwards of 150 tumours. The nerves of the front of the left lower extremity were similarly affected, and presented, from the lumbar plexus to the knee, more than 130 tumours. They were all of an oblong shape, the smallest being about the size of grape-stones, while the largest exceeded an almond in magnitude. Upon the surface of many of them delicate nervous filaments were distinctly visible, running from above downwards.

"The right sciatic nerve presented, at the lower margin of the glutæus maximus, an oval tumour, five inches in length, and three and a half in breadth; the principal portion of the nerve lay upon its anterior surface, but numerous filaments, widely separated from each other, traversed its lateral aspect; it was inclosed in a capsule formed by the distended neurilemma. From the lower extremity of the tumour down to the popliteal space, the trunk of the nerve varied from three quarters of an inch to an inch and a half in diameter, and each of its component fibres presented a series of oval or oblong tumours, several of which were as large as an almond. The anterior and posterior tibial nerves were in like manner studded with small tumours. The total number discovered upon the right sciatic and its branches exceeded seventy.

"It was, however, upon the left sciatic that the most remarkable example of neuroma hitherto recorded was observed. One immense tumour occupied the posterior surface of the nerve, from the lower margin of the glutæus maximus to within four or five inches of the centre of the popliteal space. Its vertical diameter was eleven inches, and its transverse ten. The extent of its surface from above downwards measured fifteen inches, and from side to side one foot and a half. A dense capsule, formed by the neurilemma, invested its entire surface, to which, however, it did not adhere very closely, except in a few situations, where the connexion was intimate.

"The portion of the trunk of the nerve, which corresponded to the anterior aspect of the tumour, was spread out into a vast number of flattened fibres, which, following the convexity of the neuroma, formed a series of curved lines, diverging above, and at the lower extremity of the tumour reassembling to form the nervous trunk; these separated and flattened fibres were themselves the seat of more than fifty tumours, the largest of which was about the size of a grape, and upon the surface of some of them numerous fibrillæ could be traced. The trunk of the sciatic, at its origin from the sacral plexus, and for some distance below it, measured an inch and a half in diameter, and several oblong tumours were connected with the enlarged fibres. At the lower part of the popliteal space, there was found a tumour of the form and size of a lemon, connected by its deep or anterior aspect to the posterior tibial nerve, but the external popliteal and several other branches likewise crossed its surface; they presented a plexiform arrangement, and were all enlarged, some of them being equal to the branches of the sacral plexus in magnitude. The nerves of the leg were also of immense size, and covered with tumours throughout the entire of their course, from the popliteal space to the feet. The number of tumours which existed upon the left sciatic and its branches exceeded 200.

"*Nerves of the right upper extremity.* The nervous trunks which enter into the composition of the right brachial plexus commenced to enlarge immediately upon emerging from the intervertebral foramina, more especially the fifth and sixth cervical, to the trunk formed by the union of which there adhered a circular tumour, larger than a cherry; the neurilemma formed for it a loosely adherent capsule, the internal surface of which was smooth and glistening; the plexus itself, before giving origin to the nerves of the arm, presented several oblong tumours, which it was difficult to separate from each other. A perfectly oval tumour, as large as a hen's egg, sprung from the external root of the median nerve, which was flat, like a ribbon, and its fibres separated from one another; this tumour was in close relation to the brachial artery, and was inclosed in a dense, smooth capsule, continuous with the sheath of the nerve. The entire of the remainder of the median nerve presented numerous oblong swellings of various sizes, the most considerable of which was placed immediately above the carpus; it was pear-shaped, two inches and a half in length, and one and a half in breadth at its upper or widest part; it lay upon the front of the nerve, the separated fibres of which, as they ran over its surface, formed a series of curved lines, arranged like the divisions of a melon. The external and internal cutaneous, the ulnar, musculo-spiral, and circumflex nerves were all, in like manner, covered with tumours, more especially the circumflex, which presented one continuous mass of neuromatous swellings throughout the whole of the axillary portion of its course. Upon the musculo-spiral, besides numerous small tumours, there existed two of considerable size, one of which, as large as a grape, was situated near where the nerve reaches the external side of the humerus, while the other, which was of the size of a pigeon's egg, was placed near the elbow, between the brachialis anticus and the supinator longus. Upwards of 100 tumours were counted upon the nerves of the right upper extremity.

"*Nerves of the left upper extremity.* The branches joining the left brachial plexus were similarly affected, and presented bulbous enlargements in their course across the neck. A pyriform tumour, nearly four inches in length, and two and a half in breadth, was connected with the lower and posterior part of the plexus. It involved the common origin of the musculo-spiral and circumflex nerves, but was more especially connected with the former, which, by the growth of the tumour, was deflected from its course, flattened, and its fibres separated; its capsule adhered so loosely to it, as to admit of being everywhere detached without the use of the scalpel, except towards the apex of the tumour, where the latter sprang from the plexus. Here the capsule was closely adherent and continuous with the neurilemma. Although upon this side the tumours were not so numerous as upon the opposite, yet no branch of the plexus was exempt from them. They were all of an oblong form, existed in the forearm as well as the arm, and were most numerously developed upon the internal cutaneous and musculo-spiral nerves; they numbered upwards of sixty. I may here observe, that, both in the upper and lower extremities, the tumours existed not only in the general sheath which invested the entire nerve, but that they were, in numerous instances, found upon the most delicate fibril that could be separated from the nervous trunk by an ordinary dissection.

"*Pneumogastric nerves.* The pneumogastric nerve upon the right side was enlarged to an immense size throughout the whole of its course, from the base of the skull to the diaphragm; it exceeded the normal size of any nerve in the human body, with the exception of the great sciatic, and throughout the cervical and thoracic divisions of its course presented a most irregular and nodulated appearance; it resembled a large, tortuous, varicose vein. This remarkable condition was produced by a vast number of oblong tumours, varying from half an inch to one inch in length, occupying uninterruptedly the entire course of the nerve, and so closely connected to each other, that they could not be rendered distinct without a difficult and tedious dissection. One large, solid, globular tumour occupied nearly the whole of the cervical portion of the nerve, which, along with the carotid artery and jugular vein, corresponded to its deep surface. The tumour was five inches in length and four in breadth, loosely connected with the side of the larynx, and covered with a thick and adherent capsule. Upon the summit of this large neuromatous tumour, and connected with it by cellular tissue, a smaller one existed, which also sprang from the vagus nerve, and resembled somewhat in form the supra-renal capsule. The left pneumogastric was also enlarged from the angle of the gum to the commencement of its thoracic portion, and presented a series of tumours, the largest of which occupied the lower part of the cervical division of the nerve; one equalled an almond in size, and over its surface there ran a delicate fibre, separated from the trunk of the nerve; the latter resumed its normal size shortly after its entrance into the cavity of the chest.

"*Lingual nerve.* Upon the under surface of the tongue, and towards its left side, there existed nearly twenty tumours, of a white and glistening aspect, and of various sizes, from that of a hemp-seed to that of a walnut; they were all connected with the smaller branches of the left hypoglossal nerve; there were none upon the trunk of the nerve, and but four upon its primary division. They were principally connected with the terminating branches, which could not in every instance be distinctly traced over the tumour to the muscles they were intended to supply. The branches connected with the two larger of the tumours, after a short course, spread out into a number of delicate filaments, which appeared to be lost upon the capsules which invested the tumour. Some of the smallest of the tumours were situated deep between the muscles, and in size, form, and relation to the fibres of the nerve, resembled very much the Pacinian corpuscles of the digital nerves.

"*Phrenic nerves.* Upon the phrenic nerves in the neck nothing abnormal was observed; but in their course along the pericardium they each presented three or four small tumours, resembling grains of wheat in size and shape; the neurilemma was continued over them; the nerves between the tumours were of normal size.

"*Intercostal nerves.* The anterior branches of the dorsal nerve upon each side were greatly enlarged, from the spine as far forward as the costal cartilage; the enlargement commenced where the nerves issued from between the bodies of the vertebræ, but became much more remarkable after their communication with the thoracic ganglion of the sympathetic, and in most instances appeared to be owing to a general hypertrophy of the entire trunk of the nerve, rather than to the development of distinct tumours upon the individual fibres. The fourth intercostal of the left side had attained such a size at its vertebral extremity as to occupy nearly the entire breadth of the intercostal space, and completely conceal the artery and vein; the sixth was nearly as large; but by far the most remarkable appearance was connected with the fifth. A large oval tumour, about two inches in length, and one and a half in its vertical diameter, occupied the posterior extremity of the fifth intercostal space, the breadth of which was increased by the absorption of the contiguous portions of the fifth and sixth ribs, between which the tumour was firmly wedged; the bones, however, were not carious, nor had they the rough and corroded aspect which the bodies of the vertebræ present when partially destroyed by the pressure of an aneurism; on the contrary, the surfaces in contact with the neuroma were perfectly smooth, and seemingly more dense than any other portion of the bones. The increase of space required for the accommodation of the tumour was provided chiefly at the expense of the rib superior to it. The tumour projected forwards, pushing the pleura before it, and posteriorly had also passed the level of the ribs, and was in contact with the muscles of the vertebral grooves. The intercostal nerve, which was split into three portions as far back as its junction with the sympathetic, traversed the inferior part of its anterior surface; and upon two of these portions smaller tumours existed. The principal tumour was inclosed in a very thick capsule, which was easily divisible into distinct layers, the external being connected with the neurilemma of the intercostal nerve, while the internal appeared to be proper to the tumour itself. The communicating branch of the sympathetic joined an oval tumour, nearly as large as a cherry-stone; and in many other of the intercostal spaces these branches were much larger than natural.

"Two large tumours existed in the pelvis; they lay upon the front of the sacrum, and were connected with the anterior branches of the sacral nerves, several of the filaments of which traversed their anterior surface; these filaments were enlarged, and several small tumours existed upon them. The capsules of the larger tumours were remarkable for the looseness of their connexion. There was no morbid alteration noticed in the brain or spinal marrow, or in their membranes; nor did the enlargement of the nerves, in any instance, extend within the spinal canal; it did not commence until after the trunk, formed by the junction of the anterior and posterior roots, had passed through the intervertebral foramen.

"In this remarkable case the total number of tumours which were removed from the body exceeded 800; they presented a striking uniformity, both in their external characters and in their internal structure; their form was oval or oblong; their colour, a yellowish-white; they were solid, and each surrounded by a capsule, which was continuous with the neurilemma; their surface was smooth; their long axis corresponded with the direction of the nerve upon which they existed, and they were only moveable from side to side. Their section exhibited an exceedingly dense close texture, of a whitish colour, and somewhat glittering aspect, presenting a uniform degree of solidity, and remarkable for a total absence of vascularity.

"Examined by the aid of the microscope, they were found to be composed essentially of a fibro-cellular structure, the fibrous structure predominating in by far the greater number, the areolar preponderating in a few; the fibres were arranged in bands or loops, amongst which permanent oval or elongated nuclei became apparent upon the addition of acetic acid. In no one instance, out of the numerous specimens examined, was there any trace discovered of nerve-tubes, nor any indication whatever of the presence of any of the structures considered by modern pathologists as characteristic of malignant disease." (pp. 13 et seq.)

This most interesting and well-reported case is illustrated by a series of beautiful lithographic plates, very graphic, and executed in a manner highly creditable to the artist.

A few months only elapsed before another similar case occurred in Dublin. This also is detailed at great length by Dr. Smith, and like its fellow made more instructive by the drawings with which it is accompanied. It will be sufficient here to state that the development of these neuromatous tumours was still more numerous than in the person of M'Cann, amounting, it is believed, to at least 2000 of various sizes and shapes.

In a third instance, which came under the care of Mr. Colles, an attempt was made to control the growth of one of these tumours by means of graduated pressure; only, however, with the unpleasant effect of accelerating its development. Whenever one of these tumours was in process of formation on any of the nerves, the patient experienced a remarkable feeling of general irritability, together with a peculiar sensation in the part, something like a rheumatic pain, but not always referable to the exact situation in which the neuroma was about to appear. This case and the preceding ones sufficiently disprove the correctness of Knoblauch's opinion, that neuromas are accessory ganglia, being the result of an original vice of conformation; and it will not have escaped the attention of the reader, that in no one instance was there anything like a fresh formation of true nervous substance; a circumstance which it is important to note, inasmuch as it differs from what happens when the ganglionic system of nerves is the seat of the disease. When this part of the nervous system is affected, the disease consists generally in a hypertrophied condition of some of the elements of the ganglia, and not in the formation of distinct tumours upon the nervous branches.

That remarkable example of the affection which is recorded by Cruveilhier, where the cervical ganglia of the great sympathetic nerve were enlarged, will occur to most of our readers.

The instance which Dr. Smith details as happening in the Richmond hospital is not so well known.

A female patient of Dr. Hutton's, 40 years old, suffered from the most intense neuralgia, in the course of the branches of the fifth pair of nerves. The pain commenced at the point of exit of the superior maxillary nerves from the infra-orbital foramen, and was of so acute a character as eventually to destroy the sufferer four months after her admission into the hospital; having in that time almost entirely prevented the motions of the lower jaw in speaking or eating.

"*Autopsy.* When the cranium was opened, and the brain removed, a tumour was seen in the right division of the spheno-temporal fossa; it was somewhat of the size and form of a walnut, and occupied the situation of the Casserian ganglion; it extended across the inner extremity of the great wing of the sphenoid bone, as far forwards as the foramen lacerum orbitale, and was covered by the superficial lamina of the dura mater, which was attenuated to a remarkable degree. The trunk of the fifth nerve appeared to enter the posterior part of the tumour, the interior of which, however, presented no trace of nervous structure; the ophthalmic divisions crossed the anterior part of its superior surface; the superior maxillary emerged from it at the foramen rotundum; and the third division seemed to be identified with a remarkable prolongation of the tumour, which passed through the foramen ovale, the circumference of which was increased to nearly double its natural extent.

> "The surface of the petrous portion of the temporal bone, which supported the posterior part of the tumour, was absorbed, as well as the superior wall of the horizontal portion of the carotid canal; between this rough and denuded portion of the bone and the deep surface of the tumour ran the median nerve, which, throughout its whole course, from Meckel's ganglion to the hiatus fallopii, was much larger than natural. The tumour was solid, and of uniform consistence; its section exhibited a cellular structure, without any trace whatever of nervous tissue; nerve-fibres could, however, by the assistance of the microscope, be seen upon various parts of its surface. The non-ganglionic portion of the nerve was compressed, but not enlarged." (p. 20.)

Traumatic neuroma is a disease that occasionally comes under the notice of most surgeons, and, as is well known, is always attended with severe suffering; this is not, however, proportioned to the size of the tumour, for a neuroma, no larger than a common pin's head, is often more painful than one as large as a bullet. When this form of neuroma follows the wound of a nerve, it is generally solid, not invested by the neurilemma, and without a distinct capsule; and is especially to be dreaded when a nerve has only been wounded, and not completely divided.

Dr. Smith narrates cases of this disease; but as they are not so rare as the other forms, we shall content ourselves with merely alluding to them. One caution in operating for their removal is inculcated by Dr. Smith, which it is surprising any surgeon should have required; it is, always to divide the nerve upon which the growth is seated—*above* first and *below* afterwards; inasmuch as to adopt the opposite proceeding is a gratuitous piece of cruelty. Amputation can scarcely ever be required in cases of traumatic neuroma, for the tumour is never so large as to make it perilous to remove the corresponding portion of the nerve.

Neuroma succeeding to amputation.—There is good ground for the opinion that a bulbous enlargement is the normal condition of the extremities of nerves divided in amputation. In some instances this enlargement is general, commencing gradually, and ceasing as gradually; in others, it presents the form of a distinct tumour, whose magnitude is determined by that of the nerve upon which it is seated. The general condition of the nerve-filament in these cases, is that of becoming incorporated and finally lost in the substance of the growth; but sometimes the nervous termination is sudden, and in others, filaments pass through and are traceable the whole length of the tumour. Such growths are destitute of vascularity, inclosed in a distinct capsule, and, in by far the greater number of cases, productive of no uneasiness whatever.

This condition of the ends of nerves, evidently designed to protect them from receiving injury by pressure and similar mechanical causes of irritation to which they are exposed, is sometimes the occasion of neuralgic suffering of the most severe character, of which no satisfactory explanation, in the present state of our knowledge, can be offered. Dr. Smith says:

> "The adhesion of the end of the nerve to the cicatrix of the stump; its irritation by osseous spiculæ springing from the extremity of the sawn bone; the including of a nervous filament in a ligature; the occurrence of a conical stump: such are a few among the many circumstances which have been stated as the occasional causes of these terribly painful tumours." (p. 24.)

The reader will find an instructive example of this affection detailed by

Dr. Smith; but as similar ones are not of very unfrequent occurrence, we refer them to the work itself for the details of it.

To relieve this condition, it is open to us to cut away a portion of the affected nerve, or to perform a fresh amputation higher up in the limb. When the affected nerve can be clearly demonstrated, the first of these operations is indicated, but it is not to be forgotten that its success is uncertain; for not only may the tumour which is the cause of the distress be situated above the extremity of the nerve, but also there is at times a condition of chronic inflammation of the trunk and sheath of the nerve, which is itself a sufficient cause of the neuralgic distress.

There is yet another condition under which nerves are liable to enlargement, and that is when they are exposed to continued irritation, in which case they follow the ordinary law of hypertrophy. Naegele has described such a state as existing in the tibial nerve of a patient with elephantiasis; and Dr. Smith says, he has often noticed it in the ulnar and posterior tibial nerve, behind the olecranon and malleolus respectively.

Such are not, however, true examples of hypertrophy of nerves, they are swellings caused by the effusion and deposition of plastic matter in the neurilemma and its connecting cellular structure; facts that bear out the statement we have quoted from Rokitansky, that such tumours perform the office of bursæ.

We have thus fulfilled our promise, and placed our readers in a position to judge for themselves of the merits of Dr. Smith's work. We confess to have judged it by a high standard, perhaps the highest—to have adopted a lower one would, we conceive, have been to pay but a poor compliment to Dr. Smith; and if, according to this test, he has failed in accomplishing *all* that could be wished, he has, notwithstanding, produced a work that will long endure in honorable association with his name.

We repeat, that to all who have been concerned in its production—to author, draughtsman, publisher, and printer—the medical world owes a debt of gratitude, which will, we are sure, be as gracefully acknowledged, as it has been honorably earned.

ART. VIII.

1. *On Parthenogenesis, or the Successive Production of Procreating Individuals from a Single Ovum. A Discourse introductory to the Hunterian Lectures on Generation and Development, for the Year* 1849, *delivered at the Royal College of Surgeons of England.* By RICHARD OWEN, F.R.S., &c., Hunterian Professor and Conservator of the Museum.—*London*, 1849. 8vo, pp. 76. With one Plate.
2. *Lectures on the Processes of Repair and Reproduction after Injuries. Delivered at the Royal College of Surgeons of England, for the Year* 1849. By JAMES PAGET, Professor of Anatomy and Surgery to the College. (From the 'Medical Gazette.')

ALTHOUGH we have freely expressed our opinion, on a former occasion, as to the absence of all effort, on the part of the College of Surgeons, to sustain the dignity of the profession by the public advocacy of its rights, or to elevate its character by raising the tone of its examinations; and although it has been too evident that, in the part it has taken in the Medical Reform question, it has been guided by a jealous regard for what it believed to be its rights and interests as a Corporation, rather than by a consideration for the general welfare of the body which it ought to represent; yet it is impossible to deny to it the credit of having most faithfully discharged one important trust which it undertook to fulfil, namely, the conservation of the Museum of John Hunter, and the propagation of the great principles which it was his glory to have enunciated. We shall not look back with even the shadow of blame over the long series of years during which these objects were very imperfectly effected. It was some time before the best methods were found out; it was not at first that the right men presented themselves. But the College can now boast of possessing in its Conservator and Assistant-Conservator, the two individuals most eminently qualified in this country for their respective offices; whilst of its Professors of Physiology and of Surgery, we believe that it may challenge the institutions of any age or country to produce the equals. Year by year are its magnificent collections now utilised by the men best able to develope and expound the great truths which they teach; year by year are some parts of the great design unfolded and displayed by those who have most profoundly and reverently studied it; and year by year are the opportunities thus presented to the metropolitan practitioner and to the advanced student, of learning, not merely the results of such studies, but the methods by which they have been pursued with such success. We cannot think of any kind of training more valuable than that, in which the learner is carried on, step by step, towards the perception of a great truth, in a style and with a manner that cannot but awaken his interest and fix his attention; and we have augured better of the rising generation of medical students, since we have seen with how much earnestness the benefits offered by the annual courses of Professors Owen and Paget have been embraced by no small proportion of those within whose reach they have lain.

Professor Owen, as is well known, has given to the world, from time to time, by separate publications, a portion of the valuable series of Lectures delivered in the theatre of the College. On the present occasion he offers us the introduction to his recent course on Generation and Development, which is designed as an exposition of a theory he has formed respecting the nature of that non-sexual mode of propagation, which has acquired so greatly increased an interest in later years, from the numerous additional examples of it which zoological research has brought to light, and from the attempts which have been made to generalize them.

Professor Paget's lectures for the present year form the continuation of the series of which we had the pleasure of noticing the commencement in our First Volume. The subjects of nutrition, hypertrophy, and atrophy, which he selected for his introduction, were appropriately followed last year by a course upon the life of the blood; but as this was not published, we had not the opportunity of bringing its principal topics before our readers, as we much desired to do. The present course forms a most natural continuation of the subjects of its predecessors; whilst it has happened, somewhat, as it would seem, designedly on Mr. Paget's part, that it has also formed a most apt sequel to Professor Owen's course, which it immediately followed. It being our purpose to take a somewhat critical survey of the doctrines advanced in the lectures of both our Professors, we shall commence with the latter; since we think that our readers will be better prepared to consider Professor Owen's theory, when they have been put in possession of Mr. Paget's method of viewing those facts of an analogous character with which they are themselves practically familiar.

Mr. Paget commences his course by laying down the principle which forms the connecting link between his subject and his predecessor's; and this he does in the following elegant and felicitous manner.

> "The mind, indeed, might gladly rest in the contemplation of such unhindered perfect works of Nature, as those of which he (Professor Owen) has discoursed; and it may at first seem an ungrateful task to turn from these, and consider the losses of perfection to which such works are liable, especially those losses which, being out of the ordinary course of Nature, abrupt and violent, we might expect would quite spoil the structure and the order of the living body. But, in truth, while studying the means by which living bodies, having suffered injury, regain their perfection, we may find continual manifestations of the same mysterious properties of the germ by which, through development, they first attained it. And in these manifestations we may discern the best evidence that, in developing the body to its perfection, the power of the germ is not lost, or quite exhausted, but rather is diffused through all the parts of the completed being; and that, so diffused, it works in them through all their life, determining, as with continuous design, every natural formative process, in accordance with the specific character of the individual. For I believe that we cannot form a just conception of the scope and nature of even the least of the processes of repair and reproduction after injury, or of the maintenance of the body in ordinary nutrition, or of its natural changes in the course of age, unless we admit that each organism, in its perfect state, retains, diffused through all its parts, some of the specific properties from which the power issued that actuated the impregnated germ in its development." (p. 1013.)

This doctrine, as he rightly states, is not new; being essentially nothing else than that of the "nisus formativus" of Blumenbach, and the "organic force" of Müller. But the general tone of modern physiology is considered by Mr. Paget as in some degree opposed to it; although we cannot but regard the opposition as more apparent than real. For although we may adopt the "cell-theory" to its fullest extent, and maintain that the vital actions of every component integer proceed from its own endowments as an independent centre of life; and although we may refer to chemical agencies some of the changes of composition which are concerned in the assimilating process; no one can overlook the fact, that in every organism these independent agencies are made to work together harmoniously for a common purpose, just like the separate performers in an orchestra, or the individual artisans in a factory. Mr. Paget, however, considers that both in the processes of common nutrition, and in the phenomena of repair and reproduction, "*a power* is in exercise, which is not admitted by the cell-theory, or by organic chemistry,—a power continuous with that manifested in the germ, and acting in all essential things like it." This power he thus defines:

> "The characteristic property of an impregnated germ is, that when placed in favorable circumstances, all the materials of which it first consists, and all that it appropriates, are developed according to the same method as was observed in the development of its progenitors,—in other words, in conformity with what we may regard as a law of specific character. In all the wonders of development that my

colleague has detailed, none, I think, appeared more marvellous than the constancy, the seeming tenacity of purpose, with which the germ is thus developed to the likeness of its parents. However vast its power of multiplication and increase,—however various its metamorphoses,—however far in some of these changes it may deviate from the form in which its parents generated it,—however near in some it may approach the perfect characters of another species,—or, which is stranger still, however much alike all germs may be in their primal structure and earliest developments,—yet, through all these things, each germ moves, with unswerving progress, guided by the same Power as created its first parents, to the formation of a being in which the parental form and properties are reproduced.

"Now, the constancy of this result, and its little dependence on external circumstances, justify the expression that every impregnated germ has, in itself, and in the properties with which its Maker has endowed it, the power to develope itself into the perfection of an appropriate specific form. However mysterious the nature of such properties, we cannot deny their existence, or refuse to recognise a law (in the sense in which we generally use that term), in the regularity with which the power acts that issues from them, when the germ is placed in favorable conditions. And, therefore, if it appear that this power is peculiar in its modes of operation, we may specialize it, whenever acting, as the "power of the germ," or "germ-power," in consideration of its having its apparent origin and intensest action in the germ." (p. 1014.)

Now we cannot but perceive, in the foregoing extract and in other passages of a like nature in this most interesting lecture, somewhat of that tendency to attribute to intermediate powers or agencies what is nothing else than a manifestation of the *modus operandi* of the Supreme Power, which is, in our apprehension, the fatal error of many bygone systems of physics and physiology. We do not mean to say that Mr. Paget is himself chargeable with this error; indeed we think it impossible that a mind so sagacious as his can be led away by it; but we should have been glad if his phraseology had been such as clearly to explain that in attributing to this "germ-power" a certain series of phenomena, he means nothing else than that these phenomena take place according to a certain designed order and with a perfect mutual harmony, *as if* under the direction of some controlling agency. What that controlling agency can be, if not the Supreme Creator himself, we must confess ourselves at a loss to imagine. Nought but the lingering phraseology of bygone systems can prevent the logical mind, as it seems to us, from recognising the First Cause as in direct and immediate operation in the production of every natural phenomenon; and from doing away with the whole machinery of "secondary causes" as a philosophical fiction, ingenious and sometimes useful, but prejudicial, as leading to the belief that these "agents" are distinct *entities*, and that their "laws" have a power of action *per se*. This phraseology runs through our familiar language upon scientific subjects. Thus we say that a stone falls to the ground *because* of the law of gravitation; whereas the fact is, that the law of gravitation *is* a law—that is, a generalized expression of facts,—*because* all stones tend to fall to the earth, the earth to the sun, and all masses of matter towards each other. We talk, again, of a law *governing* the phenomena, as if it had power to coerce them; whereas the real state of the case is, that the phenomena take place *according to* a certain plan, of which that law is an expression, so far as the plan is known to us. Thus it is a law that heat causes the expansion of bodies, and cold their contraction; but this law cannot prevent the expansion of water as it cools from 39½° to 32°; and it is evident that our law is imperfect, because we have not got at the whole truth of the case. So, again, we speak of having *explained* a phenomenon, when we refer it to a general law; the real fact being that, in so referring it, we merely place it on the same footing with other related phenomena, and are able from our knowledge of them to predict its occurrence under a certain set of conditions. Or, again, we say we have *explained* a phenomenon when we have been able to refer it to a certain "force," as that of gravity, electricity, chemical affinity, &c.; but this is no more of a real explanation than the other; for these "forces" are, like "laws," but generalized expressions of certain modes of agency which it is convenient thus to express, and particularly of such as can be rendered evident to our senses. Thus when we speak of the "force of gravity," we mean that tendency to the mutual approximation of all masses of matter, which manifests itself in their movement towards each other, or in the pressure they exert against ourselves if we attempt to antagonize it. And if we inquire into the source of that tendency, the cause of that pressure, it is obviously no answer to say, "it is the force of gravity," for this is merely changing one form of words for another; we can seek for the source and cause of these phenomena only in the will of their great Original Source, their great First Cause.

It is, doubtless, very convenient, in order to avoid continual reference to the Supreme and really Sole agent, to employ the terms "force," "law," "secondary cause," &c.; in fact, we can scarcely carry out a piece of philosophical reasoning or speculation without them; but still we think it incumbent upon any one who brings them prominently forward, and who seeks to establish the "laws" or to prove the existence of "forces," that he should clearly define the sense in which he employs them, in order to avoid the chance of leading his followers astray. Now in creating his "germ-force," and laying down the "law" of its action, without any such caution, Mr. Paget seems to us to lead his readers to suppose that this is some new controlling power, which is to overrule all the subordinate agencies at work in the organism, and to cause them to work together for a common end and on a common design. We cannot see how *any* power can have such a mediate agency between the Creator and any part of his works, unless it possess intelligence as its own attribute; and we are justified in this conclusion by the history of Dr. Prout's speculations on the subject. For, commencing with the idea of an "organic agent" as a being "endowed with little short of intelligence," he has at last been driven to regard it as actually "an intelligent being," residing in every organism, and directing all its operations. This seems to us to be the *reductio ad absurdum* of the whole doctrine of secondary causes as intermediate agencies; for we expect that few, save the worthy propounder of the hypothesis, will carry their love of philosophical fiction to such an extent, as to uphold the doctrine that not only animals but plants, not only the complex fabric of man, but the simple elementary cells of the aerial flags, are the habitations of these "intelligent agents."

We do not think it difficult to show that Mr. Paget's "germ-force" is nothing more or less than the comprehensive expression of all the individual forces, which are separately concerned in the evolution, maintenance, and reparation of a living being. Thus if we were so minded, we might say that the regular movements of the planets round the sun take place, that their perturbations are controlled, and that the stability of the system is maintained, by a solar-system force; yet we know that the regular movements, the perturbations, the limitation of these apparent irregularities, and the continued stability of the whole, are secured, not by any such controlling power, but by the harmony of all the separate actions of each component, resulting from the perfection of the original design, and the uniformity with which it is carried into action. Reverting to our former simile of an orchestra, we might point out that it is only whilst the individual performers are imperfect in their parts, that a general control is requisite, and that control must be *intelligent;* when each performer knows his own part thoroughly, and has been trained to execute it in such a manner as to express the composer's meaning, the performance is independent of the "conductor," the most consummate harmony being evolved by the executive skill of the performers, who act out the mind of the composer. Is not this all that is requisite in an organized fabric, to build itself up, to sustain its perfection during its allotted period, and to repair the injuries to which it is subject? If the original design be perfect, and every component part be endowed with a capacity for executing its own share of it with similar perfection, what more can be needed for the production of the result? The wonder lies in the marvellous coadaptation of all the separate actions to the common end; a coadaptation that seems to us as remarkable in the history of the life of the simplest, as in that of the most complex organism. But the wonder should only lead us to look with increased admiration and awe at the wisdom of the Original Designer, which could thus provide for every contingency without any departure from the sublime uniformity of His action.

We have no objection whatever to the use of the term "germ-force" in such a sense as this; and heartily thank Mr. Paget for its introduction, since it affords us a convenient expression of the general fact, that the *mode of action* which we witness in the first development of the germ continues during the whole subsequent period of life; and serves to link together in the mind the various phenomena which manifest this continuity of the original plan.—We shall now follow him in his summary of these manifestations.

In the first place, as the "germ-force" tends always to the attainment of the perfection of the specific form, so it is strictly limited to the attainment and maintenance of that perfection, and to certain rules of time, space, and mode of progress. Wherever, then, in the full life of any creature, we see organic formation ensuing in conformity with these restricted laws, we may regard the germ-power as in action. This we shall find to be a characteristic of the ordinary reparative operations, which all tend to the more or less complete restoration of the lost perfection.

Secondly, this power is diffused through the several organs and systems of the body, and is manifested in their harmonious concurrence, with various time and measure, in the attainment of the design and destiny of the whole. Mr. Paget very justly points out that "it is only in few and trivial instances that we can regard the development of one part or system of an embryo as the consequence of the development of another; rather, the several systems and the parts of each are developed independently and commensurately." He considers, therefore, that the "germ-power" is manifested in all those acts of adult life, in which a multiplicity of processes, effected by heterogeneous organs—all descended, however, from the original germ-cell,—is subordinated to an unity of purpose. This, too, is a striking characteristic of the reparative operations.

Thirdly, the germ-power manifests itself in its entire independence of any *model* existing in the structure itself. The ordinary assimilating processes act by fashioning the new material according to the model, and under the influence, of that in which it is inserted; but in embryonic development, there is no such precise imitation, for the very idea of "development" implies a change of condition; and "the only model after which each germ is developed, is that type according to which, from the first, and by the divine appointment, all the generations of its ancestors have been constructed." As examples of this mode of action in the adult body, Mr. Paget refers to that numerous class of operations in which the new parts are *not* developed from structures similar to themselves; such as the continued generation of epidermic and epithelial cells, of blood-corpuscles, of new teeth in fishes and reptiles, &c.; which were classed in his former course under the head of "nutritive repetition." All such phenomena, consisting in fact in the continued evolution of *new* parts, must be regarded as resulting from the continuance of the "germ-power," rather than from the ordinary assimilating power; and this, of course, holds good also in the production of new structures for the reparation of injuries.

The continuous action of the "germ-power" through life is further manifested in this —"that when, in an adult animal, a part is reproduced after injury or removal, it is made conformable, not to the condition which was proper to it when it was first formed, or in its infantile life, but to that which is proper according to the time of life in which it is reproduced—proper, because like that which the same part had, at the same time of life, in members of former generations." Thus, in the reproduction of the leg of the full-grown salamander after amputation, as described by Spallanzani, it is clear from the first that whilst the process itself is of a similar nature to that concerned in its first development, it is tending to produce, not the leg of a larva, but that of the adult animal. "The power, therefore, by which this reproduction is accomplished, would seem to be, not the mere revival of one which, after perfecting the body, had lapsed into a dormant state, but the self-same power which, before the removal of the limb, was occupied in its maintenance by the continual mutation of its particles, and now engages itself, with more energy, in the reconstruction of the whole."—To us it appears that, whilst it is convenient for the sake of the classification of the phenomena, thus to distinguish between the operations of the "germ-power" and the "assimilative force," we shall best keep in view their true relation, if we consider them both but as manifestations, under different conditions, of that original *nisus formativus*, which is in operation from the first commencement of the activity of the impregnated germ, to the final decay and disintegration of the structure into which it has been developed. The term *nisus*, moreover, seems less objectionable than "power," inasmuch as the former only expresses the *tendency* of a certain mode of action, whilst the latter conveys the idea that we have discovered the cause or source of the activity; and, as we have already attempted to show, the latter is far from being the truth,—the only step we have gained, or are likely to gain, in this kind of inquiry, being the perception of the pre-existing unity in the plan of formation of each being, and in the methods adopted to carry it into effect.

Mr. Paget then proceeds to inquire into the principal conditions which affect the degree of this reparative power; and, first, of the place of the being in the animal series, respecting which he makes the following very discriminative remarks:

> "The general statement sometimes made, that the reparative power in each species bears an inverse ratio to its position in the scale of animal life, is certainly not proved; and many instances are contrary to it, such as the great reparative power possessed by the Triton and other lizards, and the apparently complete absence of it in the perfect Insects. Rather, the general rule which we may expect to find true, and for which there is already much evidence, may be, that the reparative power bears an inverse proportion to the amount of germ-power consumed in the development and growth of the individual, and in its maintenance in the perfect state.
>
> "Our ideas of the consumption of power in the organization of matter, are, perhaps unavoidably, very vague: yet are there facts enough to prove that the power which can issue from the mysterious properties of a germ is limited; that it is not ever communicated to an indefinite quantity of matter; and there are enough to justify the hypothesis, that the germ-power, thus limited, is in some measure consumed in the development of every new structure, and, in a less measure, in the growth and maintenance of those already formed. The instances to which I shall refer are explicable on this hypothesis, and are, therefore, evidences of its probability." (pp. 1018-19.)

He then adverts to the well-known facts which indicate, that the reparative power is greater in all parts of the young than of the older individuals of the same species, and that it is greatest of all in the embryonic state; and he shows that all these harmonize with the general statement, that the germ-power is more diminished by *growth* than by mere *maintenance*, but that it is most of all diminished by *development;* or, in other words, that the reparative processes are most completely effected in the earliest stages of development, but that as development advances they are less perfectly accomplished; that during the whole period of growth they are more active than after the full size has been attained, and that in adult age they are more energetic than in an advanced period of life. So, again, when we compare together the reparative powers in the adult state of different tribes of animals, we find that they may be generally said to vary inversely to the amount of dissimilarity between the adult and the embryonic states of each respectively.—In illustration of this view he refers to the extraordinary reproductive powers possessed by zoophytes and the lower articulata; and he quotes from the work of Sir J. G. Dalyell a very interesting experiment, which indicates that in these creatures the recovery from the effects of injury is gradual; a succession of heads being produced from the cloven stem of a tubularia, of which the first was imperfect, whilst the latter ones gradually improved until the normal conformation was attained.—So among the higher articulata, it appears from the observations of Mr. Newport that the reparative power in the complete state is limited to the orders in which that state is attained by a comparatively simple and direct course of development; thus the Myriapoda, whose highest development scarcely carries their external form beyond that of the larvæ of more perfect insects, the power of reproducing antennæ and legs is retained up to the time of their last moult; and among hexapod insects, we find that the Phasmidæ, whose change of state is marked by little else than the development of wings, are those in which this power seems most enduring. On the whole, however, the class of Insects is the one of all the articulate series, in which there seems to be least reparative power: and it is remarkable that they should be inferior in this respect to the Arachnida, although the latter in many points of their conformation approach more nearly to the vertebrate type. Still more remarkable does it seem, that among Reptiles we should find so high a degree of reparative power persistent, as to suffice for the regeneration of a limb or an eye; when insects are so destitute of it. But we believe that Mr. Paget's principle completely holds good in these cases; for insects must be regarded as presenting the highest possible degree of development of the articulated type, which they only attain after a long series of metamorphic changes; whilst the arachnida reach their perfect state through a more direct development; and the salamander with its allies stands far lower in the vertebrated series than do the insects in the articulated, and a comparatively small amount of metamorphic change takes place in their passage from the embryonic to the perfect form.

The reparative powers of man and the higher vertebrata are limited, according to Mr. Paget, to the three following classes of parts:—1. Those which are formed entirely by "nutritive repetition," such as the blood and the epithelia:—2. Those which are of lowest organization, and (which seems of more importance) of lowest chemical character; as the gelatinous tissues, the cellular and tendinous, and the bones:—3. Those which are inserted in other tissues, not as essential to their structure, but as accessories, as connecting or incorporating them with the other structures of vegetative or animal life, such as nerve-fibres and blood-vessels. With these exceptions, injuries or losses in the human body are capable of no more than repair in its more limited sense, i. e., in the place of what was lost, some lowly organized tissue is formed, which fills up the breach, and suffices for the maintenance of a less perfect life.

Deferring until the close of the present article, as its more appropriate place, the citation of the eloquent peroration with which this first lecture concludes, we shall now follow Mr. Paget through the principal details of the inquiry which he has thus introduced. In the second lecture he treats of the *materials* provided for the repair of injuries inflicted on the human body; understanding by the term injuries, visible breaches of continuity, such as wounds and fractures. There is every probability that recovery from the effects of disease takes place in conformity with the same general laws as recovery from injuries; but the whole details of the process have yet to be discovered. He commences by drawing attention to the very different conditions of a subcutaneous wound, and of one open to the air; a principle on which John Hunter insisted, and on which is founded the whole science of subcutaneous surgery. And he then announces, as the result of his own experimental inquiries, that "the materials produced for the repair of open wounds are not the same, or, at least, do not develope themselves in the same manner, as those for the repair of closed or subcutaneous ones."

That the blood extravasated in a wound may itself become organized—that is, may assume the characters of a tissue, and may coalesce with the adjacent parts, and become vascular—was the doctrine, as every surgeon knows, of John Hunter; and although it fell for a while into disrepute, it has been clearly established by recent experiments and microscopic investigations (especially those of Dr. Zwicky), of which an admirable account was given by Mr. Paget, in his course last year. He now adds the following valuable observation, which he has himself had the opportunity of making since its delivery:

> "The specimen was obtained from an insane person, by my friend Mr. Holmes Coote. A thin layer of pale blood-coloured and ruddy membrane lined the whole internal surface of the cerebral dura mater, and adhered close to it. Its colour, the existence of patches of blood-clot imbedded in it, and all its other characters, satisfactorily proved that it had been a thin clot of blood,—an example of such as are effused in apoplexy of the cerebral membranes, of such as Mr. Prescott Hewett has so fully described in his paper in the Medico-Chirurgical Transactions. Numerous small vessels could be seen passing from the dura mater into this clot-membrane, and with the microscope, while they were still full of blood, I made a sketch, from which the diagram was copied. The arrangement of blood-vessels bears a close resemblance, but, perhaps, more in its irregularity than in any positive characters or plan, to that which exists in false membrane formed of organized lymph; but the vessels were, I think, generally larger.
>
> "Such were the blood-vessels of this organized clot. Its minute structure showed characters which are of peculiar interest, because of their resemblance to those observed in the material that is commonly formed in the repair of subcutaneous injuries. In the substance of what else appeared like a filamentous clot of fibrine, sprinkled over with minute molecules, the addition of acetic acid brought into view corpuscles like nuclei, or cytoblasts, very elongated, attenuated, and, in some instances, like short strips of flat fibre. Of course, such corpuscles are not to be found in any ordinary clot of fibrine; neither are they seen in any stage of the organization of such lymph as is commonly effused in inflammation; but they exactly resemble such as may be found in certain examples of rudimental cellular tissue, and (among these) in the material for the repair of subcutaneous injuries. In short, the minute structure of this clot now organized was an example of what I shall have often to refer to under the name of 'nucleated blastema.'" (Lecture ii, p. 1066.)

It is certain, however, that extravasated blood has ordinarily no share at all in the reparation of injuries; the smallest portion of blood being effused in the cases in which the largest quantity of reparative material is produced in the shortest time, and in which the healing process is most perfectly accomplished. Thus, in cases of subcutaneous division of tendons and fracture of bones, it is rare to find clots of blood lying between the divided parts. When blood is effused into open wounds, it seems to have less tendency to become organized than when poured into a closed cavity; and it is liable to act as a foreign substance, especially if effused in large quantity, and to excite inflammation, so that the tumefaction of the parts, or their incipient suppuration, pushes it out of the wound. In more favorable circumstances, the blood is absorbed; but Mr. Paget states, as the result of his observations, that this absorption is much less rapid than is commonly supposed, and that it is not effected in order to make way for the effusion of other reparative material, but is actually a consequence of that effusion, being inclosed within it, and absorbed by the vessels which are developed in its substance as its organization proceeds. Still he believes that blood effused into wounds *may* be organized and form part of the reparative material; but as its progress towards organization is much slower than that of coagulable lymph, the healing of the wound is probably retarded by its presence.

The fibrinous plasma, or coagulable lymph, which is effused into wounds as their proper reparative material, does not seem to differ in any essential characters from that which is poured out on the surface of serous membranes. Its distinctive vital endowment is its tendency to spontaneous development into a fibrous tissue; but this, according to Mr. Paget, may take place in two modes. For the plastic material which is produced for the repair of open wounds, generally developes itself, in the first instance, into cells, from which the fibrous tissue is formed by a subsequent metamorphosis; whilst that which is poured out for the healing of subcutaneous wounds, as generally developes itself into fibrous tissue by the fibrillation of the blastema, in which nuclei are present, but no cells. "Both these," remarks Mr. Paget, "are repetitions of natural modes of development of the same forms of tissue. In the embryo and fœtus you may trace very well the development of subcutaneous cellular tissue through nucleated cells, and that of tendons and other *formed* fibro-cellular or fibrous tissues through nucleated blastema." It need not appear an objection to this statement, that there should be two modes of development for the same tissue; for this is seen in the case of the blood-corpuscles, as explained in Mr. Paget's former course; and it is now well known to be the case also in the production of bone.

The development of the fibro-cellular or connective substance through the medium of cells, may be observed in the material of granulations, or in a wound healing by primary adhesion, as well as in inflammatory adhesions. The cells, at first, bear a close resemblance to the colourless corpuscles of the blood; but they gradually elongate, and attenuate themselves into the filamentous form. It was stated by Schwann that such cells metamorphose themselves into *fasciculi* of fibres, but Mr. Paget has not seen a single cell form more than a single filament; the long filaments, indeed, being formed of two or more cells, attached end to end. The nuclei seem to take the initiative in this metamorphosis, becoming oval even before the cell does; in other cases, however, the cells seem to take on the reproductive instead of the metamorphic action, in which, also, nuclei are the prime agents,—large compound cells being frequently met with in granulations, containing eight, ten, or more nuclei, pretty evidently derived from the subdivision of the original nucleus, and destined to be developed into new cells. Mr. Paget has not been able to trace the further development of the nuclei of the filamentous cells into fibres of elastic tissue (as described by Henle) in the tissue of granulating wounds or inflammatory exudations; the nuclei appearing rather to waste and be absorbed.

On the other hand, the development of fibrous tissue through nucleated blastema is observed in the material poured out for the reparation of subcutaneous wounds. The following is Mr. Paget's account of the process, as traced by him in the reunion of divided tendons.

"When the first effusion of the products of the inflammation, excited by the violence of the wound, is completed, then a quantity of finely molecular or dimly-shaded substance, like homogeneous or dotted fibrine, begins to appear in the space in which the bond of union is to be formed. This substance is infiltrated in the tissue that collapses into the space between the retracted ends of the tendon.

At first there is no appearance of nuclei or cytoblasts in it; it seems to be merely a blastema of fibrine; but as it acquires firmness and distinctness, the nuclei appear in it; they seem to form out of collecting clusters of granules, and presently appear as oval bodies, with dark hard outlines, soon becoming elongated, with clear contents, without nuclei, irregularly scattered, but so firmly imbedded in the blastema that, in general, they cannot be dislodged. They may be seen in very fine fragments without reagents; but, commonly, the application of acetic acid is necessary to make them distinct, by making the intermediate substance transparent, while the nuclei themselves acquire dark edges and shrivel up a little. The nuclei undergo little change, while the blastema in which they are imbedded is acquiring, more and more distinctly, the filamentous appearance, and then the filamentous structure,—only they appear to elongate, and to attenuate themselves, and to grow more irregular in their outlines as if by shrivelling.

"The blastema may become at length perfect fibro-cellular or fibrous tissue,—tissue not to be distinguished from that found in normal conditions. The final disposal of the nuclei is doubtless sometimes, as Henle describes it, that they are developed into the nucleus-fibres, and constitute some of the various forms in which elastic yellow tissue is found mingled with the proper white filaments. But, in the process of repair by tissue thus developed, as well as by that which is formed through cells, my impression is that the nuclei finally shrivel,—gradually contracting into little crooked or branched lines,—and at length disappearing: for, as I have already said, well-formed nucleus-fibres, or such elastic yellow fibres as might be developed from them, do not generally occur in cicatrices of recent formation, or in the large bonds of union by which divided tendons are healed." (Lecture ii, p. 1071.)

Now this description as closely corresponds with Henle's account of the mode in which the fibrous tissues are at first formed, as did the preceding account of their production by the metamorphosis of cells with that of Schwann; and Mr. Paget's observations thus go to prove that both these admirable anatomists are right in what they severally affirm, though both are wrong in conceiving the method witnessed by them to be the only one. For whilst the development of fibres in a nucleated blastema appears to be the regular mode of formation and of reparation in the case of tendons and ligaments, that by the metamorphosis of cells appears to be equally characteristic of the formation and reparation of the subcutaneous areolar or fibro-cellular tissue.

The formation of the cell-tissue appears to take place wherever inflammatory action participates in the reparative processes; "for of such cells, in various stages of development, are formed not only long suppurating granulations, but also the walls of abscesses, inflammatory infiltrations, producing succulence, induration, and thickenings of soft parts; and in the lymph produced in inflammation of serous membranes, which organizes itself into false membranes." In the early stage of the reparation of most wounds in warm-blooded animals, some indications of this process may be traced. But it speedily gives place, in subcutaneous wounds, to the other method, which we find to prevail in morbid processes in which there is no sign of inflammation; as, for example, in the growth of warts and condylomata, in the simple fibro-cellular tumours of the subcutaneous tissue, in nasal polypi, and in organizing clots of blood. We therefore seem justified in concluding, generally, "that inflammation ensues in the healing by adhesion and granulations, but does not exist in the healing of subcutaneous wounds."

Mr. Paget next proceeds to speak of the different modes of healing of wounds; and in the first place of such as are externally open. He first describes that which was designated by Hunter (who erred in imagining, however, that a stratum of blood is the connecting medium) as "union by the first intention;" but as that term is now generally applied to another process, he adopts from Dr. Macartney the term "immediate union" to designate that which is effected by the simple adhesion of the wounded surfaces, without any connecting medium of blood or lymph. It seems to have been supposed by Dr. Macartney, however, that such union can only take place in the case of comparatively small and simple incised wounds, such as cuts of the finger or hand; but Mr. Paget has no doubt that even very large wounds, such as those made in the extirpation of mammary tumours, are thus healed under favorable circumstances. In support of this statement he details an extremely interesting case in which he was afforded the opportunity, by the death of the patient from erysipelas, of examining the condition of a wound of this kind, the greater part of which had healed rapidly; of which he says that "the state of parts cannot be better described, than by saying that scarcely the least indication remains, either of the place where the flap of skin was laid on the fascia, or of the means by which these parts were united: it was not possible to distinguish the relation which these parts held to each other, from that which naturally exists between subcutaneous fat and the tissue beneath it;" and no lymph- or exudation-globules could be discovered by the most careful microscopic examination. This view of the case he has confirmed by experiments upon animals. He justly designates this "immediate union" as "the best imaginable process of healing;" and points out its two essential conditions, as being the exactness of the coaptation of the wounded parts, and the absence of all inflammatory action. The following practical hints founded on these views are well deserving of the surgeon's consideration.

"To obtain the former, the simple replacement of the raised pieces of skin may sometimes be sufficient. But there is a class of cases to which this mode of healing is peculiarly applicable, and in which more than this may be required: I refer to the removal of large subcutaneous tumours,—fatty tumours and the like,—where, after the operation, large cavities are left, and commonly left to granulate. In these cases I venture to express my belief that modern surgery does not often enough employ the older method of carefully and softly padding the parts, and of so bandaging them that the exposed surfaces may be held in contact for the one, two, or three days necessary for immediate union. Many surgeons, I know, commonly employ these means; but by many,—and, I think, the majority,—they are avoided, through fear of exciting inflammation by overheating the parts, or hindering the discharge of secreted fluids. Doubtless, no single rule of management would be safe; but I think with regard to this fear of exciting inflammation, it need not be entertained, if the means I have alluded to be employed only during the first two or three days after the infliction of the wound. For one may generally observe that, for at least two or three days after such an injury as an amputation, the raising a flap of skin in a removal of the breast, or the like, scarcely any reparative process appears in the parts that are kept from contact, no granulations are formed, no pus secreted, only a little serous-looking fluid oozes from them. Now, during this calm, which would certainly never be disturbed by the parts being softly padded and kept in perfect rest, the immediate union may be accomplished: if, through any untoward circumstance, it be not in this period completed, its occurrence is, I believe, impossible, and then the means more appropriate for other methods of healing may be employed.

"The attainment of the other necessary condition,—the absence of inflammation,—is quite consistent with these means for ensuring perfect and continued contact of the wounded surfaces. How the condition is to be fulfilled I need not say: the means are some of those that are commonly laid down for preventing inflammation from being, as it is said, more than is necessary for the union by the first intention; and the best of them are temperance and rest. The necessity of observing them will appear the greater, if it is remembered that what is wanted for immediate union is, not a certain undefined slight degree of inflammation, but the complete absence of inflammation;—for, the probability of the occurrence of immediate union may be reckoned as being in an inverse ratio to the probability of inflammation occurring in the time necessary for its accomplishment. (Lecture iii, p. 29.)

Not many years since, it was imagined that Hunter's "union by the first intention," and "union by adhesion," or by "adhesive inflammation," were one and the same thing, the only difference being in the degree of the inflammation concerned. We have seen, however, that although Hunter was wrong in the opinion that *blood* is the connecting medium in such cases (since blood cannot undergo organization so rapidly), yet an immediate union without inflammation may take place; and the process is very distinct, therefore, from those in which inflammation essentially participates. The first of the latter is appropriately distinguished by Mr. Paget as "primary adhesion," in contradistinction to that by granulations, which is called "secondary adhesion." The former is seen, as every surgeon knows, in cases of incised wounds the edges of which are not brought into perfect coaptation, or in which some inflammatory action is present which gives rise to effusion of lymph. In either case, the connexion is finally re-established by the organization of the lymph, into which vessels pass from both surfaces; but the intervention of this bond is manifested by the persistence of the cicatrix, which is indicated by the peculiar condition of the newly-formed skin. A very good example of this process, as it takes place under favorable circumstances, is presented after operations for harelip; the wound left by which, however, may partly heal by "immediate union." The steps by which the plastic effusion becomes converted into tissue are essentially the same as in the "secondary adhesion," which occurs in cases where the wounded surfaces are not united by either of the foregoing means, but are, in the ordinary phrase, "left to granulate." The first stage in the healing process of open wounds is the formation of a "glazing" on the exposed surface, which closely resembles the buffy coat of the blood, being composed of coagulated fibrine and colourless corpuscles. Mr. Paget attributes the presence of the latter simply to their peculiar adhesiveness, which makes them less readily flow from the blood-vessels when the bleeding is about to stop; so that afterwards, when the vessels finally close and empty themselves, a large proportion of these corpuscles may issue from them and adhere to the cut surface over which they roll. The adhesiveness of which he speaks appears to be merely physical, and dependent upon the nature of their surface; we scarcely think that he allows enough weight to the attraction which they seem to have for any of the solid tissues which are in state of exalted formative activity, as manifested by their stagnation and accumulation in the vessels of such parts. The increase of this "glazing" is the prelude of the formation of granulations; but whilst it is going on, there is, in and about the wound, an appearance of complete inaction, a sort of calm, in which scarcely anything appears except a slight oozing of serous fluid from the wound, and which continues from one day to eight, ten, or more, according to the nature and extent of the wounded part, and the general condition of the body.

> "These periods of repose after severe injury are of equal interest in physiology and in surgery; but in the former it is only the interest of mystery. Observations on injuries of the frog's web make it probable that the blood is stagnant for some little distance from the wound during several days after the injury; but why it is so, and what are the changes ensuing in and about it, preparatory to its again moving on, we cannot tell. The interest to the surgeon watching this period of repose is more practical: the calm may be the brooding-time for either good or evil; whilst it lasts, the mode of union of the wound will, in many cases, be determined; the healing may be perfected, or a slow uncertain process of repair may be but just begun; and the mutual influence which the injury and the patient's constitution are to exercise on one another appears to be manifested more often at or near the end of this period than at any other time. Moreover, in general, the time at which, on each tissue, granulations are produced, is determined by this calm; for they begin to be distinctly formed at its end. Thus, on a stump, after a circular amputation, one may find the margin of the skin and the surface of the muscles well covered with granulations, while the surface of the fat reflected with the skin is barren of them, and the sawn walls of the bone are dry and bare. But from the sawn end of the medullary tube there may already protrude a florid, mushroom-shaped mass of granulations, overhanging the adjacent walls; as if parts in which nutrition is habitually carried on under restraint within hard and rigid boundary-walls were peculiarly apt to produce abundant organizable material as soon as they are released. One may sometimes observe a similar fact in the growth of granulations out of the very centre of the cut end of a divided tendon, while its margins are unchanged. The abundant growth of substance, like brain covered with granulations, in cases of hernia cerebri, is of the same kind." (Lecture iii, pp. 31-2.)

The cessation of this period of calm, and the active commencement of the reparative operations, are marked by the restoration of the flow of blood in the vessels of the wounded part; but the current is not altogether normal, being slower but fuller than natural, so that on the whole more blood than usual passes through the capillary plexus. It is possible, too, that new vessels may be generated during this time. Mr. Paget regards this state of the circulation as an indication of the existence of an inflammatory condition; but we can scarcely so consider it, since, in our apprehension, genuine inflammation does not exist without inflammatory effusions. He seems to think, too, that the ordinary law of the capillary circulation, which we are glad to find that he fully recognises,—namely, that "in the discharge of natural functions, the increased supply of blood to a part appears always to be a *secondary* event, the *consequence* of some increase in the formative actions of the part,"—is suspended in this abnormal state; the increased supply here preceding the increased production of the material. It does not seem to us difficult, however, to reconcile the two facts; for it appears that, in the first instance, the "shock" of the injury has so depressed the formative powers of the parts which have sustained it, that the circulation in them is altogether stagnated; whilst, on their recovery from this, and the recommencement of their formative activity, the still-depressed vitality of the walls of the vessels occasions them to yield to the pressure from within, and thus allows them, by their distension, to give passage to a larger current. However this may be, the increased afflux of blood here precedes the formation of plastic material in increased proportion; and it is from the effusion of this material, that the granulating process properly commences. This process may take place in the healing of subcutaneous wounds, the granulations being then composed of "nucleated blastema," and being free from suppuration. But in by far the greater proportion of cases, granulations are only formed in exposed injuries; they consist of cells, which may develope themselves into fibro-cellular tissue; and they have a suppurating surface. The formation of this tissue and of the investing skin takes place in a manner essentially the same as in the embryonic state; and it is interesting to remark, that we find the character of the new cuticle varying, as in the fœtus, with the part on which it is formed, being more opaque and thicker on the soles of the feet and the palms of the hands than elsewhere,—another instance, as Mr. Paget justly remarks, of the identity of the powers that are put in operation in the acts of first construction and of repair. But the latter process is more liable to defect or error than the former; so that, by arrest of development, by degeneration, or by complete decay, the granulation-cells are prevented from developing themselves into the new tissue which it is their purpose to generate, and the wound does not heal.

Mr. Paget's observations lead him unhesitatingly to adopt the conclusion of Valentin, Gerber, and others, that *pus-cells* are nothing else than ill-conditioned granulation-cells; but he points out that this lower condition may be acquired in two modes,—either by degeneration from a previously higher development, or by originally imperfect development. And, in like manner, he regards the *liquor puris* as the degenerate form of the solid blastema, being composed of albuminous matter incapable of organization. This is the doctrine which has always appeared most probable to ourselves; the microscopic observations which we long since made having led us to the belief, notwithstanding the positive statements of many practised observers, that no distinct line of demarcation can be drawn between the "exudation-cell" of inflammatory lymph, the "granulation-cell," and the "pus-cell," either of the two former showing every gradation of form and condition to the latter. We are, therefore, very glad to find our views confirmed by the far more comprehensive and elaborate researches of Mr. Paget; and are disposed to consider the question as now settled in the conclusion, "that the true relation which the cell in granulations or in inflammatory lymph holds to the pus-cell, is that of a well-organized structure to the same structure either ill-developed or degenerate after having, up to a certain point, been duly formed." The following extract will show the felicity with which Mr. Paget brings facts with which every practical man is familiar, to the support and illustration of his doctrines:

> "An observation, which any one may easily make, seems to indicate that the liquor puris may be the product of the degeneration and liquefaction of the solid blastema, as the pus-cells appear to be of the granulation- or exudation-cells imbedded in it. If the formation of abscesses be watched, one may see, on one day, a large solid and inflamed swelling, firm and almost unyielding, giving no indication of containing any collection of fluid; but next day, one may detect in the same swelling the signs of suppuration; the border may feel as firm as before, but all the centre and the surface may be occupied with an ounce or more of matter. And observe,—this change from the solid to the liquid state may have ensued without

any increase of the swelling. Such an increase must have occurred had the pus been secreted in a fluid state into the centre of the solid mass; and the changes cannot, I think, be explained except on the admission that the inflammatory product, which was effused and infiltrated through the tissue in a solid form, has been liquefied; its exudation-cells degenerating into pus-cells, its blastema into liquor puris.

"Such a liquefaction is not that assumed in the older doctrines, which held that pus was partly formed of the dissolved materials of the original tissues. The original tissues doubtless remain, unless partially absorbed; yet there appears to be thus much of liquefaction in the formation of an abscess, that the inflammatory product, first formed as a soft solid, degenerates and becomes fluid." (Lecture iv, p. 77.)

Mr. Paget adds the suspicion that other rudimental forms of tissue may, in degeneration, sometimes assume the characters of pus-cells. This, he thinks, may be the case, for example, in regard to mucus-cells; since the pus so readily secreted from inflamed mucous membranes exhibits forms of transition between the two. And the line of distinction between pus-cells and the colourless corpuscles of the blood is equally indefinite; so as to suggest whether the purulent diathesis may not have its essential anatomical character in the degeneration of these corpuscles, possibly induced or favoured by the introduction of pus already formed into the blood.

Whilst the conversion of the granulation-cells into fibrous tissue is thus going on in the part most removed from the exterior, and their degeneration into pus-cells is taking place on the surface, the substance of the granulations becomes vascular, and nerves also may possibly be developed in it. We have here a plain indication, however, that organization is independent of what has been frequently but erroneously called "vascular action;" for not merely are the cells developed in the blastema, but their metamorphoses are making considerable progress, *before* the granulations are penetrated by blood-vessels; and the purpose of these is obviously nothing else than to supply the materials for further development, the afflux of blood being obviously proportional to the activity of the changes taking place in the part, and lessening with their diminution. The mode in which the formation of vessels in a new part takes place, is a question which has been hotly discussed by different observers; and here, as elsewhere, Mr. Paget shows that their apparently opposite views are not really inconsistent, since the process takes place on either of three different plans, according to the circumstances of the case. His account of these plans is so valuable for its clearness and comprehensiveness, that we shall quote it nearly in full:

"In embryos, we may discern three several modes according to which blood-vessels are formed—a good example of the manifold ways by which, in development, the same end may be reached. In the first and earliest method, they are constructed around the blood-corpuscles, which being gradually developed from some of the embryo-cells, are laid out in the plan of the earliest and simplest circulation of the blood. Thus, in the larvæ of Batrachia, as in the common tadpole, before even the walls of a heart are formed, one sees a crowd of embryo blood-corpuscles collected where the heart is to be; and, in the substance of the out-growing external branchiæ, are looping lines of blood-corpuscles, around which as yet no walls can be discerned. It is so, also, with the blood and vessels of the warm-blooded vertebrata; the corpuscles are rapidly developed from some of the cells of the germinal area, and are laid out in the plan of the heart, and the terminal sinus, and their communicating channels. But at first it is only as a plan; the blood does not move, and is not walled in. Then, as the heart and vessels are formed around the blood, its circulation in these simple channels is established. In this case, the vessels appear to be formed of the plasma of fluid material which lies between the cells, and gradually assumes the condition of a membrane, and is then developed into the more complex structures of the blood-vessels.

"After this earliest period of embryo-life, it is probable that blood is never formed except within the vessels already constructed. It would seem as if none but the original embryo- or germ-cells could be directly transformed into blood-corpuscles; all those that are later made derive their materials through a process of gradual elaboration in lymph- or blood-vessels, to which process no resemblance can be discerned in the substance of granulations. To increase the extent and number of vessels that must be added in adaptation to the enlargement and increasing complexity of the embryo, two methods are observed; of which the one appears chiefly appropriate to the interstitial formation of new vessels; the other, for the construction of those of superadded or outgrowing parts.

"For the former, one finds, in the interspaces of vessels already existing, primary cells, which enlarge and elongate, and send out branches in two or more directions,—branches sometimes so exceedingly slender, that one might take them for mere threads of embryonic fibro-cellular tissue. But they are hollow; and while some of them are directed into anastomosis with each other, others extend towards, and open with dilatations into, the vessels already formed and carrying blood. Then, these fine branches of each stellate cell becoming larger, while the main cavity of the cell, from which they issued, attenuates itself, they are altogether transformed into a network of nearly uniform calibre, and through these the blood, entering by the openings of communication with the older vessels, makes its way. Thus the wide spaces of the network formed in the primordial circulation are subdivided into smaller meshes, and each part receives a more abundant supply of blood. Such a development (as shown in the diagrams) may be seen in the soft gelatinous matter within the amnion of embryo-sheep, and in the tissue of the tail of the tadpole; though in this last the development is often abortive.

"But, for parts that are formed by super-addition or out-growth, another mode of development of blood-vessels is observed; and this, I believe, is the only mode in which new blood-vessels are ever formed for granulations, or for superficial deposits of lymph, adhesions, and the like. For, though the process in granulations or in lymph cannot be exactly watched during life, yet every appearance after death is consistent with the belief that it is the same as I have described, and no appearances are found which would justify a suspicion that either of the other methods of development has occurred. The method may be termed that by *out-growth* from the vessels already formed. Suppose a line or arch of capillary vessel passing below the edge or surface of a part to which new material has been superadded. The vessel will first present a slight dilatation in one, and coincidently, or shortly after, in another point, as if its wall yielded a little near the edge or surface. The slight pouches thus formed gradually extend, as blind canals or diverticula, from the original vessel, still directing their course towards the edge or surface of the new material, and crowded with blood-corpuscles, which are pushed into them from the main stream. Still extending, they converge; they meet; the partition wall, that is at first formed by the meeting of their closed ends, clears away, and a perfect arched tube is formed, through which the blood, diverging from the main or former stream, and then rejoining it, may be continuously propelled.

"In this way, then, are the simplest blood-vessels of granulations and such outgrowths formed. The plan on which they are arranged is made more complex by the similar out-growths of branches from adjacent arches, and their mutual anastomoses; but, to all appearance, the whole process is one of out-growth and development from vessels already formed. And I beg of you to consider the wonder of such a process; how, in a day, a hundred or more of such loops of fine membranous tube—less than 1-1000th of an inch in their diameter—should be upraised, not by any mere force of pressure, though with all the regularity of the simplest mechanism, but each by a living growth and development, as orderly and exact as that which we might trace in the part most essential to the continuance of the life.

Observe, that no force so simple as even that of growth or mere assimilation can determine such a result as this; for, to achieve the construction of such an arch, it must spring with due adjustment from two determined points, and then its flanks must be commensurately raised, and these, as with mutual attraction, must approach and meet exactly in the crown. Nothing could accomplish such a result but a power determining the concurrent development of the two out-growing vessels, in conformity with the same law as that according to which the same power actuates the germ. We admire the intellect of the engineer, who, after years of laborious thought, with all the appliances of weight and measure, and appropriate material, can begin at points wide apart, and force through the solid masses of the earth one tunnel, and can wall it in secure from external violence, and strong to bear some ponderous traffic;—and yet he does but grossly and imperfectly imitate the Divine work of living mechanism that is hourly accomplished in the bodies of the least conspicuous objects of creation—nay, even in the healing of our casual wounds and sores.

"The wonder of the process is, perhaps, in some degree, enhanced by the events that will follow what may seem to be an accident. When the new vessel has begun to project, it sometimes bursts; and the blood-corpuscles that issue from the ruptured pouch or diverticulum collect in an uncertain mass within the tissue, like a mere ecchymosis; but, before long, they manifest a definite direction, and the cluster bends towards the line in which the new vessel might have formed, and thus opens into the other portion of the arch, or into some adjacent vessel. For this mode of formation from vessels, the name of *channelling* seems more appropriate than that of out-growth; for it appears certain that the blood-corpuscles here make their way in the parenchyma of the tissue, unconfined by membranous walls. That they do so in a definite and purposive manner, though their first issue from the vessel has appeared so accidental, may be due to the fact that, in the more regular development by out-growth, the cells of the parenchyma concur with the extension of the new vessels, by clearing away from them as they approach; so that, even before the out-growth, the way for it, or for its contents (should they happen to escape), is, in some measure, determined. The occurrence of such a process of channelling as is here indicated loses all improbability, when we remember that in insects the blood habitually flows, in a considerable and important part of its course, through sinuses, spaces, or channels without proper walls, such as are here supposed to exist only for a time. In such channels, too, it seems very probable that the blood moves in part of its course through some of the softer medullary and other morbid growths; at least, in these I have often found it impossible, with the microscope, to detect even the rudiments of such vessels as could carry their great supplies of blood." (Lecture iv, pp. 70-2.)

The structure and arrangement of the vessels of granulations,—whether those of wounds, ulcers, or the walls of abscesses,—correspond with the plan of development by "out-growth." In the earliest period of their appearance they present no indication of being formed by the fusion or any transformation of the granulation-cells; but consist of thin membrane, in which, if it be not quite simple, nuclei are imbedded, which gradually come to present the same appearances as those exhibited by original vessels of the same size. And the plan of their disposition is at first what might be expected on this idea of their mode of formation; the arched arrangement being obvious enough near the surface of the granulations, and being discernible in the deeper portions. With regard to nerves, Mr. Paget says that he has never been able to see any, either in granulations or cicatrices; and he seems almost inclined to attribute the sensibility which granulations appear to possess, to that of the excited tissues from which they spring: of their presence in cicatrices, however, we should think that no reasonable doubt can exist. From the investigations of Professor Schrœder Van der

Kolk, it appears that lymphatics do not exist either in granulations or cicatrices; although he has succeeded in demonstrating them in false membranes.

When two opposite surfaces of granulations, well developed, but not yet covered with cuticle, are brought into apposition, they have a tendency to unite, like the two original surfaces of an incised wound; this union, which was noticed by John Hunter, is appropriately termed by Mr. Paget "secondary adhesion." He gives the following examples of it, which serve alike to indicate its nature, and to point out the practical applications which may be made of this knowledge:

> "There are several circumstances in which the healing by secondary adhesion should be attempted,—such, for example, as I witnessed in a case which was lately in St. Bartholomew's. After an ordinary circular amputation of the thigh, no immediate union and no primary adhesion had taken place, and the whole interior of the stump was granulating. Had it been, as the expression is, left to granulate, or to fill up with granulations, the healing process would have occupied at least a month or five weeks more, and would have greatly exhausted the patient, already weakened by disease. But Mr. Stanley ordered the stump to be so bandaged that the opposite surfaces of granulations might be brought into close contact: they united, and in a week the healing of the stump was nearly perfected.
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> "In all such cases—and I need not say that they are very frequent—the healing by secondary adhesion may be attempted without danger, and with manifest advantage.
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> "Again: Mr. Hunter operated for harelip, and no primary adhesion of the cut surfaces ensued. He let them both granulate: then brought the granulations together, as in the common operation, and they united, and healed soundly.
>
> "Or, again: Mr. Skey, not long since, operated for fissure of the soft palate. The very edges of the wounds sloughed and retracted, and the case seemed nearly hopeless; but he kept in the sutures, and granulations sprang up from the edges of the cleft, after the separation of the sloughs: they met in the mid-space of the cleft, and coalesced, and formed a perfect scar.
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> "Doubtless, cases like these are of no rare occurrence; but I am induced to mention them as illustrations of a process of which the importance and utility are not generally considered, and which is rarely applied in practice.
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> In applying it in practice, certain conditions are essential to success; especially that—first, the granulations should be healthy, not inflamed or profusely secreting, or degenerated, as those in sinuses commonly are; 2dly, that the contact between them should be gentle but maintained; and perhaps they should be as much as possible of equal development and alike." (Lecture iv, p. 74.)

The healing of wounds by "scabbing" is next brought under consideration by Mr. Paget. This is, as John Hunter observes, the most natural mode, for it requires no art; and it is the method according to which we see nearly all open wounds healed in animals, even the warm-blooded. The formation of a scab, in fact, reduces the wound more nearly to the condition of a subcutaneous one; so that the reparative growth and formation of new tissue take place, under favorable circumstances, without any suppuration, and with scarcely any irritation; the subsequent cicatrix, too, being much more like the natural parts than are any scars formed in wounds that remain exposed to the air. It is obvious, then, that it would be very desirable to heal all wounds by scabbing, which cannot be brought to close by immediate union or primary adhesion; if it were not that in the human subject the process is far less certain than it is among animals, owing to the liability to inflammation in the wounded part, and the consequent effusion of fluid, which produces pain, compresses the wounded surface, or forces off the scab, with great discomfort to the patient, and retardation of the healing. The many instances of disappointment from this cause, seem to have led to the general neglect of this process in the treatment of wounds; but we quite agree with Mr. Paget in the belief that the observance of perfect rest and of the other means for warding off inflammation might yet make it an available auxiliary in the treatment of wounds, especially of large superficial ones (collodion answering all the purpose of a scab in the treatment of smaller ones); and we are strongly inclined to believe that the cause of failure is chiefly to be looked for in the *unnatural* condition in which by far the larger proportion of our population habitually live,—the experience of less civilized nations having abundantly shown that continual exposure to fresh air, the moderate use of wholesome food, and complete abstinence from stimulants, will place the human subject on a most desirable level with the lower animals, in regard to the healing of wounds. And the experience of travellers and sojourners among them, who have conformed to these habits, is equally favorable; showing that the variation is not due to difference of race, but to constitutional state induced by mode of life. Seeing, as we continually do, the effects of foul air, of habitual excess in diet, and of the constant abuse of stimulants, in impairing that form of the restorative process which must be regarded as the least favorable, namely, the closure of a wound by suppurating granulations, it cannot be thought improbable that to induce the more favorable method, the most perfect freedom from all such pernicious agencies should be maintained. Mr. Paget does not admit the "modelling process" of Dr. Macartney to be anything else than healing by granulations under the most favorable circumstances; and in this we have little doubt that he is right. But the surgeon should surely endeavour to ascertain what these conditions are, and to make them to apply as far as possible to every case in which he is called upon to superintend the reparative processes, so as to bring about the most favorable issue that the nature of the injury and the constitution of the patient admit of. Nothing can so much conduce to the acquirement of this knowledge, as the scientific study of the various modes of reparation; and we trust that Mr. Paget will find opportunities of carrying into extensive practical application the conclusions to which his valuable investigations on this subject have led him, and will at some future time communicate to the public the results of his inquiries.

Mr. Paget's Fifth Lecture is devoted to the history of the reparation of fractured bones. Upon a subject which has already been so fully investigated by men of distinguished ability, it might be thought that little remained for him to say; but even here we meet with important results from his original researches, which show that even the most hackneyed topics will yield an abundance of novelty in the hands of a really original investigator who interrogates nature in a right spirit. The points which he has specially chosen for elucidation in this lecture are, first, the differences between the process of the reparation of fractured bones in man and that which has been so fully investigated in the lower animals; and, second, the nature of the reparative material previous to its ossification. On the first of these points he says:—

"I must express my conviction that the description drawn by Dupuytren and others, from examinations of fractures in dogs, rabbits, birds, and other animals, cannot be applied without great deductions to the case of fractures in the human subject. True as the pictures are of the cases of the animals examined, they are exaggerations of the process in our own case. With a few exceptions, all that is written in these accounts of external and internal, provisional and definitive, callus, of the formations of cartilage and bone within the medullary tube and beneath the periosteum, can be traced only, as it were, in rudiment in the fractures of the human bones. . . . .

"For fractures in the human subject, the evidence that union is accomplished by the reparative material being placed between, not within and around, the fragments —i. e., as an intermediate, not an ensheathing callus—this evidence may be obtained by the examination of such fractures even long after they are completely healed. In as many as you like to examine you will find the new bone formed exclusively between the fragments. Whether they were in apposition, or nearly so, or wide apart, still there is no appearance of new bone being formed on the outer side of any fragment—I mean on that side which is turned away from the other fragments. And this is the case even in those instances in which there is so much displacement of the fragments, and so much distortion, that we can hardly suppose the repair to have proceeded very quietly. Neither in any of these do you find new bone within the medullary tube. It may be objected by some to these specimens, that the fragments were once ensheathed and blocked up with callus, and that it has been since absorbed. But this is not probable, seeing that in many cases there remain, on the outer surfaces of the fragments, certain marks of their original form and slight irregularities. In one of the specimens which I present, we have traces of the healing of a long fissure, which appears now as a sunken groove, making it nearly certain that no new bone was formed over it. In another, is a detached piece of the wall of a femur turned quite round, so that its periosteal surface lies on the periosteal surface of the principal fragment; yet on the outer surface of this piece (which was the inner surface of its wall) the thin plates forming the boundary of the medullary tube are still unchanged.

"But if any deem these and the like characters insufficient to prove the absence of ensheathing callus, and of callus extending into the medullary tube, yet recent specimens are not open to such doubts. I add, therefore, that (with the exceptions presently to be mentioned) in all the specimens of fracture that I have been able to examine, in the human subject, within six months of the time of the injury, there has been the same absence of provisional or ensheathing callus." (Lecture v, pp. 116-17.)

This difference appears to depend upon the quietude in which fractures of human bones are usually maintained, even independently of surgical care; and on the naturally greater tendency to the production of new bone, which animals always manifest. The influence of the first of these conditions is well marked by the fact, that the exceptional cases just referred to, in which a provisional callus is generally or naturally formed for the repair of fractures, are the ribs, whose immobility cannot be secured; whilst it is sometimes noticed also in fractures of the clavicle, in which there has been a want of repose.

As an introduction to his account of the reparative processes of bone, Mr. Paget gives a brief account of the researches which have led to the conclusion, that a large part of the later growth of bone consists in the calcification of a fibrous tissue, and is not dependent on the pre-existence of cartilage. Dr. Sharpey's observations on this subject have been confirmed and extended by that most accurate observer Kölliker; who has shown that in the increase of all the bones of the skeleton by superficial deposit, that deposit takes place in the substance of their fibrous investment. The ossicula of the ear are the only bones which attain their full size in the cartilaginous state; and the only participation which cartilage has in the development of any bones subsequently to their first ossification, is in the cases where a layer of cartilage intervenes between the principal mass of the bone and its epiphyses, this layer being in a state of continual growth, whilst the portion of it in contact with the bone already formed progressively undergoes ossification. Thus a long bone increases in *length* by means of the cartilages interposed between the shaft and the epiphyses; but it increases in *diameter* by the ossification of the fibrous tissue commonly regarded as the inner layer of the periosteum.

In the reparation of fractures, according to Mr. Paget, we may find several varieties of the ossifying process, which show us how Nature (so to speak) can take advantage for this purpose of the ordinary operations of reparation. First, it may be accomplished through perfect fibrous tissue; that is to say, the effused plasma may proceed to that grade of organization before ossification commences in it, which is believed by Mr. Paget to be ordinarily the case in the formation of new bone after fractures of the skull; or the new bone may be formed in fibrous tissue previously existing near the seat of the fracture, as in the periosteum or interosseous membranes. —Secondly, the new bone may be formed by ossification of the fibrous tissue in a rudimental state; and this may be that either of nucleated cells or nucleated blastema. The ossifying process takes place at the expense of the first of these materials when it occurs in granulations or inflammatory effusions; as is frequently seen in compound fractures, but best of all in the formation of that mushroom-shaped mass of granulations which is protruded through the medullary canal of a bone sawn across in an amputation. The ordinary mode of reparation, however, is believed by Mr. Paget to be still simpler; namely, the ossification of a nucleated blastema or rudimental form of fibrous tissue; a finely and very closely granular osseous deposit taking place in it, and gradually accumulating so as to form the delicate yet dense lamellæ of fine cancellous tissue. The nuclei of the blastema appear to become inclosed in the newly-forming bone, and to constitute, in fact, the bone-corpuscles, occupying the places which are afterwards to become the lacunæ. This we apprehend to be the mode in which all but the first imperfect network of bone is formed in even the intra-cartilaginous ossification; for after the disappearance of the cartilage-cells and the coalescence of some of the first formed areolæ, we find the spaces which are afterwards to become the Haversian canals and cancelli filled up with a "nucleated blastema," which evidently becomes converted in some mode or other into the lamellæ of osseous substance, by which the inclosing walls are progressively thickened.—But thirdly, that the reparation may take place through the medium of cellular cartilage, has been established by the observations of Miescher and others upon dogs, pigeons, &c; but Mr. Paget has not met with any examples of it in the human subject, though he considers that it might not improbably occur in favorable instances of simple fractures in children. In youths and adults he has found only varieties of fibrous cartilage, presenting various intermediate gradations of structure, from the simply fibrous to the perfect cartilaginous. In advanced life there seems to be a tendency to the production of fibrous rather than of cartilaginous tissue; and wherever the ossifying process is imperfectly performed, we find that it is by ligament, not cartilage, that the bones are held together.

"Through any of these structures, and apparently by the same method through all, the reparative new bone may be formed. It may be formed, first, where the reparative material is in contact with the old bone, and thence extending, it may seem as if it grew from the old bone; or it may be formed in the new material in detached centres of ossification, from which it may extend through the intervening tissues, and connect itself with the old bone.

"The new bone, through whatever mode it be formed, appears to acquire quickly its proper microscopic characters. Its corpuscles, being first of simple round or oval shape, and then becoming jagged at their edges, subsequently acquire their canals, which appear to be gradually hollowed out in the preformed bone as minute channels communicating with one or more of the corpuscles. The laminated canals for blood-vessels, I think, are later formed. At first, all the new bone forms a minutely cancellous structure, much like that of the fœtal bones in their first construction; but this gradually assimilates itself to the structure of the bones that it repairs, while its outer portions assume a compact laminated structure, and its inner or central portions acquire wider cancellous spaces, and a more perfect medulla. But, in regard to many of these later changes in the bonds of union of fractures, there are so many varieties in adaptation to the peculiarities of the cases, that no general account of them can be rendered. Only, specimens and drawings, one and all, show the most striking evidences of design in the adaptation of the reparative process to the particular exigencies of each case; so that it might be said, with complete truth, of every instance, that whatever is necessary,—whatever may best, under the circumstances, repair the damage,—that is done." (Lecture v, p. 121.)

The last Lecture is somewhat fragmentary, treating of the repair of injuries of cartilage, tendons, muscles, and nerves.—On the first point, Mr. Paget has no new facts to offer; the absence of any power of reproduction of true cartilage after injuries of this tissue, which are repaired either by fibrous structure or by bony deposit, being already well known. He makes the ingenious remark, however, that notwithstanding the non-vascularity of cartilage and the vascularity of bone, the replacement of the former by the latter must be considered as a process of degeneration rather than of development. This is indicated by the fact that in cartilage true interstitial growth can take place; whilst in bone no change can be effected but by superficial deposition and removal. So, again, the granular deposition of earthy matter in an animal tissue is everywhere else considered as a process of degeneration, involving as it does an approximation towards the characters of inorganic substances. And if it be objected that bone is superior to cartilage, because an osseous skeleton is proper to the most highly-developed condition of the individual, it might with as much right be replied that the atrophied thymus gland and the renal capsules almost arrested in their growth, which are normal parts of the more perfect organism, are in a state of advanced development from their fœtal state.

The reproduction of *tendons* is effected very perfectly and speedily in the lower animals; and it appears to take place as perfectly, though perhaps not quite so speedily, in man. We shall not follow our author through the details of the process of conversion of the nucleated blastema, which at first occupies the entire space left by the retraction of the tendon, into the firm fibrous cord, which connects their retracted extremities; since we have already quoted the general account of this metamorphosis when speaking of the development of filamentous tissue. "In every experiment," says Mr. Paget, "one finds cause for admiration at the manner in which a single well-designed and cord-like band of union is thus gradually formed, where at first there had been a uniform and seemingly purposeless infiltration of the whole space left by the retraction of the tendon." The new tissue may become at last, to all appearance, identical with that of the original tendon, so that even microscopic examination fails to discover the line of original section; as occurred in a case of which a preparation was presented to the College by Mr. Tamplin. But this perfect reparation is comparatively rare, and probably only occurs in very young subjects. In general the new cord looks grayer, less glistening and compact, and less regular, than the proper tendinous substance; and is dovetailed in, as it were, between the fasciculi of the latter at each divided extremity. The strength of this connective band, however, is very great; in one instance, when it was only ten days old, and no more than two lines in diameter, it supported a weight of fifty pounds. The following are the disadvantages which Mr. Paget's experiments lead him to attribute to the section of the tendo Achillis by an open wound, as compared with the subcutaneous method of division:

> "1. There were always more inflammation in the neighbourhood of the wound, and more copious infiltration of the parts, than in a subcutaneous division of the tendon in the same rabbit. 2. Suppuration frequently occurred, either between the retracted ends of the divided tendon or beneath its distal end. 3. The skin was more apt to become adherent to the tendon, and so to limit and hinder its sliding movements when the healing was completed. 4. The retracted ends of the tendon were more often displaced, so that their axes did not exactly correspond with each other, or with that of the reparative bond of union.
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> "Such mishaps were often observed in the open wounds, but were rare after the subcutaneous operations. In the cases of open wounds, they were avoided as often as the wound through the integuments healed quickly; and, whenever this happened, the case proceeded like one in which the subcutaneous division had been made. It was evident that the exposure of the wounded parts to the air did little harm, if it was continued for only a few hours,—a fact which may be usefully remembered when operations must be performed on tendons which it is not convenient to divide unseen." (Lecture vi, p. 134.)

Of the healing of *muscles* subcutaneously divided, Mr. Paget has not much new to say, except that the first tendency seems always to be to a sort of inversion or "tucking-in" of the divided ends, so that nearly all the fasciculi direct their cut ends towards the subjacent bone or fascia, as if to prevent the entire loss of the action of the muscle by securing to themselves fresh points of attachment. The reparative material is the same as in tendon, but seems to be more slowly organized; and, by its gradual contraction, it draws together the retracted portions of the muscle, so that at last they nearly coalesce.

The reunion of divided *nerves* commences on the same plan; but new nerve-tubes gradually extend themselves through the connective tissue, and restore the continuity of the divided fibres. This process of regeneration, however, is slow, twelve months commonly elapsing before any restoration of the function is observed; and we occasionally meet with examples which prove that a more immediate union may take place, when the ends of the nerves remain in approximation. Mr. Paget mentions a remarkable case which fell under his own observation; in which a boy's wrist was half cut through by a circular saw, the flexor tendons, with the radial artery and nerve, and the median artery and nerve, having been completely divided. After ten days or a fortnight, the conducting power of the median nerve began to return; but the sensations were in some degree confused, as he could not always tell (when blindfolded) whether the thumb or the forefinger was touched; and there were a few and distant small portions of the skin, from which he still derived no sensation at all. This imperfection, as Mr. Paget justly observes, is exactly what we might expect in such a mode of union; for some of the fibres might fail to unite at all, so as to leave the spots insensible from which they proceeded; others might unite with fibres with which they were not properly continuous, so as to confuse the sensations; and among them all, there might be some of which the continuity might be properly restored at once, so as completely to re-establish the function. Another case is related by Mr. Paget, in which the wrist was so nearly severed by the knife of a chaff-cutting machine, that the hand was merely connected with the fore-arm by the ulnar vessels and nerve, and the flexor carpi ulnaris, with a portion of integument about an inch wide; here, too, sensation returned in the thumb in about a fortnight, and the recovery of the motor and sensory powers of the hand was nearly complete. "I need hardly add," he concludes, "the practical rule we may draw from these cases. It is briefly that we may, with good hope of great advantage, always endeavour to bring into contact, and immediately unite the ends of divided nerves; and that we need not in all such cases anticipate a long-continued suspension of the sensation and other nerve-functions of the part."

The course concludes with a summary of the chief doctrines which Mr. Paget set himself to elucidate; but as our review will itself have placed before our readers the same material in a different form, we shall quote no more of this than the concluding paragraphs, which bring forward a new and striking argument for the constancy of specific forms.

> "Thus the constancy of both the method and the end of the reparative process may confirm us in the belief of the essentiality, we may almost say the immutability, of specific characters. For it shows that with each of all the countless varieties of visible form and structure, which mark the specific characters of living beings, there correspond peculiar internal properties of its constituent matter—properties which are capable of transmission and communication to all that can be incorporated in the living body, and of accumulation in the form of germs; and which, in any way thus transmitted, can manifest themselves in the unchanging power of constructing and composing matter according to the one determined and specific type.
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> "Thus, by watching the life of any being, in all the varieties of external circumstance and accident, and by seeing that, whatever be the deviation from its perfection into which, by the force of these, it is for a time compelled, it yet reverts to the same specific form,—or, if it fall short of it, assumes no lower one; by watching such a life, we discern an image of the constancy of the law of specific characters; we gain an assurance that matter was not, and is not now, cast without design into the world, to be shaped by the force of chance and circumstance, and to be raised or degraded by their various ebb and flow; that the living occupants of the world have been fashioned and adapted for it, not by it; and that each was from the first endowed with properties that might descend unchanged from one generation to another, and in their immutability might be a symbol of Him who at the first created them, and by whose unchanging Will and Power they have been ever since maintained and still subsist." (Lecture vi, p. 140.)

We must not omit, however, the eloquent conclusion of the first lecture; since we are sure that our readers will agree with us that it combines singular beauty and true philosophy; and in bidding Mr. Paget farewell

for the present, we have only to express the earnest hope that he will favour us with many such courses; convinced, as we are, that their influence on the character of our profession, and more especially on that of its younger members, can be for nothing but good, morally as well as intellectually.

"Let me, then, express my belief that, if ever we are to escape from the obscurities and uncertainties of our art, it must be through the study of those highest laws of our science, which are expressed in the simplest terms in the lives of the lowest orders of creation. It was in the search after the mysteries—that is, after the unknown highest—laws of generation, that the first glance was gained of the largest truth in physiology,—the truth of the development of ova through partition and multiplication of the embryo-cells. So may the study of the repair of injuries sustained by the lowest polypes lead us to the clearer knowledge of that law, in reliance upon which alone we dare to practise our profession,—the law that lost perfection may be recovered by the operation of the power by which it was once achieved. Already, in the facts that I have quoted from Sir Graham Dalyell, we seem to have the foreshadowing of the facts through which the discovery may be made.

"And let us not overlook those admirable provisions, which we may find in the lives of all that breathe, against injuries that, but for these provisions, would too often bring them to their end before their appointed time, or leave them mutilated, to complete a painful and imperfect life. We are not likely to undervalue, or to lose sight of the design of all such provisions for our own welfare. But we may better appreciate these, if we regard them as only of the same kind as those more abundantly supplied to creatures whom we are apt to think insignificant: indeed, so abundantly that, as if with a consciousness of the facility of repair, self-mutilation is commonly resorted to for the preservation of life. When the Ophiura, or any of the brittle Star-fishes, break themselves to fragments, and disappoint the grasp of the anxious naturalist, they probably only repeat what they are instinctively taught to do, that they may elude the jaws of their more ravenous enemies. But death would be better than such mutilation, if their rays could not be reproduced almost as easily as they can be rejected. The experimentalist, too, who cuts off one or the other end of any of the Anellata, perhaps only puts them to a necessity to which they are liable from the attacks of their carnivorous neighbours. Almost defenceless, and so easily mutilated, their condition, were it not for their faculty of reproduction, might be more deplorable than that of any other creature; and even their existence as species might have been endangered long ago. It would almost seem as if the species that have least means of escape or defence from mutilation were those on which the most ample power of repair has been bestowed;—an admirable instance, if it be only generally true, of the beneficence that has provided for the welfare of even the least (as we call them) of the living world, with as much care as if they were the sole objects of the Divine regard.

"Lastly, if I may venture on so high a theme, let me suggest that the instances of recovery from disease and injury seem to be only examples of a law yet larger than that within the terms of which they may be comprised,—a law wider than the grasp of science—the law that expresses our Creator's will for the recovery of all lost perfection. To this train of thought we are guided by the remembrance that the healing of the body was ever chosen as the fittest emblem of His work whose true mission was to raise man's fallen spirit, and repair the injuries it had sustained; and that once, the healing power was exerted in a manner purposely so confined as to advance, like that which we can trace, by progressive stages to the complete cure. For there was one, upon whom, when the light of Heaven first fell, so imperfect was his vision, that he saw, confusedly, 'men, as trees walking;' and then, by a second touch of the Divine Hand, was 'restored, and saw every man clearly.' Thus guided by the brighter light of revelation, it may be our privilege, while we study the things of which I have been speaking, to gain, by the illustrations of analogy, a clearer insight into the Oneness of the plan by which things spiritual and corporeal are directed. Even now, we may trace some analogy between the acts of the body and those of man's intellectual and moral nature. As in the development of the germ, so in the history of the human spirit, we may discern a striving after perfection,—after a perfection, not viewed in any present model (for the human model was marred almost as soon as it was formed), but manifested to the enlightened Reason in the 'Express Image' of the 'Father of Spirits.' And so, whenever, through human frailty, amid the violences of the world, and the remaining 'infection of our nature,' the spirit loses aught of the perfection to which it was once admitted, still its implanted Power is ever urgent to repair the loss. The same Power, derived and still renewed from the same Parent, working by the same appointed means, and to the same end, restores the fallen spirit to nearly the same perfection that it had before. Then, not unscarred, yet living—'fractus sed invictus'—the Spirit still feels its capacity for a higher life, and presses to its immortal destiny. In that destiny the analogy ends. We may watch the germ-power developing the body into all its marvellous perfection and exact fitness for the purpose of its existence in the world; but, this purpose accomplished, it passes its meridian, and then we trace it through the gradual decay of life and death. But, for the human Spirit that has passed the ordeal of the world, there is no such end. Emerging from its imprisonment in the body, it soars to the element of its higher life: there, in perpetual youth, its powers expand, as the vision of the Infinite unfolds before it; there, in the very presence of its Model, its Parent, and the Spring of all its Power, it is 'like Him, for it sees Him as He is.'" (Lecture i, pp. 1022-3.)

We shall now follow Professor Owen through his exposition of the series of phenomena which he aims at generalizing; offering, as we proceed, such remarks as may suggest themselves by way of comment or criticism on the statements which he puts forward. In doing this we trust that we shall in no instance lose sight of the claim which he has so worthily acquired to the respectful consideration of any doctrine that proceeds from him; whilst at the same time we assert our own right, which we are sure that he would be one of the last to deny us, to the maintenance of an independent opinion.

After some very pertinent introductory observations, which we are sorry that our space will not allow us to quote, Professor Owen thus enters upon his subject:

"Since the first fiat went forth, the propagation of the species of plants, animals, and mankind has been left to the operation of certain natural secondary causes, which we sum up as the 'act of generation.'

"Botanists and physiologists have observed and progressively analysed the phenomena until they have reduced them to a great degree of simplicity, the essential conditions being the same, or closely similar, in both realms of organic nature.

"With regard to the animal kingdom, the generation of which here concerns us, the essential conditions of the act appear to be a nucleated cell, and the product of a nucleated cell, with the combination of the two: the nucleated cell is the 'germinal vesicle,' and is the essential part of the ovum; the other nucleated cell is the 'sperm-cell,' and its product is the spermatozoon.

"It is essential to the development of the germ that the ovum receive the matter of the spermatozoon: it is then said to be impregnated." (p. 4.)

Now we wish to lay yet more particular stress upon this fundamental fact than our author has done, because we believe that, when reduced to its simplest and most essential form, it is the expression of a law, not merely of the animal but of the entire vegetable kingdom, and therefore of organized nature at large. We havė shown in our second article, that recent investigations in the physiology of the Cryptogamia lead to the belief that the problematical *spermatozoida*, which have been long known to exist in certain tribes of them, are in reality almost universally present, and that they perform the same office in the plants which produce them as they do in animals,—the union of the contents of a "sperm-cell" and a "germ-cell" being essential to the production of a fertile germ. This union we have seen to be effected in a different mode in the flowering plant; but still its essential nature remains the same. And in the "conjugation" of the lowest forms of Cryptogamia we still recognise the same process, although the "sperm-cell" and the "germ-cell" may be to all appearance identical in attributes. This *mixture or reunion of the contents of two cells* is, therefore, the essential condition of what is universally regarded as the "act of generation" throughout the organized world.

But we have seen that the germ thus called into existence developes successive broods of cells, which go on multiplying by the processes of *fission* or *gemmation;* though not, as it would seem, without a limit. These new cells may be all similar to each other, may become detached and live independently, and may thus (in ordinary language) be entitled to rank as distinct "individuals," as in the Palmelleæ, or other simple Algæ and Fungi. Or they may hold together, so as to produce a filament or leafy expansion, in which every cell is still but a repetition of the rest, as in the Confervæ and Ulvæ. Or by slight variations in their mode of multiplication, they may develope a fabric possessing a definite form and a certain degree of heterogeneousness of structure, as in the higher Algæ. Or, to proceed at once to the highest types of vegetation, they may increase for a time by this method of multiplication, so as to form a homogeneous "germinal mass;" this, in its further development, shall first take the form of an almost homogeneous leafy expansion, the single or double "cotyledon;" and from this a heterogeneous fabric, composed of numerous dissimilar parts, may be gradually evolved by a series of morphological and histological changes. Further, the single plant or "phyton" thus developed, extends itself in many instances by the gemmiparous multiplication of similar "phytons," every one of which is capable of existing separately, and is hence to be regarded as entitled to the rank of individuality. But in all these cases the power of cell-multiplication seems to have a limit. If this cannot be definitely assigned in regard to the multiplication of cells by fission, in the inferior Cryptogamia, it is obvious enough in the higher forms of vegetation. The "annual" plant dies down; the so-styled "perennial" gradually loses its capability of producing new buds; and the restoration of the expended power can only take place through an "act of generation." The union of the contents of the "sperm-cell" (pollen-grain) and of the "germ-cell" (embryonal vesicle) again takes place in the ovule; and the result is a vigorous germ, capable of giving origin to a similar multitudinous progeny, in virtue of the new creation of what Mr. Paget would call "germ-power," which this process has called into existence.

These facts, about which we think that there can be no dispute, ought to lead physiologists towards some definite understanding as to the meaning of the term "generation," when used to signify not the *act* but the *result* of the act, not he creation of a new stock but the stock thus called into existence. Now since physiologists have always been accustomed to speak of the sexual process in animals as "an act of generation," and to regard the evolution of new parts to any extent, which results from the multiplication of the germ-cells by fission or gemmation, and from the subsequent transformations which their descendants may undergo, as an "act of development," we see no good reason for departing from the ordinary signification of these terms, even when this evolution of new parts should proceed to the extent of producing organisms that are capable of maintaining an independent existence. The process by which the individualized cells of a Palmella are called into existence, differs in no respect from that by which the cells of the germinal mass of the higher plant or animal are multiplied, whilst retaining their connexion with each other. Anatomically the homology is perfect; physiologically there are these differences,—that the former are capable of maintaining a separate existence, whilst the latter are not,—that the former are complete in themselves, whilst the existence of the latter is preparatory to that of something else which is to be evolved from them. But so long as the multiplication of parts takes place after a method essentially the same as that which is followed in the evolution of the primal structure, so long must we regard it as entitled to be regarded as a process of "development" rather than as one of "generation;" and this whether the new structure is formed in continuity with the old, or whether it originates in a *gemma* detached at a very early period of its existence; for both these methods, as we have heretofore seen, are but modifications of one and the same, consisting in the mere *subdivision* of the contents of a parent-cell, and in the individualization of each of the portions thus separated. They are, therefore, in complete antagonism to the process of "generation" properly so called; which consists in the *reunion* of the contents of cells thus separated, the individuality of two being merged again into one, to form the germ.

If, then, there be such an antagonism between the two operations,—"development" by subdivision, and "generation" by reunion,—it is surely unwise to confound them together by the indefinite or perverted application of terms; and notwithstanding all that has been said in reply to our criticisms on the theory of "alternation of generations," we still hold to the assertion that the term "generation" can only be rightly applied to the entire phase or series of phases of life which intervenes between one "act of generation" and another; and that however remarkable may be the multiplication and succession of so-called "individuals," this multiplication and succession only repeats on a more extended scale the same processes of "development" as those which are in operation in the multiplication of the individual cells of the Palmellæ, the extension of the filament of a Conferva or of the frondose expansion of an Ulva, and the evolution of the first phyton of the flowering plant from its primordial cell. To use Mr. Paget's language, it is one and the same "germ-force" which is in operation in all these cases; a new "germ-force" can only be created by a new "act of generation;" and all that is the product of the continued action of the same "germ-force" must either be distinguished as belonging to the same "generation," or some other term must be invented to designate it.

It may be said that this is a question rather of words than of realities;

but it is a question in which the right use of terms will be found singularly conducive to the attainment of clear ideas; whilst the indefinite or perverted use of terms will prove, we are confident, a most especial obstacle to the perception of what are really analogous phenomena. Those who designate every fresh bud of a perennial plant as "a new generation," because it is capable of maintaining an independent existence, must needs do the same with regard to every cell of a Palmella or every joint of a Conferva; and we do not see how they can refuse the designation to every successional group of cells which constitutes the germinal mass of higher plants, and multiplies by binary subdivision, or which is concerned in the subsequent multiplication of its parts. It is according to the laws of *homology*, that such designations must be given, in order that they should mean anything consistent and universally applicable; the *physiological* limits of individuality are hard to draw, and often artificial. When, we may ask, does individuality commence in a growing bud? Is not its acquirement of that character a process so gradual, that it cannot be said when it first comes to possess it? How, then, can any definite commencement be assigned to the "new generation," if individuality is to be the sole criterion? Such is a specimen of the difficulties (to which we might add many more) that are involved in the application of the term "new generation" to the multiplication of individuals by any modification of the process of subdivision; instead of restricting it to what we regard as its only legitimate meaning, the origination of a new germ by an "act of generation;" leaving the term "development" to express the whole process by which that germ may be evolved into a single individual, or may give origin to many, whether contemporaneously or successively. But our opponents are quite welcome to use the terms in any sense they think proper, if they will only define that sense in a manner that shall admit of no obscurity, and will keep clearly in view the difference between *sexual* reproduction, or reproduction by the *reunion* of cells, and *non-sexual* reproduction, or multiplication by the *subdivision* of cells.

We may be thought to have wandered far from our subject, in thus parting company with our author at the outset; but we believe that we shall be able to show that the views which we have thus endeavoured to develope, whilst in the main harmonious with those of Professor Owen, tend to simplify and extend our conception of the character of the phenomena, which he aims at reducing to a formal and definite expression. After sketching briefly the mode in which the successors of the first-impregnated germ-cell are gradually metamorphosed into the fabric of the complete animal, he thus continues:

> "Not all the progeny of the primary impregnated germ-cell are required for the formation of the body in all animals: certain of the derivative germ-cells may remain unchanged and become included in that body which has been composed of their metamorphosed and diversely combined or confluent brethren: so included, any derivative germ-cell or the nucleus of such may commence and repeat the same processes of growth by imbibition, and of propagation by spontaneous fission, as those to which itself owed its origin; followed by metamorphoses and combinations of the germ-masses so produced, which concur to the development of another individual; and this may be, or may not be, like that individual in which the secondary germ-cell or germ-mass was included.
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> "It has been found that in proportion as the subjects of anatomical investigation descend in the scale of animal life, the number of the derivative germ-cells and

nuclei which retain their individuality and spermatic power is greater, and the number of those that are metamorphosed into tissues and organs less." (pp. 5-6.)

This last statement is exemplified, in the first instance, by reference to the lower forms of vegetable life in which the fabric consists exclusively of cells; Professor Owen might have added that even in the highest, it is from the cellular portions of the fabric that new growths invariably arise. As an example of the simplest form of animal life, he cites a curious parasitical animal, the *Gregarina*, lately studied by Kölliker, which seems essentially to consist of a single cell, whose animality is accorded on the ground of the vital contractility of its tunic, and the solubility of its cell-wall in acetic acid. We can scarcely regard either of these characters as sufficient for the positive determination of the animality of this curious being; since the cell-walls in many plants possess vital contractility, whilst their primordial utricle (the essential cell-wall) is composed of a protein compound. The strongest indication, as it seems to us, of the animality of this parasite, is the adumbration or shadowing-forth which it exhibits of the characters of the higher entozoa, of whose animal nature no doubt can be entertained. The polygastric Infusoria are regarded by Professor Owen as still essentially retaining the character of parent-cells, although their granular contents have been developed into secondary cells, and some of these have combined and coalesced to form special organs. The Hydra is described as having its body chiefly made up of "retained and unaltered nucleated cells and nuclei, which are identical in all recognisable characters with the progeny of the primary impregnated germ-cell, and which are ready, therefore, when favorable circumstances concur, to repeat the acts of assimilation and spontaneous fission, and, each individually, thus to lay the basis of a new polype."*

These 'derivative germ-cells' are said by Professor Owen to be retained without change, also, in the compound hydriform Polypes and in the parenchymatous Entozoa; a smaller proportion in the Acalephæ and cavitary Entozoa; also in the last segment of the Nais, and the young of other Annelides. "We find," he continues, "derivative germ-cells, and masses of nuclei like those resulting from the final subdivision of germ-cells, retained unchanged at the filamentary extremities of the branched uterus forming the ovaria of the larval Aphides." So, too, in all those cases in which development is arrested at certain stages, in such a manner as to enable the embryo to maintain its own existence for a time, as if it were a completed individual, and to take in a store of material which shall serve for its further development, we find a certain proportion of the 'derivative germ-cells' entering into its constitution,—this being larger in proportion to the earliness of the stage at which the development has been arrested to form the 'larva.' Now in all these cases, we find that reproduction may take place by *fission*, or by *gemmation;* two processes which we agree with Professor Owen in regarding as one and the same. The essential condition of both is, according to him, "the presence of the pellucid nucleus of a derivative germ-cell, as the centre from which all the processes in the formation of the new individual radiate." This may be true to a certain extent; but it seems to us too limited an expression, since it will not apply to the corresponding process in plants. As we have already shown, the *entire endochrome* in the inferior algæ seems concerned alike in the processes of fission and gemmation; the nucleus, which seems to possess a concentration of this power where it exists, being the instrument of it in the higher forms alone of the vegetable cell.

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* We cannot help imagining, from the description of which this is an extract, as well as from the account and figure given by Professor Owen, in his 'Lectures on the Invertebrata,' that he rests this statement chiefly upon the assertions of Corda, which have not been confirmed (so far as we are aware) by more trustworthy observers, furnished with better instruments. To ourselves, the tissue of the Hydra and of its marine congeners has always appeared rather to resemble the "nucleated blastema" of Mr. Paget; and we have recently learned, that Professor Ecker of Basle has formed a very similar idea of its character. He has recognised in the parietes of the stomach three layers, nearly similar to each other in character; each consisting of an amorphous, homogeneous, semi-transparent substance, whose continuity is interrupted by cavities, which inosculate, so as to form an irregular network, in which fluids are seen to move. This network is continuous from one layer to another. The external layer does not present the epidermis described by Corda. The central is distinguished from the external by the green granulations which it includes. The internal is distinguished by brownish granulations, but is not covered with the ciliated epithelium which other anatomists have described. (Ann. des Sci. Nat., Dec. 1848.)

Professor Owen then proceeds to notice some of the principal cases in which reproduction by fission or gemmation alternates with the sexual process; dwelling, of course, particularly upon those curious phenomena which have been grouped together under the title of 'Alternation of Generations.' Of these we particularly considered some in a former article;* and as we purpose to take up others ere long, we need not now dwell upon them. We are very glad to find ourselves strengthened by our author's authority in the fundamental position which we there took up,—namely, that there is no "alternation" in the products of the true "generative" process, but that the intermediate forms are uniformly the results of a fission or gemmation independent of sexual congress, to which he gives the appellation "Parthenogenesis." To this term we have no objection whatever, except that it tends in our apprehension to indicate a relation between two things which are not only *different* but *opposed.* For if we are correct in the view we have taken of the essential nature of these acts, the *reunion* of the contents of the "sperm-cell" and "germ-cell" which is characteristic of the one, and the re-creation of "germ-force" thereby effected, are altogether opposed to the act of *separation* whereby cells are multiplied, and the "germ-force" distributed among them, and thereby progressively weakened.—Again, to speak of a "larval polype," budding off Medusæ, as a "virgin" implies that it is a *female.* Now our notion of a "female" implies the antagonistic action of a "male." The female furnishes the "germ-cell," which is not fertile until it has received the contents of the "sperm-cell." Surely a single cell which is propagating itself by spontaneous fission or gemmation, is neither male nor female; why, then, should we give the latter appellation to a polype which is forming gemmæ without sexual congress? The Hydra produces gemmæ at one period of its existence, indifferently, as it would appear, from any part of its body; at another time it forms germ-cells at one spot and ova at another, by the congress of which a new generation is commenced; if, then, the individual Hydra is to be regarded as a female in virtue of its producing gemmæ, its sex must be metamorphosed to enable it to develope spermatozoa. We still hold to the analogy which we formerly put forth between a zoophyte and a plant.† The gemmæ of the Hydra, or of any budding polype, we take to be the homologues of the leaves, whilst the true generative organs represent the flower. Whilst the plant is merely putting forth its leaves, nobody considers it as possessing sex; it is simply extending itself by cell-multiplication. It may afterwards develope both kinds of sexual organs, or only one, and this one may be male; how, then, can the plant have been properly female in the earlier stage of its life?

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* On the Development and Metamorphoses of Zoophytes, Vol. I, p. 183.

† We meant thereby not a "tree" or composite plant, as represented by Professor Owen (p. 51), and by Professor E. Forbes (Monograph on the Naked-eyed Medusæ, p. 87); but a plant of simplest construction, as described in the next paragraph.

Professor Owen does not seem insensible to the justice of this comparison; but he escapes from its consequences by representing even the simplest plant as an assemblage of individuals. Every leaf, every petal, every stamen, every carpel, is in his apprehension entitled to rank as a distinct being; notwithstanding that the sum of the actions of all of them is necessary to make up the idea of the life of a Plant, and that neither leaf, petal, stamen, nor carpel can maintain an independent existence, except in a few rare cases. Such cases, however, he adduces in support of his argument; urging that, because the leaf of the Bryophyllum and of some other plants can develope buds from its crenated margins, each leaf must be considered as a distinct individual. Now if we look to what a part can develope or *become*, instead of to what it *is*, as our test of individuality, we shall find ourselves reduced to a state of great perplexity. For not merely the entire leaf of the Bryophyllum, but a mere fragment of a leaf (as we have ourselves witnessed), can produce buds under favorable circumstances; and consequently, even the single leaf is to be regarded as an aggregation of individuals. So, again, as the Hydra can produce buds not merely when its body is entire, but as, when it has been minced up into thirty or forty pieces, every fragment may be developed into a new Hydra, we must, if Professor Owen's reasoning be adopted, regard the single Hydra as really an aggregation of individuals. The fact is, as we have already remarked, that the limits of individuality are hard to define; and whatever test we set up will lead, when rigidly applied, to results for which we are scarcely prepared. And hence we think that we should not base our fundamental notions of the reproductive process upon the question, whether or not the several parts of a composite structure are to be considered as distinct individuals? If we reduce our "ideal plant" or "phyton" to its simplest possible elements, the "idea" will still involve as its components, not merely the leaf, but the root, and not merely the leaf and root, but the pollen-grain or the ovule. All these parts, which subdivide the functions performed by the single cell of the Protococcus, must be present to make up a complete phyton; and although a leaf or a fragment of the leaf may be so "potentially," because it is capable of developing the rest, it is not so "actually," any more than a fragment of a Hydra is a complete Hydra.

A considerable portion of Professor Owen's second lecture is devoted to a comparison between his doctrine of "Parthenogenesis," Steenstrup's doctrine of the "Alternation of Generations," and the modification of the latter which we offered in the article before alluded to. In many respects he agrees with us,—fully recognising the importance of the distinction between the "oviparous," or "sexual," and the "gemmiparous" forms of reproduction, and admitting all the applications which we made of the latter to the relation of the Polypes and Medusæ. But he considers that the single or multiple offshoot which results from each act of gemmiparity or "parthenogenesis," should rank as a "new generation," and that the parent or "stock" should be regarded as a "female." And he brings every case of the kind under his general statement, that the condition of the parthenogenetic process is "the retention of certain of the progeny of the primary impregnated germ-cell, or, in other words, of the germ-mass, unchanged in the body of the first individual developed from that germ-mass, with so much of the spermatic force inherited by the retained germ-cells from the parent-cell, or germ-vesicle, as suffices to set on foot and maintain the same series of formative actions as those which constituted the individual containing them." (p. 72.) These positions we shall now successively examine.

Professor Owen lays especial stress upon the case of the Aphides, as demonstrating the female sexuality of the parent-stock, and as proving the right of the successive broods to the title of "new generations;" and he seems to think that he either forces us to deny the parallelism of the phenomena, or proves the fallacy of our position by a *reductio ad absurdum.* Now we by no means deny the parallelism of the phenomena; on the contrary, we had previously been ourselves prepared to maintain the gemmiparous nature of the non-sexual production of the successional broods of aphides, until informed by Professor Milne-Edwards that what he believed to be true *ova* had been found in their ovaries. We are quite ready to accept Professor Owen's statement, that "the contents of the ovarian tubes differ in the oviparous and in those of the viviparous females; inasmuch as they contain oval masses of granules or nuclei comparable to the germ-mass in its state of minutest subdivision, in the virgin aphides, and not ova with the germinal vesicle, as in the oviparous females;" and to recognise with him the essential conformity between the production of the successional broods of aphides, and the budding of polypes from the sides of the Hydra. But we still maintain that the so-called "virgin" or "viviparous" aphides are *not* females; and we shall be much disappointed if we do not prove this even to the satisfaction of Professor Owen himself. For we must not here look to the conformation of the reproductive apparatus generally, but to its *essential* characters, as no one knows better than the learned Professor. When he meets with a Medusa, for example, with certain plicated membranous folds in the interior of the four chambers that surround the mouth, on what does his determination of its sex depend? Simply upon the fact, that imbedded in these membranous folds are either "sperm-cells" or "germ-cells." If the former, the animal is a male; if the latter, it is a female. The apparatus which ministers to the development of these essential parts is here morphologically the same in both; a clear proof that it alone does not afford a proper sexual character. In higher animals we usually find this apparatus assuming different forms, according to the purpose for which they are developed; but these different forms do not constitute the essence of the sexual character, which still lies in the production of "sperm-cells" on the one hand, or of "germ-cells" on the other. It is quite true that the reproductive apparatus in the viviparous aphides morphologically resembles that of the oviparous or true females; but it is easy to see the purpose of this, namely, the maturation of the gemmæ by nourishment supplied from the stock; and this teleological and purely *formal* resemblance does *not* involve that which is *essential.* For if we call a certain apparatus an ovarium because it produces ova, then it is *not* an ovarium if it does *not* produce ova; and it is almost as much a misnomer to apply this term to the reproductive apparatus of the viviparous Aphis, which developes not ova but gemmæ, as it would be to give it to the chambers surrounding the mouth of a male Medusa, which contain not ova but spermatic cells. If the viviparous Aphis budded forth its gemma on the exterior of its body, no one would any more think of calling it a "female," than of applying this term to the budding Hydra. It is only because the bud is formed at the top of a set of tubes which resemble in *form*, but not in *essence*, the ovarium of the true female, that Professor Owen, in common with other naturalists, assigns to the gemmiparous Aphis the female character. That he is led, by his own application of this doctrine to the vegetable kingdom (p. 45), to attribute the female character to a budding plant whose sexual organs may be male only, may perhaps be regarded by some physiologists as a *reductio ad absurdum* of *his* doctrine of sexes.

But Professor Owen further argues, that "the growth by cell-multiplication producing a bud, instead of being 'altogether distinct from' the growth by cell-multiplication in an egg, is essentially the same kind of growth or developmental process" (p. 45); and he seems to think that by overlooking this fact, we have made the distinction between the oviparous and gemmiparous processes a great deal stronger than it really is. Now we have nowhere said or implied, as might be supposed from his use of our words, that the development of a new individual from a *gemma* by cell-multiplication differs in any essential respect from the developmental process which is seen in the formation of the germinal mass by the fission of cells in the ovum. On the contrary, we have always regarded them, and have been accustomed to describe them, as one and the same; and we lay great stress upon this correspondence, for reasons which will presently appear. It is between the *origin* of the first cell of the *gemma*, and that of the first cell of the *ovum*, that we hold the essential difference to consist; and the importance of this difference appears to us to have been entirely overlooked by Professor Owen, who evidently aims at approximating the two processes, so as to strengthen his argument for the *female* character of any gemmiparous being. If we are right, however, in the statements we have already made on this point, we think that no doubt can remain that the two processes are, as we formerly affirmed them to be, "altogether distinct," the one consisting in division, the other in reunion; at the same time, we admit that we did not on that occasion *define* this difference as clearly as we have now done; simply because our perception of its essential character was not then so precise as it has since been rendered by the progress of discovery in vegetable physiology.* If, on the other hand, any of our facts are inaccurately stated, we shall gladly receive any correction that may bring them nearer to the truth.

Instead, then, of calling the "viviparous" Aphis a "female," we simply speak of it as "gemmiparous," and deny it any real sexual character, in spite of its possession of organs having the *form*, though not the *essence* of the female ovaries. And this denial is fortified by the consideration, that if we assign the female character to any organism that produces new individuals by gemmation, we must attribute it not merely to the budding Hydra but also to the budding leaf-plant; not merely to the entire leaf of a Bryophyllum, but also to a fragment of it; and, descending to the simpler plants, to the extending filament of a Conferva, or to the subdividing cells of a Palmella; and hence, in fact, to *every* cell undergoing fission, or extending itself by gemmation, and thus even to the component cells of the germinal mass. The employment of the designation "female" after this fashion, is altogether a perversion of its *usual* meaning; for it is at present applied to *one* of the *two* sexual organisms, by the concurrence of which in the "act of generation" a new germ is produced; and is the antagonist, or rather the complement, of "male." Professor Owen, on the other hand, would regard every "reproductive" individual as a female; and looks for its male complement in the spermatic force derived from an *antecedent* act of generation. But this latter assumption seems to us rather hypothetical; and we shall presently show that we have no reason to regard any such remnant of the original spermatic force as being more specially concerned in the production of gemmæ, than it is in that reparation or reproduction of parts which may occur in the male among higher animals, just as well as in the female.

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* Professor Owen has done us the honour to acknowledge the justice of our remark upon the inaccuracy of the statement which appeared in his published Lectures, that the polype-bud of the Hydra is developed from an *ovum*. We, on our parts, are quite ready to admit that our definition of the essential character of gemmation as that of "continuous development from the parent," was imperfect, and open to the criticisms which he has passed upon it; and we thank him for having given us the opportunity of putting forward our doctrine in a less exceptionable form.

In regard to the question whether or not the successive broods of Aphides which result from the repetition of the process of gemmation constitute so many "generations," we freely admit that we may appear to refuse to the term its natural sense, and are taking a position in which we stand alone, in maintaining that they are *not* entitled to that rank. This, however, is more a question of mere words than the other. We think it most philosophical to apply the term "generation" to the whole product, whether single or multiple, contemporaneous or successional, of an "act of generation;" because we find that these phenomena can be best compared, and their true relations determined, when they are arranged according to the principle involved in that idea. The case of the Aphides certainly appears an extreme one; but it does not essentially differ from that of the Hydra. The buds are internal instead of external; and they are developed and nourished in the interior of the body of the parent, so as to arrive at maturity before they come forth, instead of sprouting from the exterior, and obtaining their own food even before their detachment. And through the Hydra we are conducted to cases as extreme in the other direction; for it would be easy to show, that if the successional polypes propagated by gemmation from a single Hydra are so many "generations," so also are the successional cells produced in the polygastric Infusoria and the simplest Algæ, by the subdivision of a single individual; and if these be admitted to the title, we do not see how it can be refused to the similar succession in the filament of a Conferva or the frond of an Ulva; from which the transition is direct (as Professor Owen has himself urged) to the self-multiplying successional cells of the germinal mass; and thence onwards to every case of cell-multiplication in the growing organism. If Professor Owen, and those who take the same view, should object that the "new generation" only comes into existence when new "individuals" are produced, we have not only to reply (as we have already done) that the limits of individuality are too difficult to define, for the establishment of so important a distinction as that between one "generation" and another;

so that perhaps no two of them would agree as to the point at which the line should be drawn in the series we have just indicated; but we will go further, and assert that in the increase of the germinal mass, up to the stage at which its component cells first begin to exhibit a difference of attributes, there is really as complete a generation of "individuals" as in the propagation of the Polygastrica by spontaneous fission. For we must look to this mass as it *is*, not at what it is to *become*. We see a single cell subdividing itself into a pair, this pair subdividing itself into four, the four into eight, and so on, precisely as in a Paramæcium or a Protococcus. And each of these new cells is as much entitled to the rank of a distinct "individual," as are the component monads of a Volvox, or the single cells of the higher Palmellæ, in both which cases they are held together by a common investment, and a definite form is given to the cluster by the regularity of the mode of multiplication. For whilst the attributes of all the cells of the germ-mass remain alike, neither can be regarded as in any way dependent on the rest; and we have no reason to doubt that each might go on living and multiplying when isolated from them, provided that its other conditions of existence were not interfered with.

If, then, the successive broods of viviparous Aphides are to be regarded as so many "generations," so also are the successive broods of cells in the "germinal mass;" and if the former case present a *reductio ad absurdum* of our denial of the title to organisms produced by gemmation, the latter does the same in regard to our opponents' application of the term to such successional productions. As there are cases, then, in which either mode of using it runs counter to our ordinary notions, as well as to our accustomed phraseology, we seem left free to choose between them; and we think it will be found that as, in Professor Owen's apprehension as well as our own, all the phenomena of reproduction which intervene between one "act of generation" and another are to be regarded as *developmental results* of that act, whether a single individual only, or a succession of individuals be thus evolved, it will be most convenient and precise, as well as most philosophical, to regard all these results as constituting but *one generation*, and to speak of the formation of new and independent organisms by gemmation as the *reproduction of individuals*, just as we speak of the restoration of members that have been removed as the *reproduction of lost parts*.

We have now to consider the most important of the doctrines sustained by Professor Owen in this publication; and the one which may be regarded as particularly his own; namely, that this *reproduction of individuals* by gemmation, or parthenogenesis, essentially depends on the retention of a portion of the primary germ-mass in the body of the first individual developed from it. He does not profess to have substantiated this affirmation by direct observation in any considerable number of cases, but contents himself with the citation of a number of instances in which the part whence the gemma springs has an obviously cellular composition, and with applying to these the interpretation which he thinks himself justified in putting upon his observations on the Aphides.

"One sees," he says, "such portion of the germ-mass taken into the semi-transparent body of the embryo Aphis, like the remnant of the yelk in the chick. I at first thought that it was about to be inclosed within the alimentary canal, but it is not so. As the embryo grows, it assumes the position of the ovarium, and becomes divided into oval masses, and inclosed by the filamentary extremities of the eight oviducts. Individual development is checked and arrested at the apterous larval condition. It is plain, therefore, that the essential condition of the development of another embryo in this larva is the retention of part of the progeny of the primary impregnated germ-cell." (p. 70.)

Now we may admit that in the case of the first larval Aphis which results from the true generative act, a portion of the *original* germinal mass is thus retained; but can the same be said of the succession of individuals which take their origin in this first product? It can scarcely be said that the germs which *their* oviducts contain are the very cells supplied by the original germinal mass, and included without metamorphosis in the developed larvæ; they are, indeed, the descendants of those cells; but so also were all those which have been metamorphosed into the various tissues of the larval Aphis itself. Hence what is *peculiar* in the character of the parts from which the germs arise, is rather that they retain the original cellular *type,* than that they are the *actual residue* of those cells which directly inherited any portion of the spermatic influence. And this, we apprehend, will be a truer expression of the fact in its application to the vegetable kingdom. For whilst we find that the cellular parts of a plant are those which give origin to buds, we have no reason to regard these parts as the permanent residue of the original germinal mass; for this is almost entirely expended in the formation of the cotyledons; and although the first buds may originate from what remains of it, we cannot say the same of the subsequent ones, which, like the successional broods of Aphides, are the products of the developmental processes of the first. Nor do we see any proof that in any of the other instances cited by Professor Owen, there is a retention of the actual cells of the germinal mass that resulted from the generative act; for even if this be admitted with regard to the Hydra which first sprung from the egg, it can scarcely be supposed that such cells are shared amongst all the polypes which shoot forth from it, every one of which seems endowed with the same amount of budding power as the first. We are not sure that Professor Owen himself really needs to have his attention called to this point, for the instances we have cited can scarcely have escaped his consideration; but his language appears to us likely to lead his readers to the conclusion, that in *every* case in which a gemma is formed, that gemma originates in one of the cells of the germinal mass, which originally received the spermatic influence, and which has escaped metamorphosis into the tissues of the individual organism, so as to be ready for development into a new one. The more correct expression of the phenomena, as we have hitherto considered them, appears to us to be, that the gemma always originates from a cell; and that this cell may either be one of those constituting the original ovum, or may be a descendant of one of these,—its essential feature in either case being, that it has escaped those histological transformations, which seem incompatible with the retention of the original reproductive power which is proper to the cell. There is no adequate reason, therefore, to consider these cells, any more than those which develope the first individual, as the special inheritors of the "spermatic force."

But we are not sure whether even this statement will hold good with that universality which is essential for the establishment of a law. For we cannot see how any line can be drawn between the phenomena of the gemmiparous reproduction of entire individuals, and those of that reproduction of parts which occurs in beings that are unable to develope the whole. We need not recapitulate the arguments by which Mr. Paget has so well demonstrated the identity of their essential conditions. In fact, Professor Owen himself appears to admit it; for he cites the reproduction of claws in the lobster, which is stated by Mr. H. Goodsir to commence from little masses of cells at the basal end of the first joint, in proof that "the reproduction of parts in higher animals depends on pre-existing cells retained as such." No one, we think, can doubt that the regeneration of the claw of a lobster, or of the leg of a salamander, are phenomena of the same order with the production of an entire polype by gemmation, who will attentively consider the facts determined by experiment upon the Hydra. For it there appears that nearly every portion of the body* may reproduce the whole, alike in the spontaneous act of gemmation, and in the reparation of injuries; and this after a precisely similar fashion in both cases. On the other hand, the restoration of a portion of bone, tendon, nerve, or areolar tissue, in the higher animals, which cannot regenerate entire limbs, being phenomena of the same order with the reproduction of claws in the lobster, are to be referred to the same category of "gemmation;" and there is nothing in the history of the reproduction of these tissues, which lends the least support to the doctrine that any "residual cells" are requisite as the starting-point. For we here find that the tissues most readily regenerated are *not* those in which the primitive cellular type is most completely retained. Thus we have seen that cellular cartilage is not regenerated; while fibrous tissue, whether in bone, tendon, or areolar structure, is re-formed with great rapidity and completeness. And it also appears that new structures need not originate in the *solid* fabric of the original; for the observations of Mr. Paget render it nearly certain that the *fluids* effused from the vessels, after the receipt of an injury, are capable of giving origin to tissues, and this quite irrespective of any floating cells which may exist in them; so that we quite lose sight of the original germ-mass in these instances. Moreover, of all the organs in the human body, the thymus and thyroid glands, and the supra-renal capsules, are those most directly derived from it; being essentially infoldings of the germinal membrane, as Professor Goodsir has demonstrated (Phil. Trans. 1846); yet we do not find that these parts are distinguished for their regenerative power, even though the cellular type predominates in them. Hence when the phenomena of reparation are taken into the account, we find that the reproductive power cannot be restricted to parts retaining the cellular type, far less to parts in which any residue of the original cells of the germ-mass can be said to exist; and it cannot be even shown that parts which most completely retain the cellular type, or which are most nearly related to the primitive germ-mass, are those in which the reproductive power is the greatest. Consequently the doctrine of Professor Owen fails in its application to the reparative processes among the higher animals; although, if it possess the universality attributed to it by its author, it ought to be as true of them as it is of the production of independent gemmæ in the lowest and simplest.

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* Professor Owen states, on the authority of Roesel and others, that the budding process pretty constantly takes place from a certain portion of the body of the *Hydra fusca;* which he affirms to be the part "where those nucleated cells are assembled in greatest number, which most closely resemble the secondary or derivative germ-cells in the ovum." Though he cites this as a correction of our statement, that "buds *may* arise from any part of the parent structure," yet it is not so in reality; since we by no means dispute that buds *more frequently* arise from the part in question than from any other, which is all that we understand Roesel to assert. An examination of Trembley's accurate observations and varied experiments will establish our position, we venture to assert, beyond all question. Trembley particularly states, that when a wound was made in the body of a well-nourished Hydra, new buds were peculiarly apt to spring from the cut edges; as if the formative effort had gone on to the production of new individuals, instead of being restricted to the reparation of the original.

We shall, in conclusion, briefly sum up the points of agreement and of difference between Professor Owen and ourselves.—1. Starting from the primary fertilization of the "germ-cell" by the "sperm-cell," all the phenomena of formation and reproduction which take place up to the next act of generation are regarded by us both as referable to the same category, that of cell-multiplication by fission or gemmation, which may be termed the *developmental process.*—2. But when this formative process goes to generate a distinct and independent individual, it is considered by Professor Owen as worthy of being ranked under a separate designation, that of "Parthenogenesis;" the stock or being from which the gemma originates is regarded by him as a female, and the gemma itself is ranked as a "new generation." To us, on the contrary, it seems that the stock is of no sex; that the process of gemmation is one antagonistic of sexual production, and ought not to be assimilated to it in any way; that the term, "Parthenogenesis" is inappropriate; and that the term "generation," may be best made to include all that intervenes between one "act of generation" and another.—3. By Professor Owen it is affirmed that the essential condition of the gemmation of new individuals among the lower tribes, or of the reproduction of members in the higher, is the retention of cells resembling those of the germinal mass, and specially impressed by the "spermatic force." To us, on the contrary, it appears that the most that can be said is, that gemmation or reproduction most readily proceeds from unmetamorphosed cells; but that there is no sufficient proof that these cells are more immediately related to those of the germinal mass, or have received any more special share of the spermatic influence, than those which have contributed to the development of the individual fabric: whilst, in the reparative processes of the higher animals, there is not the least proof of dependence on any such cells; the "germ-force" (to borrow Mr. Paget's expression) being diffused through the formative fluids, at least as much as among the solids.

We trust that our readers will not think that we have dwelt at too great length upon a question which may seem too abstract to be of any practical utility. But those of them who may have followed us through this article, will have seen how closely the true comprehension of the phenomena discussed by Professor Owen, is linked on to the scientific understanding of the subject of which his colleague has so ably treated. And if, whilst keeping in mind the essentially practical bearing of the latter, they reflect upon that immortal axiom of Bacon, which sets forth that "the office of science is to shorten the long turnings and windings of experience," we think that no apology will be needed by them for our full discussion of the doctrine of "Parthenogenesis."

ART. IX.

Die Erkenntniss und Heilung der Ohrenkrankheiten. Vom Dr. W. KRAMER.
The Nature and Treatment of Diseases of the Ear. By Dr. KRAMER.
Second Edition.—*Berlin*, 1849. pp. 860.

WE may enumerate among the numerous set-offs to the evils springing from the revolutions of 1848 the production of an improved edition of a very good book; for Dr. Kramer informs us, that having armed in the cause of order, and found his efforts ill-supported by his fellow-citizens, he sought in literary occupation a solace for the consequent mortification and a means of recruiting his bodily strength. If, however, we may judge by the vigorous attack he has made upon all other writers upon the subject, the spirit militant still clove to him, the scene of the battlefield being alone shifted. The greater part of those who have written since his first edition, have been dealt with even more severely than his predecessors were in that; and for this procedure he has unfortunately too good an excuse in the fact, that no region of the body has its maladies so empirically and ignorantly treated as this. The author of a treatise which set forth the true method of investigation, and substituted something like physical demonstration for vague hypothesis, may be pardoned for expressing himself somewhat warmly, when, on taking a retrospect a dozen years afterwards, he finds little or no progress to have been realized. With respect to our own country, Mr. Pilcher's work is the only one having the slightest pretension to a scientific character, and even that is a rather meagre production; while of most of the other writings which have appeared of late, with the exception of Mr. Wilde's papers, we feel heartily ashamed. According to Dr. Kramer, things are little or no better in Germany and France; and no inconsiderable portion of the present edition is occupied in exposing the groundlessness of the various hypotheses put forward, and of the so-called practical procedures founded on them. Judging from the amount of space thus lavished upon the English "aurists," as measured, at least, by the reputation they enjoy in their own country, this portion of the work would have admitted of great curtailment.

With some modification of the arrangement of the diseases, and the addition of some three or four, the work, in its essential features, continues the same; few, if any, changes of the author's views having taken place as the result of prolonged experience. He commenced his investigation by the careful examination of 50 cases, with the view of establishing a more accurate diagnosis than then prevailed. Further observation of 300 cases formed the material for his first edition; and the present one is founded upon 3700, each carefully entered in his journal. From these abundant sources he draws the conclusion, that no satisfactory diagnosis or treatment of the diseases of the ear can be conducted, without an attentive study of the objective symptoms. It is from the utter neglect of such local examination, and their reliance upon subjective symptoms, or such as are related by the patient, that the details of cases and of treatment furnished by most authors are completely valueless. Another very important conclusion drawn, is the remarkable tendency which diseases of the various portions of the auditory apparatus have to continue limited to those portions;

so that even years afterwards the more essential parts of the organ may remain quite uninjured, and hearing be at once restored by the removal of the intervening ailment. It is this localization of these diseases that has impressed the author with the conviction of the paramount importance of local treatment, and the inefficacy, in the great bulk of cases, of general measures. He rejects the syphilitic, gouty, scrofulous, stomach, sympathetic, &c., deafness of so many authors; and insists that, instead of assuming any of these as ground for treatment, we should diligently search out the part of the ear that is affected. In the vast majority of cases, the most diligent investigation will fail to discover any preceding or present derangement of the health explanatory of the deafness; but in making this statement, Dr. Kramer protests, we think justly, against the conclusion drawn by one of our predecessors,* that he is an "exclusive practitioner," and shows that before he devoted his attention to this much-neglected branch of special practice, his education and practice had been that of a general practitioner. Moreover, while denying the agency of these general causes, he never doubts that the cure, where possible, may be expedited or assisted by the reparation of any defective state of the general health that may exist. He adds:

> "I cannot omit this opportunity of narrating the observations which I have for some years collected at the foot of the Hartz mountains, made upon about fifty male and female peasants, suffering from diseases of the ear. While in the enjoyment of excellent general health, and at the most vigorous period of life, they suffered from that marked form of ear-disease, torpidity of the auditory nerves, accompanied with very great difficulty of hearing and tinnitus; the meatus, membrane and cavity of the tympanum, and the Eustachian tube on both sides, manifesting no disease. The most careful examination could detect in the general health of none of these persons the source of so important a lesion. In none of them had any abdominal affection, hemorrhoids, gout, or any eruption ever occurred; and their malady was nothing but a local affection of the auditory nerves." (p. 171.)

We will briefly recapitulate Dr. Kramer's general principles of diagnosis and treatment, and advert to the new portions he has added to the present edition. His prognosis, he observes, will be found less sanguine, and his treatment far less successful, than that published far and wide by the English "aurists;" but in their utter neglect of the means of accurate diagnosis, and the consequent uncertainty of their therapeutics, and in their deficiency of carefully-related cases, he believes the explanation of this will be found. He insists strongly upon the necessity, in our accounts of cases, of carefully recording the "hearing-distance," as ascertained by admeasurement of the distance at which the same watch is heard at different stages of the progress of the cases. Without this test both the patient and the practitioner may be greatly deceived.

Inflammatory diseases of the *meatus* have occurred in Dr. Kramer's practice in nearly the proportion of 1 to 7 (i. e. 281 times in 2000 cases), and under this appellation he embraces that perverted condition of the secretory apparatus which gives rise to accumulations of cerumen. It is really surprising in how many instances deafness has continued in greater or less degrees for months or even years, and has led to the patient being subjected to a great variety of local and general measures, without the essential preliminary of a careful examination of the meatus having been instituted by the practitioner. Of course, this can only be effectually performed by means of the speculum; but in some of these cases, examination without its aid would have sufficed to detect the easily removeable cause of the affection.

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* Med.-Chir. Rev., 1839, p. 429.

The neglect of the employment of the speculum has also given rise to the most fanciful hypotheses concerning abnormal degrees of tension or relaxation of the *membrana tympani*, first hazarded by Willis, and since fondly cherished by the whole tribe of ear-doctors. Dr. Kramer makes the following statement of the results of his examination of this by the aid of the speculum and strong artificial light, or, better still, the rays of the sun:

> "By the most careful ocular inspection of well-nigh 4000 cases of ear-diseases, repeated on some of these many times, I have arrived at the most undoubting conviction that the *healthy* membrana tympani is entirely colourless, shining, and diaphanous, having a well-marked concavity externally; and that under the influence of the inflation of the tympanum, while the mouth and nostrils are closed, it is raised only into two round, longish swellings near the malleus, whence we may conclude that it is in a state of expansion. The *diseased* membrane, on the other hand, I have always found either inflamed, and then reddened, thickened, degenerated, swollen, or indurated; or, the inflammation having ceased, some of the numerous consequent changes of organization have been detectible, these likewise all excluding the idea of relaxation. With proofs so carefully obtained, I must demand of the supporters of the doctrine of the diseased tension and relaxation of this membrane to indicate some previous signs of the existence of such conditions, and for the future to abstain from all explanations derived from theoretical possibilities and pretexts. The membrane is visible in its whole extent, and changes in its tension must be detectible by a quick and practised eye; but since none such exist, we must omit them from among the really existing diseased conditions of the ear.
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> "In the former edition of my work I put forth the opinion that the membrana tympani was never *ruptured* without prior inflammation. More precise observation has brought to my notice four cases in which the membrane has been ruptured by a blow on the ear, and therefore from the external compression of the air; and two in which this has followed the employment of the air-press, and consequently the pressure of air from within; but that the malleus should become broken within the tympanum of a living man, and, by the friction of the fragments, rupture the membrane (as stated by Ribes), I as little believe as that it also sometimes becomes ruptured by the pressure of accumulations of indurated cerumen in the meatus. In more than 400 of such accumulations I have never seen anything of the kind." (p. 317.)

Acute inflammation of the membrane is a rare affection, 45 examples having only been met with in 2000 cases of ear-disease; but of such frequent occurrence is *chronic* inflammation of this part, that Dr. Kramer states he finds it in every fifth case. The disease presents a remarkable example of the narrow limits within which so many of the affections of the ear continue for so long to be confined; for, contrary to the assertions of many authors, Dr. Kramer found, in 800 carefully examined cases, accompanying affections of the meatus to be a very rare occurrence indeed. He considers the inflammation under different sections, accordingly as it is simple, accompanied with the formation of polypi, or followed by perforation of the membrane, with or without extension of the inflammatory action to the periosteum of the tympanum. *Perforation* is of frequent occurrence (it was observed in 179 out of 397 cases); the opening being sometimes as small as a needle-point, and at others as large as a bean. It was at one time believed that such an accident was destructive to all hearing, and then again, that it scarcely influenced this; but in truth both opinions were wrong, for certainly while there is perforation from chronic inflammation, an entirely healthy state of the hearing is out of the question; yet it is remarkable and quite inexplicable, that a greater proportional amount of hearing is often preserved where such perforation is as large as a bean, than where it only equals the size of a lentil or even a needle-point. The course of the disease is often very tedious, and it may last the whole life of the patient. In nearly half of Dr. Kramer's cases it had existed from ten to twenty years, and in an eighth from twenty to thirty. This is greatly owing to the prejudices against the healing of discharges of the ear in childhood, the especial period of the development of the disease (two thirds of the cases occur during the first ten years of life), and the one best adapted for its removal. In treating this disease, all the purulent or other products of the inflammatory process must be removed by careful syringing with tepid water, by which means cleanliness and some relief from disgusting fœtor are obtained. The ear is to be protected from inclement atmospheres by wool, and the diet and regimen should be of the cooling description. The author does not view Mr. Wilde's division and treatment of the affection, as arising from scrofulous, syphilitic, and other diseased states of the constitution, with any favour. If evidences of such diseases exist, they should be treated; but he declines assuming from local appearances the existence of any such general cause. He entertains a high opinion of the utility of tartar-emetic ointment applied over the mastoid process, while he drops into the cleansed meatus some stimulating solution; one formed of acetate of lead, used at first in very weak proportions, being that to which he gives preference. The meatus is filled with it, and it is left in for a quarter of an hour. If the improvement, which generally soon takes place, becomes stationary, he substitutes after a while a weak solution of the nitrate of silver; and as a means of restoring transparency to the membrane when much thickened, he has found a weak solution of corrosive sublimate very useful. He has no opinion of the operation of perforating the membrana tympani in these cases, and indeed denies its practicability in such. In various parts of his work he censures the empirical manner in which this fallacious operation has so often been performed. The premature bestowal of the Copley medal upon Sir Astley Cooper may be cited in justification of the more leisurely proceedings with which scientific bodies are sometimes reproached. When the inflammation is accompanied or followed by polypi, the removal of these is obviously indicated, though by the careless practitioner they may remain undiscovered. If very small, the solution of acetate of lead sometimes suffices to shrivel them up. Inflammation with perforation is to be treated in the same manner as simple inflammation. It is not always easy to say whether reparation will take place; but this, however, will not until inflammatory action has subsided, nor when this leaves the parts in a whitish, dry condition. It is desirable during the application of the injections, that the patient hold his head so as to prevent these passing through the aperture into the tympanum. The propagation of the inflammation to the tympanum, involving its mucous membrane and the periosteum of both it and the petrous bone, is a most serious aggravation, which indeed may go on to the production of meningitis. Here active general measures, as far as the patient's strength will permit, are indicated. Even in the simple form of the disease its treatment is encompassed with difficulties, in part because of the very long time the affection has continued, and the great changes to which it has given rise in the structure of the membrane.

> "Referring back to my observations on the diseases of the outer ear and the membrana tympani, it will be found that almost every one of their forms gives rise to a peculiar, diseased, puriform secretion—*discharge from the ear*—which, whether as regards quantity or quality, is in no way whatever characteristic of one or the other of these affections; and is, in fact, no form of disease at all, but a mere symptom of disease. Itard, as also all his predecessors and followers, have then acted most erroneously in installing it as an independent form of disease, and even dividing it into a mucous and puriform variety. In this way is the attention of practitioners diverted from the truly important point, the organic sources of such discharge, and a mischievous superficiality of treatment is engendered; so that the interests of science, as well as those of the patient, render it highly desirable that the expression 'discharge from the ear' were entirely discontinued." (p. 459.)

In describing the diseases of the *tympanum*, Dr. Kramer limits his observations in this edition to *inflammation of the mucous membrane;* regarding affections of its periosteum, or bony structure, and destruction of the ossicula, as consequences of inflammation spreading from the perforated membrana tympani; and treating the *otitis interna* of authors, independently of such perforation, as fabulous. The pathological researches of Mr. Toynbee, he observes, do not so distinctly lead to the inference, that in most cases deafness arises from a diseased state of the tympanum, as they might at first seem to do; for the history of the subjects whose ears he examined not being known, it is just as possible that the retention of hearing is not inconsistent with great changes in the condition of this cavity. In investigating the state of the tympanum, *catheterism* is our great resource. If the Eustachian tube and the tympanum be free, on blowing air through the catheter we can hear it impinge on the membrana tympani, unaccompanied by any gurgling sound; the patient at the same time feeling as if it were rushing from his ears, which, where the membrane is entire, is of course a deception. If there is so much hinderance to the passage of air that even a strong blast from the mouth will not overcome it, it is to be propelled by means of the air-press, the practitioner placing his ear near that of his patient to listen to its progress. If the passages are free it enters with a rushing sound, compared by Deleau to that of rain; and this normal sound should be well studied by him who would investigate the diseases of this part. Inflammation of the mucous membrane may be accompanied by *accumulation of abnormal secretions;* and of this we can only assure ourselves by catheterism, after ascertaining by the speculum the healthy condition of the meatus and membrana tympani. The air on entering makes a more or less gurgling sound, according to the amount and tenacity of the mucus present. Notwithstanding the confident assertions of so many writers, Dr. Kramer has not been able to satisfy himself whether inflammation and obstruction of the mucous membrane of the Eustachian tube may exist, independently of such affecting the tympanum. Of one thing, however, he speaks positively enough, that deafness is never caused by enlargements of the tonsils; and considers their extirpation as a most unjustifiable procedure. In treating these cases, the various appropriate general means for correcting any disorder of the general health should be resorted to. Air must be blown into the tympanum by the mouth or the air-press, according to the facility with which it can be accomplished, and afterwards a drop or two of a very weak solution of iodide of potassium or corrosive sublimate insufflated. If the disease be not very deeply rooted, the patient after each sitting finds an improvement in the hearing-distance, and by perseverance in the use of the same means at intervals, for weeks or months, the hearing may be restored completely. In the very old cases, the air-press should be at once resorted to. In a second variety of the inflammation, we have a *tumefied state of the mucous membrane of the tympanum, and stricture of the Eustachian tube.* Here the catheter is introduced with difficulty, and air forced in enters the tympanum in a feeble stream, unaccompanied by any gurgling sound, and its insufflation is not followed by improvement of the hearing-distance, but oftentimes the contrary. Even by means of the air-press, the air is heard only as at a distance, or arriving against the membrane in a very small stream. The extent of the stricture may be ascertained by means of catgut bougies. Slight degrees of narrowing of the tube are not infrequently found associated with nervous deafness; but stricture, properly so called, is one of the less frequently occurring diseases. Notwithstanding the statements of other authors to the contrary, Dr. Kramer feels convinced that *obliteration of the Eustachian tube* is a very rare occurrence, having found only 2 out of 3700 cases, in which, in spite of every effort, he was unable to effect an entrance.

Excitement of the sensitive nerves of the tympanum (*Otalgia*). The following is the only case which his long practice has furnished to the author contradictory of the assertion of the non-existence of a purely nervous earache, put forth in his first edition. A woman, æt. 28, in good health, became the subject of toothache in the right upper jaw. The pain soon spread to the meatus of the same side, where, having left the tooth, it continued, with scarcely any intermission, to an agonizing extent for upwards of six months. Nothing gave her relief, and even the supervention of a gastric fever during six weeks, and of a dangerous pneumonia during four weeks, produced no diminution of her local torment. When she came to the author she was in complete despair. He found on examination the meatus and tympanum quite healthy, while the normal state of the hearing-distance, and the absence of tinnitus, proved the special sense to be so likewise. To gain a little time to consider the case, the author temporised with it awhile; but in the mean time, the patient, in despair, had a very carious but quite painless tooth of the upper jaw extracted, and with wonderful rapidity the pain ceased, never to return.

The investigation of the diseased condition of the *internal ear* is surrounded with difficulties; for there is not only the absence of means of direct investigation of the parts during life, but also of opportunities of examining them after death. The consequence is that all kinds of hypotheses have been invented. And while, on the one hand, diseases having no existence have been described, so, on the other, the participation of the auditory nerve in such has been denied. The only case of *acute inflammation of the labyrinth* known to the author, he quotes from the 'Gazette des Hôpitaux.' After a knitting-needle had been passed into the ear, the patient uttered loud cries, and fell down senseless. Restored to consciousness for a while by bleeding and other means, he again relapsed into coma, putting his hand to his head every now and then, and suffering convulsive attacks until he died, on the fifth day. The membrana tympani was found destroyed, the *fenestra ovalis* opened, the chorda tympani divided, and the tympanum filled with pus, in which floated fragments of the ossicula. The vessels of the vestibule were much congested, as were those of the cochlea and the semicircular canals. The adjacent membranes of the brain were severely inflamed. In this case the needle probably penetrated into the vestibule. Many cases are on record in which sharp foreign bodies have entered the tympanum, but none wherein they have taken so unfortunate a direction into the inner ear, and given rise to inflammation of it and of the membranes of the brain.

Dr. Kramer has a very interesting chapter on *Nervous Deafness*, to which much has been added in this edition. The disease may come on so gradually, as to have reached a considerable height prior to being detected by the patient, especially when only one ear is affected. The meatus frequently, though by no means always, contains some dry, crumbling wax, which, however, after a while disappears, the meatus then giving off white, dry squamæ, and acquiring a parchment-like, senseless surface. This dry state of the passage has often been mistaken by superficial observers for the cause of the deafness, whereas, in fact, it is but a consequence of the defective vital action which has been reflected from the auditory nerve over the rest of the apparatus. To this may be referred the dull, paper-white, non-transparent, state of the membrana tympani. Among the various accompanying unpleasant sensations in the head and ears, *tinnitus* is the most frequently abiding and annoying. It is this, indeed, which often first attracts the attention of the patient to the true state of his hearing, and like so many other symptoms, has been viewed and treated as the cause of the deafness. Its intensity is generally in proportion to the deafness, and rare, indeed, is it for it to disappear while the deafness remains; so constant and distressing is it, that in its aggravated degrees, it proves a greater annoyance to the patient than the accompanying loss of hearing. The diagnosis of the affection is furnished by *negative* signs. Air may be freely blown into the tympanum through the catheter, meets with no obstruction, produces no abnormal noise, nor yet any augmentation of the hearing-distance, while it aggravates the tinnitus. The tinnitus, though almost always present in nervous deafness, is not, as stated by many, characteristic of it, existing, as it does, in the various other forms of ear-disease. Dr. Kramer regards the disease as dependent upon *debility* of the *auditory nerve*, with which is associated inordinate *irritability* or *sensibility to stimuli*. Thus painful excitement is caused by all shrill tones, the injection of the mildest fluids into the tympanum, or the gentle introduction of air into it, while, when the tympanum is diseased, and the auditory nerve healthy, the strongest blast may be driven in with advantage. Numerous observations show that, not only is this the most frequent of ear-diseases (1028 in 2000), but that it is a primary one of the auditory nerve, and one that long maintains its individuality. The *prognosis* is mostly unfavorable, for the disease has usually long persisted, and probably been maltreated before we see it; while, if left to itself, its tendency is to advance from bad to worse. With the increase of deafness the irritability becomes so augmented, that any application strong enough to be efficacious, is often intolerable. It may be safely affirmed that *complete loss of hearing*, with its accompanying tinnitus is not only an incurable affection, but one susceptible of no improvement whatever. In very bad cases, months of very careful treatment only suffice to preserve what little remnant of hearing exists. Even in less advanced cases, when there is hereditary predisposition, the cure is often very obstinately delayed. Any improvement of the hearing, without diminution of the tinnitus, is transient and delusive; and such diminution, indicating an abatement of the diseased susceptibility of the nerve, is one of the first signs of successful treatment.

In treating this disease, one of our first objects should be to protect the ear from injurious impressions, whether produced by hard, shrill voices, or instruments, or metallic hearing-trumpets; and Dr. Kramer frequently in the course of his work animadverts upon the error of addressing even the very deaf in loud shrill notes. Great *distinctness* of pronunciation, the words being uttered very deliberately near the ear, in the ordinary tone of voice, is what they require. Unfortunately the necessity of avoiding these and other stimuli, as well as erroneous diet and habits of life, is by no means so distinctly recognised as in the cure of diseases of the eye. Although recognising the correctness of the principle that the predisposing and exciting causes are to be attacked, the author observes that these are not of easy discovery or removal. The condition of the general health is by no means a channel for effecting the removal of the disease to the extent that might be supposed, and in several of the worst cases it is quite unaffected. If any marked deterioration exists, it should be removed before resorting to the *local* treatment, which, however, may at once be employed in old standing cases. The object of this is, first to subdue the morbid irritability of the nerve, and afterwards, if hearing is not improved, to endeavour to strengthen its tone. The amount of tinnitus is the best measure we have of the increased irritability of the nerve.

> "I may lay the rule down with confidence, that when the accompanying tinnitus passes from high shrill sounds into lower and deeper ones; when these are feebler and seem to be heard at a greater distance from the ear, while the intervals of their cessation become longer, when the usual increase of the tinnitus in bad weather, or under the influence of bodily or mental exertion and excitement, does not take place, or only to a very slight degree; and when the sometimes attendant giddiness, confusion in the head, fulness or pressure on the ears, sensibility to loud sounds, and such like inconveniences lessen or even disappear, then may we say that the irritability of the nerve is lessened, and the entire diseased condition is disposed to amendment." (p. 752.)

The irritability of the nerve is best subdued by the admission through the Eustachian tube of mild vapours, either of water, mucilage, or thin gruel, in which, either at first, or at a future sitting, a minute portion of ext. hyoscy. has been dissolved. The fluid is vaporized in a flask by means of a spirit-lamp, and is allowed to enter at a temperature of 22° to 25° R. for from three to five minutes; and the application is repeated daily, or only at intervals, according to its effect. If in from two to four weeks no improvement in the hearing distance or tinnitus is observable, we may abandon all hopes of doing more by a longer perseverance than preserving what amount of hearing remains. Only when improvement has followed the above means in respect to the tinnitus, can we venture to vaporize a few drops of some tincture, as assafœtida, musk, &c., or a little ether or chloroform, such addition being omitted if over-stimulation takes place, as indicated by aggravation of all the symptoms. When this results from even the mildest applications, we conclude the case to be incurable, all means proving too stimulant for the excessive irritability of the auditory nerve; and this is found to be the case proportionately to the difficulty of hearing. In cases of incurable deafness, its accompanying distressing tinnitus is just as incurable.

Nervous tinnitus without deafness. In the present edition, Dr. Kramer relates four out of twelve cases of this which have occurred to him. The affection had come on suddenly and severely in one or both ears, and had continued for months or years before he saw the cases. The sounds in nowise differed from those which accompany the various forms of ear-disease, though now constituting the only morbid condition, as careful examination by the speculum, catheter, air-press, and watch distinctly proved. A strong blast of air augmented the tinnitus, just as it does when this accompanies difficulty of hearing. After this tinnitus has continued for years, the hearing is still found unaffected; and when this is considered, together with the other fact, that this symptom is sometimes absent in the various stages of deafness, and at others exists to a degree almost driving the patient to despair, even after all hearing is extinguished, we see its utter diagnostic worthlessness, though upon its presence most important conclusions have been drawn, and practices founded. It probably depends upon an entirely different lesion of the auditory nerve from that which produces nervous deafness; but wherein the peculiarity of either consists, our knowledge of the anatomy and pathology of the nervous system does not even enable us to guess. Most of these persons were in excellent health, and an exciting cause could not always be ascertained; though by some, exposure to vicissitudes of temperature or the concussion of the air by fire-arms was assigned as such. Time seems to exert little or no ameliorating influence, the affection remaining for years quite unmitigated; while the author has had no opportunity of testing the efficacy of medical treatment; for being unable to make any sanguine promises of recovery, even as the result of very prolonged procedures, he has not yet found any one disposed to make the necessary sacrifice of time and money. The indication is to subdue the abnormal irritability by the vaporizing treatment already mentioned.

We do not find any other portion of the work calling for remark; and indeed it may seem that we have bestowed a somewhat long critique on merely a new edition. In point of fact, however, based as they are upon so much careful observation, the reaffirmations, additions, and modifications (and these last are few) of so distinguished a practitioner called for rather a full notice, maintaining the work as they do at the head of the branch of medical science to the illustration of which it is dedicated. In the event of a translation being deemed expedient, however, much curtailment will be desirable; for Dr. Kramer seems to have thought the somewhat unusual terseness of his former edition a defect of which a German ought to feel ashamed, and to have set about repairing it with a diligence for which we question whether his reader's gratitude will compensate him. Diffuse and verbose criticisms upon works of no weight or authority fill page after page, and even some of the cases narrated in the former edition have grown in dimensions in the present. We have carefully compared the two editions, and find the present almost rewritten, and the great bulk of the 180 cases new ones; and some idea of the needless length to which it has extended may be formed, when we state, that although it occupies 870 pages, it furnishes little or no account of the anatomy or physiology of the organ, and none of its congenital defects. Apart from this undue verbosity, the edition is valuable as containing much additional confirmation of the principles laid down in the former one.

ART. X.

1. *Observations upon Bulam, Vomito-negro, or Yellow Fever; with a Review of 'A Report upon the Diseases of the African Coast, by Sir William Burnett and Dr. Bryson,' proving its highly Contagious Powers.* By Sir WILLIAM PYM, K.C.H. Second Edition.—*London*, 1848. pp. 312.
2. *An Account of the Origin, Spread, and Decline of the Epidemic Fevers of Sierra Leone; with Observations on Sir William Pym's Review of the 'Report,' &c.* By ALEXANDER BRYSON, M.D. R.N.—*London*, 1849. pp. 174.

THE conflict between the believers in the specific nature of yellow fever, headed by Sir William Pym, and the numerous observers who will not grant to this malady a position distinct from certain malarious fevers, is not likely to cease from lack of combatants, nor, let us add, from any deficiency of interest in the subject itself. Both parties now, as fifty years ago, are still eager for the fray; and when disabled or exhausted partizans retire from the field, plenty of recruits press on to occupy the vacancy. It could be wished, however, that the fight were conducted a little more after the fashion, and according to the tactics, of regular campaigns, in which men shoot down their opponents with the most gentlemanly consideration that it shall be done *secundem artem*, and in obedience to the time-honoured usages of glorious war. The very "pretty quarrel" to which we now refer, resembles more the raids or onslaughts of Red Indians or tattooed New Zealanders, who give no quarter after victory, but breathing an irrepressible hatred, murder the wounded, mangle the dead, and roast the juiciest of their captives for the satisfaction of their appetites. We should really dread to trust the worthy chief of the navy medical department to the tender mercies of Sir William Pym; nor, in spite of his urbanity in private life, should we feel quite satisfied that Dr. Bryson would not, if the occasion presented itself, imitate the great Fluellen, and insist on the champion of the vomito-negro eating the second edition of his work on Bulam Fever.

As specimens of the tone adopted by both writers, we may cite the following passages. We find Sir William Pym stating, that Sir William Burnett and Dr. Bryson "have gone unnecessarily and wantonly out of their way, to make unfounded and malicious statements," and giving utterance to a pious wish that these gentlemen may profit by the information he has given them, and may "pray forgiveness for their inconsistencies and misrepresentations." He informs us that he has been induced to take up the gauntlet again, by the "wanton, false, abusive, and unfounded attack" made by Sir William Burnett upon him in reference to the Eclair.

On the other side, Dr. Bryson accuses Sir William Pym of attacking him "in most unjustifiable and unprofessional language;" of making use "of garbled and unfair extracts;" of "disingenuously mixing up facts and statements in a confused manner, for the purpose of making out charges of inconsistency;" and he regrets that Sir William Pym's review of his 'Report on the Diseases of the African Station' "is written in a style which prevents his replying to it with the respect due to a man of Sir William Pym's age and position in society."

Now we must ask, what is the use of all this? Where is the necessity, that in a question of pure science, men should allow personal feelings to become interested in what should be entirely a judgment by the reason? Can we not object to a man's conclusions, without contemptuously depreciating his intellect? or believe that his proposition is wrong, without attributing the error to unworthy motives? Can we ourselves not be allowed to abandon a position we have hastily assumed, when, by the aid of fresh knowledge, we find it to be untenable? Are we called on to nail our colours to the mast, and refuse to yield a step to the most convincing arguments? We cannot believe that personal controversy can ever do anything but tarnish the purity of truth, and hinder us from discovering, among the conflicting opinions, whatever thére may be of stable and true. We cannot but think that men will turn away disgusted from a debate in which the great questions of science appear only as the weapons which are wielded by private animosity to give edge and sharpness to personal dislike.

Let us, however, do this justice to Dr. Bryson, that the bitterness which Sir William Pym has for more than forty years infused into this subject, is some kind of excuse why a similar manner should be adopted towards him; and we must say that Sir William's mode of speaking of the excellent 'Report on the Diseases of the African Coast' is extremely unjust, and might provoke the most good-natured of men. But we must express our regret that Dr. Bryson did not altogether disregard Sir William Pym's personalities, and preserve the great scientific value of his present work from the least taint of controversy. It is absolutely treason against truth to introduce into her domain what is evanescent and fugitive. To call off our attention from the great points at issue, to the quarrel of Sir William Burnett and Sir William Pym, is as if one should ask us to gaze at the Arab's tent, which was pitched to-day and will disappear to-morrow, and to forget the Pyramid, immovable and enduring, which frowns above it.

Quitting then, most gladly, and leaving to the arbitration of those who feel pleasure in such things, the consideration of the exact measure in which Sir William Pym has falsified or misinterpreted the 'Report on the Diseases of the African Station,' let us turn to what is really of scientific worth in these works, and let us endeavour, as concisely as may be, to put it before our readers.

The question, as regards yellow fever, at present stands thus: It is admitted we believe by all, that a yellow fever is propagated by contagion, and spreads by intercourse of the healthy with the infected. This point seems quite settled, and we trust will not lightly be reopened. The next question is, whether such a contagious yellow fever is a specific and distinct disease, or arises from, and is allied to, certain forms of malarious fever which are endemic to the countries it chiefly or solely infests. We cannot consider that at present we can venture on an absolute decision on this point, and yet it is one of great importance, as it bears not only on the nature of yellow fever, but indirectly on the nature and origin of many other epidemic and contagious diseases, and on many questions of hygiene and quarantine. In some former articles on this subject, our readers will remember that we came to the provisional decision, that although yellow fever was contagious on some occasions, it had not been proved to possess, at such times, any pathognomonic symptoms which could distinguish it from other yellow fevers which were decidedly of endemic and local origin. It appeared to us that the most probable view was that which considered it to be a derivative from these endemic fevers; and this, after a long analysis of the whole subject, was the opinion we at length adopted. We do not intend on the present occasion to retrace our argument, but we refer to our First Volume for its exposition. We may mention, however, that the two chief reasons which inclined us to this opinion were:

1. The fact that no valid distinctions had been established between the contagious and the malarious yellow fever.*

2. The circumstance that occasionally we appeared to see the conversion of the non-contagious into the contagious fever going on before our eyes, in the cases in which ships having been exposed to and having suffered from the endemic fever, afforded us evidence that this fever, or one indistinguishable from it, was actually capable of propagating itself by contagion. We selected four examples of this, as being those we considered freest from ambiguity, viz. the cases of the Kent, the Scout, the Bann, and the Eclair.

A similar opinion had been advanced by Dr. Bryson in the 'Report on the Diseases of the African Station;' the evidence he has now collected of the fevers of Sierra Leone is strongly in favour of its correctness.

Dr. Bryson has examined into every document connected with the several outbreaks of fever at Sierra Leone, and the information he has given us is by far the most accurate we have ever yet received about them. He has evidently bestowed great labour on the subject, and we are not without hopes that he has advanced us a good way towards the final settlement of the question.

Dr. Bryson does not doubt in the least that yellow fever is sometimes contagious. Did the cases of the Bann or the Eclair permit us to hesitate upon this point, we could find ample proof from other cases adduced in the work before us. But it is unnecessary to take up any of our narrow space, with what would be merely a repetition of almost similar arguments. We will proceed to some other points connected with the origin and development of yellow fever on the western coast.

The most probable opinion seems to be, that an endemic fever is seen every year in Sierra Leone, which is most prevalent in the lowest and most crowded parts of the town, and arises, according to the opinion of all who have witnessed it, from local causes. It is presumed not to be contagious. At certain periods this fever acquires a great development, and becomes epidemic; although its symptoms remain the same, they are much heightened in intensity; remissions are less common, black vomit is more frequent, the mortality is much greater, and, in addition to other modes of propagation, it spreads itself occasionally by contagion. Its contagious character has been chiefly manifested on ship-board, where, from the circumstances of the case, the sick are supposed to be placed under unfavorable hygienic conditions. The cause of this fever becoming epidemic is to be looked for partly in some increased development of the active cause, and occasionally also from the fact, that in some of these epidemics the arrival at the place, of unacclimatized individuals who are peculiarly susceptible to the disease, has unfortunately coincided with the epidemic diffusion of the poison.

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* That is, no valid distinctions when all the forms of marsh yellow fevers are taken into account. As we formerly observed, when an observer has seen only the milder forms of marsh remittent fever, and is then suddenly called upon to witness an incursion of epidemic yellow fever, he may well believe that the affections are entirely distinct. But, after a time, when the intermediate forms have been more closely scrutinized, it is found, that at no point can any valid line of demarcation be drawn between these two affections, so numerous are the connecting links which bind them to each other

In the first epidemic recorded by Dr. Bryson, viz. that of 1823, there is no doubt that a contagious character did attach to a yellow fever which the crew of H.M.S. Bann contracted at Sierra Leone. The evidence now brought forward by Dr. Bryson seems to us decidedly to indicate, that the fever thus contracted by the Bann was an offshoot from the common endemic fever, which had at that time acquired an increased development, and subsequently seemed to merge altogether into the intenser epidemic disease. There is no evidence of any importation into Sierra Leone of the contagious disease, as there is subsequently of its introduction into Ascension by the Bann. The statement, as far as we can see, leads to this conclusion. The endemic fever prevailed among merchant seamen at the end of 1822. No one seems inclined to call this the epidemic yellow fever; then on the 11th December, an isolated fatal case occurred of a remittent fever with black vomit, which is termed "a suspicious case" by the medical attendants, and is considered by Sir William Pym the first case of the epidemic. This patient was the harbour-master of Sierra Leone, and he had been on board no infected vessel, nor had been exposed, as far as is now known, to any other except to the causes of the common endemic and local fever. Six weeks subsequently, another case occurred, the exact symptoms of which are not known. The third case occurred on the 22d of February, 1823, in the person of a seaman who had been loading timber up the river, and the crew of whose vessel (the Caroline), while engaged in thus loading, had lost several men from a fever, whose symptoms are unknown.

Subsequently, cases occurred among the crews of other vessels, and among the inhabitants of Sierra Leone, and gradually the cases became so numerous as to be called epidemic. The epidemic fever continued to prevail till June or July, 1823, when it disappeared, and the endemic fever, which it had seemed to displace, was found to again reoccupy its old ground, and to manifest about its ordinary severity. Taking away the case of the Bann, there is not in this epidemic the slightest evidence of contagion, but, on the contrary, some arguments against it. The channel of introduction cannot be indicated; the early cases were isolated, and separated from each other by long intervals of time; there is no proof that there had been any proximity between the affected persons; the symptoms did not present any decided characters, but it seemed doubtful whether they were instances simply of the endemic fever, or of the fever which was shortly afterwards to become epidemic. In the Bann, however, this fever was contagious, and as the account of the fever in this vessel is given with great precision by Dr. Bryson, we shall quote his account in full:

"Her Majesty's sloop Bann anchored in the Sierra Leone river on the 11th of January, 1823, after cruising in the Bight of Benin. She remained at Sierra Leone until the 25th of March, part of her crew being employed in her tender, the San Raphael. The men were greatly exposed in refitting the rigging of both vessels, and the schooner having been sent up the river to Bunce Island to have her bottom repaired, she was there laid on the beach, and the men exposed on shore in one of the most unhealthy spots in the colony. On the 1st of March, Mr. Higgon was seized with fever in this vessel, where he had been for some days with a party of men actively employed getting her ready for sea. On the morning of the 2d, he was better, but became worse again in the evening; he had more fever, which increased until the 3d, when in the evening it remitted. On the 4th and 5th, it appears to have been moderate, and on the 6th, he was free from all complaint but debility. His eyes were slightly yellow. On the 10th of March, four cases occurred in the same vessel, the San Raphael, while at sea. In the only one of these cases which is detailed at length, there were slight remissions as in the preceding. On the 15th, the skin assumed a yellow tinge, but the patient recovered, and was discharged to duty on the 24th. On the 25th, another man was attacked in the tender, which had returned to the anchorage of Freetown (i. e. at Sierra Leone); he had been only three days on board. On the 26th, there was a slight remission followed by an accession of fever. On the 27th, there was a second remission followed by an accession. On the 29th, he began to sink, and died early on the 30th. The integuments, which were slightly yellow, assumed a very deep colour after death. This was the first fatal case that occurred amongst the crew of the Bann, but whether it, or the one which occurred on the 1st of March, or either of them, was the first of the malignant fever that followed—that is, if there were any difference in the type of the fever—I shall not attempt to explain. Three men, two in the San Raphael and one in the Bann, were attacked on the 26th, but they all recovered. The Bann sailed on the 27th of March, but appears to have remained about the mouth of the river until the 29th. Three cases occurred on the 27th, but they were all of an ephemeral nature, the whole of the men having returned to duty within a week. On the 31st, there was a case of more severity, the patient having been on the sick list for nineteen days. On the 3d of April, there were four attacks: these, with the exception of one, were all slight cases. On the 7th of April, eleven days after the Bann left Sierra Leone, there was a severe case which terminated fatally on the 12th. On the 9th, there was another of a similar character, which proved fatal on the 13th. From this time the disease assumed a greater degree of malignancy, and the deaths became more frequent. There is only one of the cases which occurred about this period that is fully detailed in the Surgeon's journal. The patient was attacked on the 14th, but had felt unwell for a few days previously. He had the usual symptoms of fever, viz. general pains, a burning hot skin, and a quick full pulse, which appeared to have been relieved by bleeding. On the 15th early, he suffered less from pain; the skin was hot, but moist, the pulse was full and quick; at 9 a.m. he was again bled, which seemed to relieve him, but at noon the bad symptoms returned; the skin became very hot, the pulse 120 and strong; there was, in fact, an accession of fever. On the evening of the 16th, slight stupor supervened, and the fever continued urgent. On the 17th, the symptoms were more favorable, and there was a decrease of pyrexia, "the fatal lull." On the 18th, about noon he became suddenly worse, and died almost immediately afterwards. Neither black vomit, nor yellowness of the skin is mentioned.

"The great difficulty of classifying these cases according to Sir William Pym's general or first division of tropical fever, will at once appear. One class of observers would be inclined to place them with the first, the endemic or bilious remittent; another with the second, or bilious continued; and another with the third, or Bulam—vomito-negro. Had the fever ceased at this time, they most likely would have been designated the common endemic of the coast; but as it did not, and as black vomit subsequently appeared in other cases, they may, I presume, be named endemic, or epidemic, according to the whim or fancy of the nosologist.

> "It was intended to proceed with the vessel to the Island of St. Thomas, in the Bight of Biafra; but on account of the rapid increase of fever cases, and the bad state of the weather, it was deemed advisable to run for the Island of Ascension, which she reached on the 25th of April, having lost thirteen men altogether up to that period. The virulence of the fever about the third week in April, a few days after she crossed the equator, and when within a few hundred miles of Ascension, running nearly before a strong south-east trade wind, appears to have frightfully increased. On the 22d, there were no fewer than eight men attacked, not one of whom survived. Three died on the 26th, two on the 27th, two on the 29th, and one on the 3d of May." (p. 22.)

In reference to this remarkable increase in violence at this period, Dr. Bryson remarks:

> "How are we to account for this sudden aggravation of the malady? Most unquestionably not from the impulse of any new morbific agent received from without the ship; neither can it be supposed that the fevers which were then occurring arose solely from the individual imbibition of any peculiar miasm at Sierra Leone, because a period of four weeks had elapsed since her departure from that port, and because it would be contrary to experience to suppose that the exciting poison, whether it were of a personal or a terrestrial nature, should acquire a greater degree of potency by the stage of latency being protracted. Therefore, as there is no reason to believe that there was any new physical cause evolved by the accumulation or the decomposition of foreign matters in the hold or bilges, or otherwise produced within the vessel, we are forced back upon the disease itself for an explanation of this extraordinary change; and this seems to be more reasonable than that which has been already adduced, viz. that although the fever did not at first appear to be contagious, yet from want of ventilation, and a great number of sick being confined in a small space, it ultimately became contagious. The type of the fever, which in the first instance was mild, and by no means fatal, in the course of a few weeks totally changed its character; it became virulently and dreadfully fatal; hence the unavoidable conclusion, that the causes which operated in the production of the first cases were either greatly altered, producing a corresponding alteration in the nature of the disease, or that there was an additional cause, the product of an excess of morbid action, emanating from the bodies of the sick, which, as a necessary consequence, multiplied itself, and increased in virulence as the number of sick increased, and the proportional space for their accommodation and ventilation diminished. There is nothing new in this mode of reasoning; the same result has been observed time out of mind, under similar circumstances, in every region of the world. And as a further proof that the type, or if that term should seem objectionable, the general character of the disease was thus changed by accidental circumstances; no sooner were the men landed than it reverted to the mild form it originally possessed, and the mortality greatly diminished." (p. 23.)

We presume that the whole history of the fever in the Bann is now as far elucidated as we can ever hope it will be; and, considering all things, we are strongly inclined to believe that the opinions advanced in the above extract will turn out to be correct.

It is perfectly true that the evidence in favour of this fever being at one time at least possessed of contagious properties, is stronger than the evidence which derives it from a local source; yet our readers will see that this evidence also is tolerably stringent. For here we have the Bann exposed at Sierra Leone to the common local causes of fever, and some of her crew in the San Raphael on duty "at one of the most unhealthy spots in the colony;" and the fever produced by the emanations at this place did not differ in symptoms from the cases which occurred after the Bann had left Sierra Leone altogether, and which must be supposed to be examples of the epidemic contagious yellow fever, if we are desirous of keeping up the series of cases, so that one might communicate the disease to the subsequent case. These early cases are therefore so considered by Sir William Pym. While, however, there is proof of this local influence, and of severe fevers consequent upon it, there is no evidence of the crew of the Bann having been in contact with any of the few cases of epidemic yellow fever which had occurred before the 1st of March, and which cases, as we have already seen, appeared to be themselves attributable to local causes, and not to any imported contagion.

Still, as this mode of viewing the case rests in part at least on negative evidence, viz. that no exposure to a contagious virus was traceable, and in part also upon the supposition that the severe yellow fevers, which obviously did arise from local causes, were identical in kind with those which afterwards were proved to be contagious,—a supposition which, though probable, is not absolutely proved,—an objection may possibly be taken to it.

In order, then, to see what importance is to be attached to such an objection, it will be necessary to consider the additional light which is thrown on the fever of the Bann by the study of the subsequent epidemics at Sierra Leone. If it appear that the case of the Bann is a solitary exception to an otherwise universal rule; if in all the other epidemics the contagious virus has been clearly introduced, and propagated from person to person; then we might conclude that this mode of production occurred also in the case of the Bann, but escaped the observation of those who witnessed the epidemic. If, on the contrary, there is any evidence in subsequent epidemics, that fevers which appeared to be derived from common endemic and local sources, also appeared subsequently to acquire contagious properties, then the argument that the fever of the Bann originated in like manner, and became afterwards contagious, will be much strengthened.

In the account of the epidemics subsequent to 1823, which Dr. Bryson has given us, we find this story of the Bann, with slight variations, repeated over and over again. Continually we find the same series of sequences; an endemic fever becoming more prevalent; dropping cases of a severer fever, which seems to arise out of the endemic disease; then, under certain circumstances, this latter appearing to propagate itself by reproduction of its active cause in the human body. We cannot pretend to follow Dr. Bryson through his most instructive treatise. We must content ourselves with selecting one or two of the most important points of evidence.

After 1823, epidemic yellow fever prevailed to some extent at Sierra Leone for a year or two, and more severely for four or five years at various other stations on the west coast, particularly at Bathurst, Cape Coast, and Accra. But for some time previous to 1829, the only fever seen in Sierra Leone itself appears to have been the common endemic. In April 1829, however, the epidemic fever reappeared. As in 1823, the first cases were scattered and were not imported; they seemed to grow out as it were from the endemic fever, from which, indeed, some of them appear to have been scarcely distinguishable. Then, gradually increasing, the disease spread as an epidemic among the European seamen and the white population residing along the lower left bank of the river, the very locality in which the fever of 1823 had committed such terrible ravages. In this locality the fever did not appear contagious. Dr. Bryson gives the following quotation from Boyle, to whose account of this epidemic we formerly referred:

> "It appears," says Boyle, "that the disease was not contagious. This is evident upon a review of the cases which occurred in the shipping in the harbour, and the situation of the houses in Freetown, in which fever prevailed. The ships suffering were invariably exposed to the north-east winds, and in the town the fever limited its course in a most striking manner to the houses between which and the north-east winds passing over the Bullom country there was no obstruction; no *cordon sanitaire* was established in Freetown or in the colony, and yet the only course the disease took was along an extraordinarily limited space near the water's edge, facing the Bullom shore. That a great many exposed persons may escape an attack of a prevailing contagious fever, is a fact of easy belief; but that a fever of a contagious character should be so accommodating as to limit its ravages to certain houses which happened to be exposed to particular winds, is altogether beyond the credulity of the most common reflecting mind, and the more especially so when it is taken into consideration, that an unlimited communication took place between the inhabitants of the different quarters of the town, and that, although very many hundreds of individuals from various parts of the colony visited the market every day, not one case of the disease occurred in the villages." (pp. 28-9.)

At the commencement of this epidemic Her Majesty's ship Eden was anchored in the roads; and, as usual, her officers and crew were occasionally on shore, and were exposed to the same causes, whatever these may have been, which were producing or were about to produce the epidemic fever among the inhabitants of Freetown. On the 3d of May, a man, who on the 1st had come on board the Eden from Freetown, was taken ill with yellow fever and died on the 7th. On the 5th of May, two days after this man had been taken ill, a midshipman, who had been on board a detained vessel of which he had charge, returned on board the Eden. He had been taken ill on the 29th, and died the day following his removal to the Eden (p. 38). On the 12th some other cases occurred among the ship's crew; and on the 20th of May the Eden left for Fernando Po. She arrived at her destination on the 11th of June, having lost during this period twenty-five officers and men. She was thoroughly cleaned, fumigated, and whitewashed at Fernando Po, and then, reembarking her convalescents, sailed for Princes Island on the 9th of July. During this voyage the fever reappeared, many relapses occurred, and the mortality was considerable. The fever continued to rage till August; and the Eden lost, between the months of May and December, 110 persons out of a complement of 160. The fever was attended by a remission after the first twelve or fourteen hours, by a marked exacerbation on the third day, by yellow skin, and frequently, though not universally, by black vomit. That the fever was contagious on board this vessel, appears very probable from a consideration of its mode of spread; but in addition we have a stronger proof of its contagious character, in the fact which appears to us highly probable, if not quite certain, that the disease was introduced by the Eden into Fernando Po, where it prevailed with great severity, and was attended by distinct remissions, yellow suffusion of the eyes and skin, and black vomit.* It seems to have been also clearly communicated to the Sybille by men from the Eden.

Now, in this epidemic we see apparently the exact counterpart of the

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* Report on the Climate and Principal Diseases of the African Station, p. 70.

epidemic of 1823. Without any marked channel of
apparently growing out of the endemic fever, the epi
appears at Sierra Leone. A ship's crew of susceptil
attacked, at the very commencement of the epidemic, w
they are exposed to the common local causes, possibly
emanating from persons already diseased, though of this
but they are not exposed to any marked and unusual con
the ship thus infected with fever puts off to sea, and
fever to a healthy station. But, although so decidedly
instance, the proofs of contagion in the town from whi
been derived fail altogether, and it is not even attem
disease to any imported contagion.—At the same time t
carried the disease to Fernando Po, the Champion sailed
with several bad cases of fever on board, arrived at Fern
after the Eden, and landed her sick. On board this s
board the Eden, the fever appeared decidedly contagiou

In the epidemic of 1837 the same series of events
traced; severe cases of endemic fever, then cases of epi
and scattered, then at a later date some evidences o
evidence of contagion in this epidemic is derived in pa
the Ætna. This ship arrived from Gibraltar in the roa
in November; the fever at this time was committing g
the prize crews and merchant seamen. On the 3d of D
having watered, and taken on board some African b
sailed from Sierra Leone. On the 8th one of the Afri
ill with a slight feverish attack, which did not attract
the 10th two serious cases of yellow fever occurred in t
had both been on shore and much exposed. On the 1
occurred; and of these four cases, three died with blac
skin. On the 20th two other cases occurred; on th
the disease attacked officers and men indiscriminatel
January, when the Ætna anchored at Ascension, nin
occurred and twenty-five deaths. Only five of the
escaped; the fever was distinctly remittent, was at
suffusion of the skin and eyes, hemorrhage from the
and black vomit in the fatal cases. The spread in
through nearly a whole ship's crew seems almost unec
of contagion, provided no local causes can be foun
But a still stronger proof of contagion during this y
following case:—The Forester left Sierra Leone for A
ber with fever; on the route to Ascension she fell i
bound also for Ascension, and sent on board a prize
after this, yellow fever appeared on board the Bonetta,
fatal. Both vessels arrived at Ascension, and shortly a
fever appeared among the residents on that island; an
not space to go into the evidence, there seems little d
derived from the Bonetta, Forester, or Ætna, which a
time with fever on board. Moreover, the Waterwitch
with a healthy crew, contracted yellow fever there a
losing in the space of about forty days fifteen men c
attacked.

points, these cases of the Bann, the Eden, the Ætna, Forester, and Bonetta. We allude to the well-known case of the Eclair. We have already on a former occasion discussed this case at such length, that we shall now merely recall the chief facts to mind. These are, that the Eclair carried a yellow fever from Sierra Leone to Boa Vista, and communicated it to the inhabitants of that island; that this yellow fever seemed to originate among the crew of the Eclair, from the exposure to which they had been subjected, in boat-service and in watching for slavers, and subsequently from irregularities on shore, at Sierra Leone. As in the former cases, there is no evidence of any distinct channel of importation. It has been attempted, indeed, to point this out, and it has been asserted that the Eclair suffered from two diseases; viz. the common endemic, and subsequently from the epidemic fever, introduced by a passenger who embarked at Sierra Leone. We formerly expressed our doubts of the correctness of this opinion; and Dr. Bryson has now, as we think, settled the question, to the satisfaction of all impartial judges. He has shown, that the symptoms presented by the cases which occurred while the Eclair was at anchor off the harbour and sea-bar, and which are considered by Sir William Pym as cases of common endemic, closely or completely resembled those which subsequently occurred on the passage from Sierra Leone to Boa Vista, and which were undoubtedly cases of contagious yellow fever; in both cases there were remissions, a lemon-yellow hue of the skin, and black vomit, which was however more frequent among the latter than the earlier cases, herein resembling the course of events in the Bann. Further he has proved, that at the time the Eclair left Sierra Leone, yellow fever did not prevail in that place, and that therefore the Eclair could not have thence derived it. The first case which at all resembled yellow fever, occurred in a seaman who had arrived at Sierra Leone ten days after the departure of the Eclair, and who was taken ill fourteen days after landing, or twenty-four after the Eclair left. The case was attended by remissions, epistaxis, black vomit, and after death the liver was found of a pale yellow colour, and the stomach was filled with a fluid like coffee-grounds. This seems to have been considered an example of common but severe endemic fever. The next case was in a captain of a merchant vessel, who had arrived in Sierra Leone three days after the Eclair left. He was taken ill on the 15th of August, and died on the 19th. Therefore it would seem to be quite certain, that the crew of the Eclair could not have been in proximity with any cases of epidemic yellow fever at Sierra Leone; since the first cases of such fever, if we admit them to be such, occurred long subsequently to the departure of the Eclair, and in persons who, at the time of that departure, were absolutely not in the colony. Sir William Pym has asserted that epidemic yellow fever prevailed at the time in Sierra Leone, and that a Mr. Dawson, a passenger on board the Albert, which vessel was in tow of the Eclair, introduced the disease into this latter vessel; but both these assertions seem to be pure assumptions, and unsupported by the least particle of evidence.

It is impossible to overrate the value of the information which this celebrated case of the Eclair has afforded. In all points the facts are precise, unquestionable, and have been thoroughly investigated by competent inquirers. The statement now made by Dr. Bryson completes the

chain of evidence. Let us shortly recapitulate the
First, we have the crew of the Eclair exposed to the
fever in a most intense degree, both in their boat servi
healthy anchorages to which the nature of their duties
find them suffering from a fatal remittent yellow fev
fluid similar to black vomit is found in the stomach aft
is considered by every one, even by Sir William Pym,
fever of the coast. Dispirited by the monotony of th
constitutions impaired by the terrible severity of the du
perform, we find the crew of the Eclair returning to
mitted to recruit themselves on shore, and indulging, u
the irregularities into which men who have been long
ship, in a disagreeable service, are but too apt to fall.
ployed to clean out the Albert, a vessel which had retu
Niger expedition, and which had remained unpurified
the period of the marsh fever, from which her crew
suffered. Finally, the Eclair puts off to sea, and ru
unhealthy station. But now a fever again appears, who
as we are acquainted with them, are the same as tho
which she formerly suffered; it is distinctly remitten
yellow skin, and, in the worst cases, with black vom
or shortly becomes, contagious; for when its ravages
affected by the efforts of the surgeon and commander,
to leave her station and make for a healthier locality,
this fever, and communicates it to the healthy dwel
which she has found shelter. To make the links of ev
the first case of epidemic yellow fever appears in Sierr
after she leaves the colony,—a fact which is proved b
well known and most able colonial surgeon, Mr. F
that time governor of the colony, and who had previ
evidence of the occasional contagious nature of yell
two cases are both in the persons of individuals who
some time after the departure of the Eclair, and who co
have been exposed to poison-particles emanating from
the time of their sojourn at Sierra Leone were unaffe
can be learnt from the diligent observations of those
contagious influences of any other case of epidemic
case at that time had occurred in Sierra Leone.

Can anything be clearer than the whole story?
after reading it, that by far the most probable origi
fever on board the Eclair is to be found in the ende
increased potency was given by the unhealthy cond
who were exposed to them? Can any one refuse to
rate, that the believers in such an origin have adva
little weight, and deserving of no little consideratio
faith which is in them? There remains, of course,
that cases of the true contagious yellow fever may h
Leone before the Eclair left, and may have been
thorities. But if so, it is not a little singular that i
1823, 1829, and 1837, we must have recourse to th
explain the appearance of the early cases, if the loc

ceded. And considering that this is a complete begging of the question, a settling of the thing to be proved, not by a reference to facts but to suppositions, we do not feel inclined to attribute much weight to it.

One very recent case recorded by Dr. Bryson possesses considerable interest. On the 26th of July, 1847, H.M. brig Syren detached a party of three officers and twenty-seven men to the Banana Islands, for the purpose of blockading the Sherbro. This party were partly employed in boat service, partly remained on the island. They held no communication with any other person, sick or healthy. On the 14th of August a case of fever occurred; and subsequently scattered cases appeared till the 20th of September, when the blockade was abandoned. "The first cases," says Dr. Bryson, "were of a purely endemical character; it then assumed a worse form, until it merged into that designated malignant or yellow fever, with its characteristic symptom, black vomit."

The following are the conclusions with which Dr. Bryson winds up his excellent treatise:

> "Firstly, the yellow fever breaks out spontaneously amongst debilitated Europeans in certain localities north of the equator, where fevers of the same type prevail; and as these cease when the former appear, and reappear when the latter ceases, there are no means of accounting for the change, but by supposing that the one is a modification of the other.
>
> "Secondly, it prevails principally, and with the greatest malignancy, at low levels on or near the sea-coast, and in ships of war, from the number of men crowded together; it infests particular spots independently of contagion; a fact which has been ascertained, but the cause is not known.
>
> "Thirdly, it occurs in various degrees of intensity; the development of the symptoms being sometimes rapid, sometimes slow; some cases terminating with one or more paroxysms of fever, and with little detriment to the system, within an average period of from two to seven days, while others, presenting precisely the same symptoms, and differing only in degree as respects the force of pyrexial action, may terminate with the same period in death.
>
> "Fourthly, if it be a distinct disease, it cannot, in its sporadic form, be diagnosed from the common remittent fever of the same regions; in its epidemic form its existence can only be ascertained or suspected by an unusual increase in the number of cases, and the more frequent occurrence of black vomit when it proves fatal.
>
> "Fifthly, it apparently becomes contagious under conditions similar to those which occasionally impart the same character to the fevers of the temperate zones, erysipelas, hospital gangrene, influenza, and probably to cholera; and under these conditions it is propagated by the same laws.
>
> "Sixthly, on the western coast of Africa it has, on several occasions, displayed or acquired great malignancy and the property of propagation on board certain ships, and has thus been communicated to Europeans far distant from the site of its origin; but in the majority of these instances there existed, both in the ships and the localities to which it was taken, concurrent circumstances favorable to the evolution and propagation of febrile disease." (p. 174.)

We have already stated that we do not consider the question at issue completely settled. We cannot, indeed, so consider it, until we have an accurate clinical detail of a series of cases, both of the endemic and the epidemic fever, which may admit of comparison, and enable us to pronounce positively whether these fevers differ or not in kind. At present, all we can say is, that no differences have been proved. The only series of cases of both endemic and epidemic yellow fevers in the same locality, is that contained in Jackson's elaborate work. But we should desire

something a little more in detail than these cases; som
invaluable work on Typhoid Fever, in which the signs
ease are brought out to absolute demonstration. Still
slightest hesitation in stating that the most precise an
dence we can as yet command, and which, in the case of
has been chiefly accumulated by Drs. Bryson and M'Wi
conclusion that those fevers, so variable in intensity, a
the appearance of the mildest and the severest forms, w
display unequivocal marks of identity, and to depend
ditions.

The second work on our list need not detain us lo
occupied with a careful selection of every particle of e
be of use in proving the contagion of yellow fever; every
is carefully excluded; and it really is impossible to av
Sir William Pym on the skill with which he has presen
question. Did we want a special pleader, acute, uncom
and bitter, to discuss a point of scientific inquiry, we k
we should prefer to the chief of the quarantine departme
the qualities which make a first-rate lawyer, ardent
the best for ensuring a dispassionate inquiry after
second edition of the work on Bulam Fever is marvel
the first. In some points it is decidedly improved;
the argument is better preserved, and doubtful points a
kept in the background; in other respects the tone of
unaltered; it is just as confident, cynical, and biting,
when it called forth the indignant rejoinders of Bancroft
reanimated the controversial flames which, after twen
were beginning to pale and languish for want of fuel.

Sir William Pym does not at all favour the view
yellow fever can be an offshoot and derivative from the
bestows very hearty abuse, indeed, on those who ado
He has not, however, advanced a single valid argume
specimen of his mode of arguing, we will very brief
in which he attempts to controvert a position which we
In our previous articles we observed on the cases
Bann, the Scout, and the Kent, that if it was denied
fever could acquire contagious properties, then we ha
assume that on board these vessels two diseases must
the same time. But this appeared to us highly impr
the Scout, and the Bann had been exposed only to
causes of disease. Now we can add, also, the Eclair
that time our deficient information did not permit us

Sir William Pym proceeds to demolish this argum
yellow fever existed at Port Royal, when the Scout sa
In answer to this we can only say that when he tells
he makes the affirmation, we will take them into con
hardly be called upon to retract our deliberate opinio
unsupported by a single additional fact. In the case
the Eclair, Sir William Pym declares that contagio
vailed at Sierra Leone, when these vessels were at

We have, in the present article, given tolerably conclusive evidence that, in the case of the Eclair at any rate, this was not the case.

In the instance of the Kent, Sir William Pym has recourse to a very singular argument. The Kent sailed from an unhealthy anchorage at Port Mahon, for Gibraltar; a fever appeared during the voyage, which was decidedly contagious, and was called yellow fever. Now Sir William Pym cannot venture on the statement that epidemic yellow fever existed at Port Mahon when the Kent sailed, because this could be disproved. Yet he cannot question the fact that the Kent carried a contagious fever, which some of her crew communicated at Gibraltar to other vessels. How does he get over the difficulty? Very simply; he denies that the contagious fever was yellow fever.

The facts of the case were these: on the 5th of August, 1809, the Kent arrived in the bay of Gibraltar. Nearly the whole of her crew had been attacked with a fever, which, according to the surgeon's certificate, "was attended with marked symptoms of yellow fever, and appeared off the coast of Barbary, on the 6th of July." In addition to the common febrile symptoms, the surgeon states that there was continual hiccough and vomiting; acute pain in the epigastrium and hypochondria, general diffused yellowness of the eyes, and the skin of a deep orange tint; syncope in the erect position, frequent hemorrhage from the nose; tongue black and parched; retention of urine occurred, which was relieved by the catheter.*

Now we had and have several reasons for calling this fever contagious yellow fever:—1. There is the statement of the surgeon that it was so. 2. There is the opinion of Sir William Burnett, who also admitted the contagious nature of the disease. 3. The Kent was boarded in the bay by an officer from Gibraltar, who inquired into the nature of the disease, and reported it as contagious yellow fever both to the governor of the garrison, and to the Army Medical Board in England. *That officer was Sir William Pym.* Now when there was this wonderful unanimity of opinion between the rival parties, we did think ourselves justified in believing that little doubt could exist as to the nature of the disease.

What are the reasons, then, which have led Sir William Pym to change his opinion? He states that he has looked over the official report again, and finds the symptoms not those of the Bulam fever. But we cannot admit he has made out his case. The symptoms he now details, and which we have given above, are all those of yellow fever, and not of any other disease. Moreover, relapses, exacerbations of fever, bilious, green, and yellow vomiting, and even increased action of the kidneys, which he says were symptoms present in the Kent fever, and which distinguish it from Bulam fever, may be, and have undoubtedly been, present in true contagious yellow fever. The sore throat and desquamation of the cuticle which occurred in most cases, according to Sir W. Pym's account, we confess we do not quite understand. We should like to know the exact nature and number of the cases in which they occurred.

As Sir William Pym will not derive this disease from Port Mahon, as he cannot admit its production in the ship, he has been obliged to look out for some other mode of origin. He has discovered that on the 22d of June, fourteen days before the fever appeared, the Kent received on

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* These are the symptoms detailed by Sir William Pym in the present edition.

board forty-eight French prisoners. But he has not t
that these prisoners had any fever when they came on b
contrary, so little was it suspected at the time that th
disease, that the surgeon omitted all mention of the fact
although he mentioned it verbally to Sir William Pym
to have made any impression, possibly because, as the
were taken on board at Toulon, which is not in the yell
such introduction might have raised doubts as to the
William Pym's assertion, that the disease in the Ken
fever.

There is not, then, the slightest proof that the French
the disease with them; they may or they may not; no
tell whether they did or not. But certainly it is ut
wrong to assert that they did, simply because in tha
opposite opinion be overthrown. But this is a good
William Pym's method of judging,—first form your
bend your facts to meet them; thirty years hence, if
opinion, bend the facts the other way.

For ourselves, we are quite unconvinced by any of
later reasonings that his early opinion was not the co
must still continue to hold to the belief that a yellow
the case of the Bann and the Eclair, acquired contagi
exist on board the Kent. Not, however, that we atta
portance to this case, but, on the contrary, would at on
too doubtful to be used in the discussion, if the prese
did not seem to be really supported by good evidence.

We trust that ere long the unlucky blockade of the
be abandoned, and that no more victims may be the pre
and unhealthy station; but still we should like to see so
arise out of, and in some small measure make amen
mortality which has prevailed there. Let us hope th
are again thrown in the way of any of our enterprising
of the navy department, they will not fail to bear in mi
which demand inquiry, and, by careful examination, en
gate and comprehend them. It is, we are convinced,
search into the signs of tropical fevers than has yet bee
that these vexed questions of contagion and non-co
dissimilarity, can be satisfactorily and finally elucidate

And, in conclusion, we would observe, that the que
discussing have a much more direct bearing on the pra
which the fevers of our own country should be viewed
readers might at first suppose. We have argued on
(Vol. III, p. 74 et seq.), that the question of the conn
cific *causes* and specific *types* of fever is the most imp
pursue; and it seems almost superfluous to insist upon t
we may derive from investigations carried on, frequen
favorable circumstances, with regard to the fevers of w
causes and whose course present a degree of intensity
country, we have scarcely any experience.

ART. XI.

Selecta Praxis Medico-Chirurgicæ quam Mosquæ exercet ALEXANDER AUVERT, Augustissimi omnium Russiarum Imperatoris Consiliarius Status, Cæsareæ Universitatis Mosquensis Professor P. O., Medicus senior Nosocomii Urbani, &c. &c.—*Parisiis et Mosquæ,* 1848.

Selections from the Practice of ALEXANDER AUVERT, at Moscow, Professor in the Cæsarean University, and Senior Medical Officer of the Urban Hospital, &c. &c.—*Paris and Moscow,* 1848. Folio.

WE have before us six fasciculi of this magnificent work, which, as its title denotes, is destined to comprise a series of selections from the practice of its author. The plan and arrangement of the work are, that, when completed, it should consist of 120 coloured plates, illustrating different forms of disease, medical and surgical, each being accompanied by its particular sheet of text, descriptive of the case which forms the subject of the drawing, and explanatory of the illustration. There will be four divisions of the work, each comprising six parts, and each part containing five plates: the whole will take four years in its appearance. The engravings, which are executed in Paris, under the inspection of M. Tardieu, are truly splendid specimens of the art of copper-plate engraving, as applied to the illustration of diseased structures; they are coloured, and retouched by the hand to give them greater accuracy and faithfulness. They are almost unmatched, and certainly not surpassed, either in elegance of execution or fidelity to nature, by anything of the sort we have ever seen. The Latin text which accompanies the illustrations, and the paper on which it is printed, are both in keeping with the plates.

In the present series, issued during last year, and constituting the first division of the work, the illustrations include certain affections of bone, some malignant diseases and peculiar forms of nævus, &c. There seems to be no pretension to pathological uniformity of plan, nor attempt to embrace in a systematic way any particular class of diseases; at least as far as we can judge from the engravings under our inspection. Therefore, in this our brief notice, we merely purpose to glance at the drawings and text, to give our readers an idea of the subjects selected and the mode in which they are treated.

In his preface, our author professes to have arranged his "selections" according to the regions affected, under the following six heads. "The head, the neck, the vertebral column, the chest, the abdomen, the extremities." The present division does not extend beyond the first heading.

The five plates comprised in Part I. illustrate the following subjects. *a.* "Hyperostosis and fungoid exostosis of the calvaria," occurring in a young girl of 19, and of a scrofulous habit. We have seen a very similar case, but in a much older person. *b.* "Perforating ulcer of the cranium, and an extensive fracture of the calvaria;" in different individuals. *c.* "Syphilitic caries of the base of the skull." The part affected is the anterior or frontal division; and the disease appears to have extended to it by contiguity, from the spongy bones of the nose; which, according to the history, were the parts first attacked. Further, the membranes of the brain were affected; nay, the very structure of the cerebrum to its centre (*usque ad ipsas hujusce organi partes centrales* faculties,—"sensibility, intelligence, judgment, will, i impaired, until just before death; when the patient exhaustion. We have seen cases quite as significant, i point of view as the above, and quite concur in the re our author closes his case, to the effect that we have before we can venture to dogmatise on the special fu that part of the encephalic centre.

d. We next have a singular specimen of "horny ex os frontis." It stands out perpendicularly from the head, is conical in form, and has, we should judge, three inches at its base. It appears to have attracted at first, and to have been slow in growth. This curio however unique) was removed by the author, who seem it as having its origin in the obstruction of the excret ceous follicle.

Lastly, the fifth plate of this division exhibits a huge de of the right frontal sinus," pressing out the eye, and in whole of the corresponding side of the face.

The first plate of the second fasciculus exhibits the sented by a case of "elephantiasis, combined with nævi and neighbouring part of the scalp." The details which occurred in a girl of 22, are interesting, bu admit of an abstract of the whole. The carotid arter tation of analogous cases treated by Dupuytren, Trave and though all pulsation in the tumour ceased, and the on the twenty-second day, yet the diseased parts ulcer the patient ultimately died, soon after a sudden attac the *left* side. Her health seems to have been much hand, and she was of a scrofulous constitution.

An illustration of a case of "elephantiasis of the sc region" succeeds; and certainly a most hideous, and at crous effect is produced by the back view which is occurred in a young and scrofulous woman.

The remaining three figures of this fasciculus are illustrations of "Fungus hæmatodes of the eye," in ferent stages, and one of "Encephaloid fungus, of t an adult. They do not present any peculiarities calli ment. The first plate of the next fasciculus represe tifully, the appearances of the encephaloid and mela removed.

In the third fasciculus we likewise have two drawin lower eyelid; or, rather, one exhibiting the disease, plastic operation by which it was cured: also a singul angiectasis of the tip of the nose; and one of a simil the right cheek: these two latter both occurred in chil case, milder means having been tried ineffectually, t was removed with the knife, the copious bleeding iced water and compression. The boy made a good re of a month.

The projecting and pendent nævus from the chee compression of both carotids; and, finally, the use of the actual cautery. We have removed a nævus from beneath the chin, similar in form but more limited in extent, by needles and a ligature so applied as to strangulate the morbid growth; the base of the disease was probably too extended in the instance before us to admit of this mode of treatment, which certainly should be preferred to the knife wherever it is practicable, and especially in young children.

"Lipoma of the right cheek" constitutes the first illustration of the fourth fasciculus. The tumour, of the size of a goose's egg was removed, and found, on section, to contain a calcareous concretion, which proved to be phosphate of lime, suspected to be deposited in the duct of the parotid gland.

The other four plates of this fasciculus, illustrate two cases of "osteosarcoma of the upper jaw." The disease, in the first case, occurred in a female, and attained an enormous size; the extent to which the bones were disorganized is shown in the second engraving associated with this case. In the second case, the morbid growth extended more into the mouth, and was removed, the appearance of the patient being exhibited both before and after its excision: the case appears to have been most successful.

The same subject occupies the whole of the next fasciculus, and the first plate of the sixth, there being double plates to each of the three cases given; all three of them being subjected to operation, and successfully. In the first of these the disease had grown to a large size, both outwards, so as to protrude the cheek, and inwards into the mouth. In the second, the disease was limited to the symphisis of the upper maxillary bones. In the third case, it took its origin from the same part, but had attained a greater magnitude, and protruded considerably from the mouth. The knife did its work effectually in all; but the deformity left in the last case was much more apparent than in the others.

Lastly, we have a huge "cystic tumour of the left parotid region" an equally large "steatoma of the right parotid region;" a case of "lipoma, with angiectasis of the upper lip;" and one of "contraction of the mouth."

The first of these (the cystic tumour) occurred in a man of 40, and its origin was dated back some twelve years. The tumour was removed by excision, large branches of the temporal, transverse facial arteries, &c., requiring ligatures. On laying open the cyst, it was found to be divided into several loculi by fibrous septa, and contained a gray atheromatous matter, in some parts granular, and mixed with a thick, grumous fluid. The author seems disposed to refer its origin to the obstruction of a sebaceous follicle.

The subject of the steatoma was a woman of 50, and the disease had existed in her likewise for about twelve years. The base of the tumour extended from a line drawn from the outer angle of the eye to the upper part of the ear, to beneath the horizontal ramus of the jaw, and nearly equalled the head in size. Though firm at its surface, it presented, on section, a softer, lardaceous character towards the centre. The colour of the investing integuments was unaltered, and the tumour itself but sparingly moveable. The patient did well.

The case of lipoma with nævus occurred in a scrofulous girl of 22,

who had received a blow on the upper lip (the sea
five years before. The tumour had attained the size
was somewhat irregular in its circumference, and its su
small vessels. The accumulation of these towards it
bright florid hue of this part betrayed the seat of the
lip was removed by a sweeping incision, extending fro
of the mouth to the other; and after the hemorrhage w
application of ice, a further dissection was performe
being able to procure sufficient healthy integument to
space. This, however, was found impracticable, and
proximated by means of sutures and strips of adhesive
circumstances would admit of. The patient recovered
to employ an artificial covering to the teeth in place of t

The deformity,—a contracted mouth,—which is repr
drawing before us, was the consequence of a contracti
burn, the patient's clothes having accidentally caugh
before, and the trunk having suffered in common wi
oral aperture was so much reduced as not to admit of t
the little finger. A modification of Dieffenbach's operat
mucous membrane was preserved and employed in p
of the divided parts and subsequent contraction, was res
apparent success. The case and operation put us in
one under our care some time since; the cause of the
case referred to was lupus, which had involved the w
practised Dieffenbach's operation, removing a portion o
the cheek on either side to the required extent, but pre
membrane, which was then divided horizontally in its
upwards and downwards, and so made fast to the ski
the operation did not equal our expectations; but the
the parts subjected to the knife was so dense and almo
not to offer a fair opportunity of testing the value of the la
ingenious suggestion.

We have only to add, in concluding this brief not
(which is in Latin) proves our author to be an accompl
pathologist. We shall be glad to take another glance at
he may afford us the opportunity by the issue of furthe

Art. XII.

The Three Kinds of Cod-Liver Oil comparatively consid
to their Chemical and Therapeutic Properties. B
m.d. Translated from the German by Edward Ca
Appendix.—*London,* 1849. pp. 176.

The above is a translation of a well-known work
which originally formed Dr. de Jongh's Inaugural Di
written in Latin. It was subsequently translated int
historical account of cod-liver oil was added, and it h
an English garb under the auspices of Dr. Carey. I
siderable value, as the author has had unusual opportu
different specimens of cod-liver oil, and has investiga

which will place all the most important facts before our readers.

Three principal kinds of cod-liver oil are met with in Germany, viz. the brown, the light brown, and the pale. The differences between these oils do not depend so much on the different species of Gadus from which the oil is prepared, as on the mode of preparing it. The pale oil is procured from the livers thrown into tubs, and allowed to become putrescent; the oil runs off by itself, and is skimmed. The brown oil is procured both by boiling the remaining portion of the livers, and by boiling the fresh livers. The light-brown oil appears to be formed either by applying moderate heat to the fresh livers, or by allowing the pale oil to stand some time upon the livers, or it is simply pale oil which has got old in the warehouse. It does not appear that the oil is obtained by simple pressure, or by heat and pressure, or by bruising the livers with tepid or cold water, and filtering, all of which methods are followed in this country.

The species of Gadus which are most plentiful, and are therefore chiefly used, are the dorse and the pollack (Gadus or Asellus striatus seu callarias, and pollachius); the coal-fish (Asellus or Gadus niger), the whiting, and the ling are more or less used in the localities they frequent. Other species of Gadus are occasionally employed, and the fishermen, in fact, seem to use all indifferently. The oils from the haddock, the poor (Gadus minutus), and the ray are employed to adulterate the better kinds of oil.

All the oils are slightly acid, have a peculiar smell, which differs in intensity in each case, a slightly bitter and empyreumatic taste, and a specific gravity of from 0·923 to 0·929. The chief substances contained in these three kinds of cod-liver oil are a neutral fat, biliary matter, iodine, bromine, phosphorus, butyric and acetic acids, a substance called gaduine, which adheres very strongly to the oleic acid and appears to be inert, and several salts.

The proportion of these ingredients differs in the case of each oil. The light brown oil contains less oleic and more margaric acid than the pale; and the brown contains a still lessened proportion of oleic acid, and a more heightened proportion of margaric, than either. The matters derived from the bile are considerably more abundant in the brown than in the two other oils. This oil also contains more acetic and butyric acids.

The following table gives the final results of Dr. de Jongh's analyses:

100 *parts of cod-liver oil contain—*

	Brown.	Light brown.	Pale.
Oleic acid, with brown substance (gaduine), and two peculiar bodies . . .	69·78500	71·75700	74·03300
Margaric acid	16·14500	15·42100	11·75700
Glycerine	9·71100	9·07300	10·17700
Butyric acid	0·15875	......	0·07436
Acetic acid	0·12506	......	0·04571
Fellic and cholic acids, with some oleine, margarine, and bilifulvin . . .	0·29900	0·06200	0·04300
Bilifulvin and bilifellinic acid, and two peculiar substances	0·87600	0·44500	0·26800

	Brown.	]
A peculiar substance, soluble in alcohol .	0·03800	
A peculiar substance, insoluble in water, alcohol, and ether	0·00500	
Iodine	0·02950	
Chlorine, with some bromine . . .	0·08400	
Phosphoric acid	0·05365	
Sulphuric acid	0·01010	
Phosphorus	0·00754	
Lime	0·08170	
Magnesia	0·00380	
Soda	0·01790	
Iron	a trace	
Loss	2·56900	
	100·00000	1(

The chief differences between these and the older
the exclusion of certain ill-defined resins, viscous anc
and in the inclusion of biliary matters, acetic and bı
gaduine. The phocenic acid discovered by Chevreu
dolphin, and which others have found in the cod-live
a mixture of butyric and acetic acids.

This last-named substance, gaduine, from Gadus, w
by De Jongh, is a brownish matter, so intimately ad
acid, that it accompanies it through the whole proce
and separation. It is obtained by separating the
saponifying process from the solidified soap, filteri
with dilute sulphuric acid. Brownish-yellow flakes t
face, which are washed, dissolved in spirit, and obtai
solution by evaporation as a brown residuum. This i
partly soluble in ether, and at first soluble in alcoh
solutions and evaporations, becomes at last partly ins
It can thus be separated into two substances,—one s
soluble, in alcohol; this latter substance can itself b
other substances by the action of ether, which dissol
other. These several soluble substances form variou
lead, from which their ultimate composition can be de
to contain from 66 to 69 per cent. of carbon, 7 to 7·5
gen, and 23 to 26 per cent. of oxygen. Dr. de Jong
as a hydrate of a basic radical, whose composition
7 (C_5 H_6 O), with two atoms of water. From th
atoms only one has as yet been displaced by a metall
composition of the radical is at present hypothetical
insoluble substances obtained by alcohol seem to be
modifications.

From the preceding analysis, it appears that the
brown oil have a great affinity with each other, being
inorganic substances—a fact which supports the con
is formed by keeping from the other. The brown c
from both, in containing more of the principles derived
of the liver—viz. biliary matters, butyric and acetic a
By preparing the cod-liver oil from fresh livers, b

oil may be procured, or a pale yellow oil of a specific gravity of about 0·934. This is often nearly free from disagreeable taste and smell. It is a question, however, how far the attempt to diminish the nauseous properties of the oil can be carried without impairing its efficacy. The oil which has made the reputation of the medicine in Holland and Germany is the brown oil, which is perhaps the most nauseous and disagreeable of any. It is very much to be wished, that accurate analyses of our pale oils were made, in order to determine the comparative differences between them and the darker kinds of oil. Possibly, we may be injudiciously consulting the palates of our patients.*

The cod-liver oil is best prescribed with wine, coffee, aniseed, or cinnamon water. It may be made up into a syrup, with gum-syrup and orange-flower water; and if this is still nauseating, hydrocyanic acid or a little creosote may be added to it. A cod-liver-oil soap is recommended by Deschamps, made by mixing 600 parts of the oil, 20 of water, and 80 of caustic soda. A mass is formed, which with tragacanth powder may be made into pills, or may be used externally as a liniment, dissolved in spirit, or mixed with water and iodide of potassium, as an ointment.

So long ago as the year 1771, Percival was acquainted with the value of cod-liver oil as a remedy for chronic rheumatism. It was used also in the infirmary at Manchester, by Bardsley, some time after this period. It was not, however, till 1822 that the attention of the profession was directed to it, by the publication, by Schenk, of 16 cases of chronic rheumatism which had been successfully treated by it. Four years later, he published 20 fresh cases, confirmatory of the favorable opinion he had formerly advanced. Since this time the publications on cod-liver oil, especially in Germany, and to a less extent in France, have been exceedingly numerous. It was little known in this country, till the publication of Dr. J. Hughes Bennett's work, in 1841. The evidence of its utility at first was rather contradictory, but of late years the testimony in its favour has been certainly gaining ground, and it may now be considered as having an undoubted and important place in the Pharmacopœia of, at any rate, all these northern countries.

On looking over the various treatises which have been published on this subject, and on weighing the opinions and statements which have been expressed in them, it is indeed abundantly evident that cod-liver oil is an agent of no inconsiderable power. It is also evident that its action is chiefly in one direction, and is exerted almost entirely in modifying, restoring, and improving the several processes partly of primary, but chiefly of secondary assimilation, the derangement of which constitutes the basis, as it were, and origin of certain symptoms which we are accustomed to treat, under various names, as special diseases. In dyspepsia, that is, in diseases resulting from some defect of primary assimilation, its effects have not been very accurately studied, chiefly perhaps from the difficulty of finding pure and uncomplicated cases of this affection. It appears to be most useful in the atonic varieties, and in those cases of inflammatory gastro-duodenal dyspepsia, in which, after reduction of the more prominent symptoms, the digestive powers of the stomach and duodenum recover themselves with difficulty. But in any cases in which the dyspeptic symptoms are dependent on more serious lesions, on ulceration of the stomach, or on incipient organic disease of the walls, &c. the cod-liver oil appears to be unsuited, and even if retained on the stomach, to be inefficacious, or perhaps hurtful. It is chiefly in affections proceeding, as far as we at present know, from secondary malassimilation (to use the term of Dr. Prout), that the oil appears most useful. Such appears to be the case in chronic rheumatism, in rickets, in the various affections which are the local manifestations of the scrofulous diathesis, scrofulous caries, ophthalmia, tabes mesenterica, tubercular peritonitis, &c.; in phthisis pulmonalis, especially in the early stages; and in various chronic eruptions of the skin, as eczema, herpes, and some forms of impetigo. It appears to be more useful in these diseases of the skin, according as they can be traced back to some disorder of nutrition, and more especially if this borders on the condition which occurs in the scrofulous diathesis.

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* It is also very much to be desired, that accurate experiments should be made with cod-liver oil, and other oils resembling it, for the purpose of determining how much benefit is to be ascribed to the mere fatty matters, and whether it would be possible to substitute a cheaper medicine in hospital and parish practice. At present, no doubt, this experiment is actually going on on a large scale; as very little genuine cod-liver oil is in the market, and the greater portion of that sold by the smaller chemists is probably anything but pure. We shall see whether the reputation of the medicine will maintain itself among the poorer classes; if it does, the question as to whether other animal oils can be substituted for the cod-liver oil receives an answer, though not a very certain and accurate answer, in the affirmative.

In addition, the oil has been used in a vast number of other affections, with an amount of benefit less marked and determinate. In true gout, psoriasis, porrigo, osteomalacia, in atonic amenorrhœa, in worms, and in the various sequelæ of smallpox, measles, and typhus, the opinions of its utility are very contradictory. It has appeared to be possessed of little power in Bright's disease, diabetes mellitus, and some other deep-seated and obscure affections. Cod-liver oil has also been found useful in cases in which tonics are indicated; as in convalescence after influenza, bronchitis, pneumonia, pleurisy, and other cases in which the disease having been partially or entirely removed, the constitution requires to have cautiously supplied to it both materials from which its impaired tissue may derive new force, and tone by which those materials may be effectively assimilated.

An opinion has been advanced, on very insufficient grounds however, that chronic rheumatism and tubercular consumption are in some degree antagonistic to each other. But independently of other arguments against such a view, we might be led to question its correctness by observing that the two diseases, or series of diseases, over which cod-liver oil appears to exert a considerable action, are chronic rheumatism and scrofulous affections. While in most other cases the benefit resulting from the use of the oil appears to be simply owing to its common tonic powers, in the above-named diseases there almost appears to be something specific about its action. In the work before us, as in other treatises on the same subject, we find very strong statements respecting the utility of the oil in phthisis. Some of these are derived from previous writings, others are made by several eminent Dutch physicians, whose opinions on this point Dr. de Jongh ascertained. Suermann, Schroeder Van der Kolk, Loncq, Pruys, Van der Hoeven, Sebastiani, and Suringar, all agree in attributing very great benefit to its employment. They almost all, however, consider that its good effects are chiefly limited to the early stages, before softening has deposition of tubercle appears to have been arrested, and the tubercles already formed have retrograded in one of the usual ways, most commonly by cretaceous deposit. When softening has occurred, the virtues of the oil are more questionable; for while occasionally it appears to be of immense benefit, at other times it has exerted no effect whatever. In phthisical diarrhœa it has seemed decidedly hurtful.

In an able paper by Dr. Williams, which has lately appeared,* the utility of the oil in phthisis, particularly in the latter stages, is rated even higher than in the above paragraph. Dr. Williams's conclusions are based on the experience of about 400 cases. Of these, 234 are recorded in his note-books, and are therefore susceptible of numerical classification and analysis. Among these 234, there were 9 cases in which the oil disagreed, 19 in which it appeared to do no good, and 206 in which its use was followed by marked and unequivocal improvement. Of the 206 patients, 62 had cavities; all of these improved materially under the use of the oil; in 34 the improvement has continued; in 11 the improvement was only temporary; in 17 the patients were lost sight of. In 100 patients the tubercles had commenced to soften, but actual cavities had not formed, and both physical and general symptoms materially and rapidly changed for the better. The process of softening seemed arrested, as the moist rhonchi in the supra- or infra-clavicular or the supra-spinous regions gradually ceased, the dullness more or less disappeared, and at last, vesicular breath-sound returned, and no physical signs whatever remained, except a little prolonged and, perhaps, tubular expiration. Coincidently with these changes, the constitutional symptoms disappeared. In the remaining 44 patients, the disease was in the early stage, and the results were not less satisfactory. Eleven of the advanced cases are related as illustrations of the degree of improvement.

In estimating the exact value of this evidence, it would be necessary to strike off from the list a certain per-centage of improvement which seems to occur in phthisis from the employment of any judicious treatment,† such as a regulated temperature, change of air, proper diet, regular habits, and various medicines, according to circumstances. The exact amount which would be thus struck off, cannot, from want of data, be at present determined, but there is no reason to believe that it would be very considerable. A certain amount of benefit would still remain, which must be ascribed to the cod-liver oil alone.

Dr. Williams's estimate of the value of cod-liver oil is certainly higher than that of most writers, and possibly higher than it would have been had the remedial measures which were employed in addition to the oil been taken into the account. But yet the opinion of a physician so eminent, and so skilled in the diagnosis of phthisis, is necessarily of great weight.

Dr. de Jongh has attempted to determine which kind of oil is most useful in scrofula and rheumatism. He made a series of comparative

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* London Journal of Medicine, Jan., 1849.

† Dr. Walshe's excellent Report on Phthisis, in our Fifth Number, indicates that the improvement under various modes of treatment is greater than was anticipated. Cod-liver oil was probably employed in many of Dr. Walshe's cases, so that we cannot tell the exact amount of improvement not referable to the employment of this agent.

trials, using the oil as the only remedy, and choosin
patients who, as far as regarded the kind and con
the external circumstances surrounding them, &c., a
subjects of comparison. The general results of the
in favour of the brown oil, the cures being effected in h
by the other oils; the light brown and the paler oils se
The cases are, however, so very few in number, six i
do not feel disposed to attach much importance to
various disturbing and modifying circumstances in
so numerous, that they can only be got over by a mu
experiments.

We shall not enlarge on the *modus operandi* of co
rheumatism and the scrofulous and phthisical diathe
lations have been advanced to account for its effica
conditions of the habit, but we must confess that the
as crude and unsatisfactory as most hypotheses of th
appears probable that the oil does not act simply as
has been supposed by some practitioners. If this we
depend simply upon the fat contained in the oil, but
other oils and fats can be substituted for the cod-live
the doses in which this last often operates most bene
to allow us to consider its use to be confined to tha
The utility of the oil depends then possibly on some p
which it contains; but which of these is the active age
mined. The iodine is in small quantity, but yet evid
the system, and can often be detected in the urin
butyric acid, and other agents do not produce the be
administered separately; the Gaduine appears alto
the fact of the brown oil appearing more useful th
has been surmised by De Jongh that the biliary matte
proportion in the brown oil, may be the active ingred
is some foundation for this opinion appears from th
without knowing that the oil contained biliary ma
could become a substitute for bile, and to this in part

Perhaps, as suggested by Dr. de Jongh, the proper
solely on the phosphorus, the iodine, the fat, or th
on the mixture of all these ingredients, the union of
a compound which acts in a way which cannot be im
components.

As we are at present almost entirely ignorant
peculiar malassimilation, if we may venture to use t
rise to the scrofulous diathesis, or to the tubercul
tinction is to be drawn between them, and as we a
the active principle or principles of the cod-liver
appear in the highest degree premature to attempt le
the method in which the oil so usefully operates in t
any one review the various hypotheses which have
candidly confess what amount of satisfaction he ha
it has been supposed that the oil is converted into al
an essential ingredient in the fat-globules which h
that it gives to the albumen the necessary additi

formation of cells, without which the albumen, incapable of organization, would be deposited as scrofulous or tuberculous deposit; that it affords to the chyle a finer "molecular base," from which it is assumed that a better plasma may be formed; or that its beneficial influence is connected rather with the aid or the relief, which the highly carbonaceous and combustible particles of the oil afford to the principles which are ordinarily consumed in the respiratory process, than with any direct influence on the process of nutrition. It would be most easy to show how scanty is the evidence which can be advanced for any one of these hypotheses; but the assumptions on which they are founded are so transparent, that it would be a waste of time to do so. It is much better, in the present state of our knowledge, to determine from experience alone the utility of our remedies; and if hypotheses are to be formed of their *modus operandi*, to be careful that such doubtful speculations are not allowed unduly to influence our notions of the morbid processes in the disease which we are treating, or to bias our conclusions of the value of the remedy itself.

We have spoken very highly, in the preceding pages, of the cod-liver oil, as a remedy in several most intractable and deep-seated affections. Is there not some fear lest the utility of this medicine may have been overrated by the zeal of those, who deem they have at last discovered a means of checking the inroads of some of those diseases, which have been the *opprobria medicorum?* How many specifics, alas! have we not been called upon to receive as almost infallible; how many have maintained their reputation; how many of these ephemeridæ have attracted all eyes by the brilliancy of their colours, and the elegance of their forms, and yet while still we gazed, the colours have become dim, and the forms have died away into obscurity! Have we not reason to dread that a reputation, born in a moment, and sanctioned by an experience so limited, cannot have its foundations planted in the immoveable basis of undeniable demonstration? Will our successors be decimated by consumption even as ourselves have been, and will the next generation record the name of our boasted remedy among the phantoms which have been worshipped as realities? We do not indeed think so. We cannot but believe that testimony so strong as that which has been given by most competent and, as far as we know, unprejudiced witnesses, in proof of the real and decided efficacy, in certain diseases, of cod-liver oil, cannot be altogether erroneous. It may perhaps be over-coloured. This is hardly to be avoided, until a greater experience has indicated the true extent of its utility; but we do firmly believe that this wider experience, while it may limit its powers in some respects, will but more certainly establish the undoubted benefit which we shall derive from the judicious employment of this remarkable agent.

ART. XIII.

1. *On Healthy and Diseased Structure, and the*
Treatment for the Cure of Disease, especially Consu
founded on Microscopical Analysis. By WILLIAM A
Licentiate of the Royal College of Physicians.—
pp. 320. With Four Plates.
2. *Thoughts on Pulmonary Consumption; with an Ap*
of Torquay. By WILLIAM HERRIES MADDEN, M
Torbay Infirmary and Dispensary, &c.—*London*
pp. 220.
3. *De l'Analogie et des Différences entre les Tubercu*
Mémoire qui au Concours, fondé par Portal, a mé
Médecine une mention honorable. Par A. LEGRAND,
de la Faculté de Paris, &c. &c.—*Paris,* 1849. 8v

IN our review of the first two of these works in th
we intimated our intention of returning to the more
ation of the question discussed by M. Legrand, as t
formity and distinction between tubercle and scrofula
is not yet decided in the minds of many intelligent p
titioners both in this country and elsewhere, and as
that great misconception is prevalent among those w
to it any large amount of special consideration.

In our treatment of this subject, we shall not t
confine ourselves to the order observed by M. Legra
way, which the reader is not assisted in discovering
tents, or even by any complete subdivision into chap
appropriate headings. His object, indeed, would a
—assuming from the marked difference as to fatality
scrofula (in their popular acceptation) an essentia
them—to point out the particulars by which this dif
nised, rather than to discover by observation and
manifested effects of the supposed two principles, w
diversity is true or not. It seems desirable, in the
in most discussions, to secure, by appropriate defin
as to the meaning of the leading terms employed,
considerable variation in the use of the word Scrofula
necessary, when the question of the relations of th
proposed for discussion, that we should have a defini
is intended by the former. M. Legrand does not
matter of primary importance; for it is not till pa
work, that we can gather, except by inference, wh
term. We then find him adopting a signification
respond pretty closely with that attaching to the term
or diathesis; for he defines scrofula to be "*the caus*
lymphatic system, most commonly characterised b
puration of the lymphatic ganglia of the neck, and
of those of other regions of the body; by swelling
bones, and by superficial ulcerations of the skin,"

element or principle, though of indeterminate nature and seat; and he gives further precision to his meaning by adopting the following passage from the authors of the 'Compendium:'

> "L'élément morbide, qu'on appellera virus ou vice scrofuleux, comme on voudra, a pour effet: 1. De donner aux maladies qui se développent un cachet particulier et reconnaissable. 2. De déterminer des symptômes morbides qui affectent certains siéges de prédilection. 3. De produire un état général, qui est déjà la maladie, mais qui ne donne encore lieu à aucun symptôme local bien tranché; c'est plutôt par l'ensemble des phénomènes qu'on reconnâit cet état, qui est désigné sous le nom de *constitution*, de *diathèse scrofuleuse*." (p. 250.)

It is therefore evident, that when M. Legrand refers to the question of the identity of scrofula and tubercle, he employs the former word in the only sense under which that question can reasonably be entertained: and the question itself may be thus stated;—whether this scrofulous morbid principle, manifesting itself in a diathesis, as well as in certain visible changes ordinarily called scrofulous, is the same as that which is assumed to hold a corresponding relation to the structural changes known under the name of tubercle. It may seem to some superfluous, if not trivial, that we should take any pains to indicate this as the only meaning of the term scrofula, under which the question of its identity with tubercle is worth discussing; for it may very well be asked,—who has ever maintained this identity in any other sense, or who, encouraged even by the weight of Broussais's authority in its favour, could for a moment be supposed to argue for the identity of the matter of a scrofulous subcutaneous gland and a tuberculous deposit in the lungs? Our reply is, that we have frequently heard the question referred to, as though the latter, and not the former, were the point in dispute. We even remember, at a meeting of the now defunct Medical Section of the British Association, to have heard an eminent author attack the views of Sir James Clark, and others who agree with him, under this same misapprehension; and what is of more importance, the arguments employed in the solution of the question (and this remark applies to others besides those adduced in the work before us) continually betray a forgetfulness, at least, of what alone is really at issue. It may be said that we are here employing the term scrofula in a way which, though sanctioned by the definition of M. Legrand, is not usual in this country. There is no doubt that with many, perhaps most, the designation of scrofula is restricted, as by Mr. Phillips, to the *manifested* disease. By the last-mentioned able writer it is indeed still further limited to those among the various affections usually, and we believe correctly, considered scrofulous, in which there is actual deposition of scrofulous material; nay, in one place, he confines it to "disease of subcutaneous lymphatic glands." The "scrofulous constitution," says Mr. Phillips, "is not scrofula." Now the terms constitution and diathesis are certainly both employed somewhat loosely in medical speech and writing; they are confounded by the author of the quotation we have given above from M. Legrand's work; but adopting the sense in which they are severally used by Sir James Clark, in connexion with tubercular disease and scrofula, the latter word being taken as equivalent to cachexia, we contend that it is only in the sense of the diathesis or cachexia—that is, of that particular condition of the system which gives rise to the deposition of the morbid product,—be this resolvable a morbid poison in the blood, or some more s dyscrasia,—that we can discuss the identity of scro

The early pages of M. Legrand's memoir are to us of the great mistake which is made, when, in the in as of any other natural phenomena, the inductive for its converse. In pointing out, in the first place, scrofula and tubercle, he naturally enough refers to i be abundant in the experience of every practical m those enlargements of the cervical glands, ordinarily were associated with, or speedily followed by, tube other internal organs. But, instead of seeing in this mon as to have impressed his own mind with the jus that in cases of doubtful phthisis, the coexistence c largements was to be taken as a diagnostic,—inst this fact a strong presumptive evidence in favour of of the cause to which both tubercles and glandul owe their existence, he is driven, by his foregone as ence of two distinct morbid principles, to explain s posing that the glandular enlargements, though n outwardly from scrofulous swellings, and though oc ance) under precisely similar conditions of constituti as predisposing to scrofula, are not scrofulous at all, l i. e. a complication of the primitive disease (the zation) or instances of mere sympathetic adenitis. says, "the microscope, if it had been used, *would* culous globule in the midst of the congested mass *would have* shown nothing but pus."

Now, we need take no pains to show that, so fa just alluded to go, their weight is all on the side of t tell for the identity of tubercle and scrofula. That such instances, would exhibit to the microscopical c difference and distinction from those, which, occu ternal tubercular complication, are unhesitatingly s wholly unproven. But as the microscopical stru product is thus referred to, not only by M. Legrand including Dr. Madden, as affording distinctive cha before we proceed to the examination of other argu far such distinctive characters can be depended up importance should be attached to the microscopical in determining the relations of tubercle and scrofula

Here, first, it may not be amiss to notice the sing exists between those, who agree in regarding the m of tubercle as of diagnostic importance. Of thos Lebert, who is followed by Legrand, assert that tub cular elements, by which it may be distinguished products; that such corpuscles are always and tuberculous matter, while scrofulous matter is dev this is a sufficient character of distinction between *therefore* between the morbid principles from w arise. On the other hand, Dr. Madden, no less affirms his agreement with Mr. Phillips, who "acknowledges that there are no appreciable differences between scrofulous and tubercular matter, so far as their physical and chemical characters are concerned;" but he as distinctly expresses his disagreement with the last-named writer, who lays "little stress upon this confessed fact," as respects its bearing on the identity of scrofulous and tuberculous affections. "To me, on the other hand," he continues, "it appears all important, constituting a difficulty from which it is no easy matter to make a scientific escape." (pp. 55-6.) That there should exist, between those who place reliance on the revelations of the microscope, and who equally regard these revelations as of fundamental significance in determining the question at issue, so wide a diversity of statement as to what these revelations are, may seem to some strong reason for suspecting that such reliance is altogether misplaced. To us, however, these discrepancies appear to admit of being satisfactorily explained, without prejudice to the particular method of investigation employed; though by means of considerations which tend to show, that an exaggerated view has been based upon some of its determinations, simply from want of a more extended study of them.

This view is derived from the fact we have stated above, that there are to be seen in tuberculous matter, in certain conditions, besides a multitude of smaller granules and molecules, bodies of cellular type, in some specimens existing in large numbers and close together, but in others, sparse and few; always, however, presenting characters of sufficient definiteness, as it is thought, to admit of their being regarded as *sui generis*. The matter discharged from a scrofulous gland, on the other hand, does not exhibit any similar structures, or anything, indeed, in addition to the characteristic forms of pus, excepting irregular molecules, or granular matter, such as is also met with in pus formed under many different conditions. So far, then, the inference would seem to be that drawn by Lebert, viz. that the difference in molecular composition is but an expression of an essential pathological difference. But before this inference can be considered as established, we must be satisfied of the constancy and uniformity of the conditions asserted, and also that they cannot be consistently interpreted in any other way. Now it may be observed, that good microscopists have failed to detect, in some undoubted specimens of tubercular matter, the corpuscles here regarded as diagnostic; and that others (as Mr. Gulliver) have spoken of them as few and irregular, and as having more the seeming of "effete or shrunken primary cells," than as exhibiting appearances which are entitled to be regarded essentially characteristic. In that form of tuberculosis in which the new product is infiltrated through considerable portions of the pulmonary parenchyma, we have often failed, after long and careful search, to detect any elements answering to the assumed tubercle-cell. The same has many times been the result of our examination of tubercles on the peritoneum. On the other hand, let it be considered whether their absence in scrofulous glands is asserted upon sufficient grounds. The evidence being merely negative, is of itself a reason for caution in the use of it. But is it not likely, that an examination of these enlargements at an early period after the deposition of the scrofulous matter—an examination, for which opportunities do not very frequently present themselves, or which at any rate has not, so far as we know, been extensively made—might disclose corpuscles, which, after the establishment of suppuration, may reasonably be supposed to have undergone destructive changes? Every one, practised in these applications of the microscope, knows how extremely rare it is for any corpuscles, corresponding to the accounts given of the tubercle-cells, to be detectible in the matters expectorated by phthisical patients: indeed softened tubercle is known to be often entirely devoid of any forms but granules, molecules, or shreds. Whether such corpuscles commonly exist in the enlarged glands of scrofulous subjects or not, at some period of their growth, it is admitted, on all hands, that the cervical glands do occasionally become the seat of undoubted tubercular deposit. And so far as all the facts now stated go, they seem to us to justify a presumption, that the presence of the cell, supposed to be characteristic of tubercle, is not a constant phenomenon; and that its presence in greater or smaller amount, or its entire absence, may not improbably be due, in part to the modification impressed on the tubercular blastema by the seat of its deposit,—in part, perhaps, to the manner of its deposition, as, e. g. whether preceded or not by the inflammatory process,—and probably also to other conditions, affecting the rapidity with which it is produced. If these inferences should be confirmed by further observation, they will evidently afford a sufficient explanation of the discrepancies above alluded to, in the descriptions given by different well-qualified observers, of the microscopical characters of scrofulous matter and tubercle. Let us now inquire, how far they are supported by the characters of the corpuscles themselves, which are to be seen in genuine tuberculous matter.

These corpuscles are invariably described as irregular in form and size, with an angular outline, and as having granular contents, but no nuclei. Now this absence of nucleus is of itself like a note of imperfection; but the idea thus suggested is more strongly conveyed by their whole appearance, their great irregularity of size, and especially the shrunken aspect of a large proportion of them. And that they are really nothing more nor less than imperfectly developed, or degenerating primary cells, such as in a well-elaborated blastema would have been granulation-cells, or plastic corpuscles, seems to us not only the view most in accordance with their obvious characters, but that also which is most in conformity with well-observed facts in the history of the cells developed in inflammatory exudations, and in the plasma effused for the repair of injuries, as well as with what has been noticed in the different parts of glands in which tubercular matter has been deposited. With respect to inflammatory exudations, and the phenomena of the healing process, we refer especially to the evidence, afforded by a study of them, that the same elements assume the form of the plastic corpuscle, the granulation- or the pus-cell, in obedience to external conditions, such as the qualities of the plasma surrounding them, the vitality of the tissues in which they are deposited, the access of air, &c. (See p. 424.) And in connexion with the probability hereby suggested, that the corpuscles of tubercle may be nothing more than abortive cells which have not been able to gain the type of the true plastic corpuscle, Mr. Dalrymple's account of his examination of a scrofulous cervical gland, given by Mr. Phillips, in his work on Scrofula, is particularly interesting. This excellent observer speaks of the proper corpuscles of the gland becoming more indistinct, and the exudation-corpuscles resembling lymph-globules, with which the congested parenchyma is infiltrated, becoming more numerous, but *irregular in size and shape, and interspersed with granular matter,* as the edges of the white deposit are approached.

Lastly, it is worth considering, that in the only instances known to us, in which the corpuscular elements of morbid products can *with certainty* be regarded as essentially related to the specific nature of the disease in which they occur,—viz. the different varieties of true carcinoma,—the cells are marked by characters, which at once and broadly distinguish them from all others; so that there is no room for attributing them to any mere imperfection or degradation of the ordinary plastic cells; from which we might expect to find in the cells of other diseased products, if of corresponding significance, characters no less distinct; and conversely, may doubt the specialty of any cells which do not exhibit analogous marks of distinctness of nature.

We are led, then, by the foregoing considerations, taken in connexion with each other, to dissent from the opinion of Dr. Madden, who unhesitatingly asserts, "that the presence of peculiar corpuscles constitutes the essential characteristic" of tuberculous matter, in whatever locality it may be found; and that the presence of these corpuscles "is the *uniform result* of the operation of one special cause." (pp. 20-1.) We think that such a conclusion would be subversive of another, in which we agree with Dr. Madden, but which rests upon different grounds; viz. that "the words scrofulous and tubercular [may be] used as interchangeable terms, as exponents of the same general fact or facts," and "that there is a perfect identity of nature in the affections to which these designations are applied." (p. 55.) And we infer rather, that though the appearances presented by the corpuscles found in tuberculous matter are sufficiently peculiar to entitle these bodies to be considered characteristic, *when they are present,* yet these external appearances do not imply such specialty in the corpuscles themselves, as to require them to be regarded in the light of essential elements; and that therefore their absence from a given deposit, in a subcutaneous gland or elsewhere, is not of itself a sufficient disproof of the essential oneness of nature of such deposit with others in which they may be present:—in other words, that they cannot be appealed to for a solution of the question of the identity of Scrofula or Tubercle.

Having shown that scrofulous tumefaction of the cervical glands is frequently associated with tubercle in the internal organs, and explained this fact in the way we have seen, M. Legrand quotes cases of *uncomplicated* glandular swelling, to show that the association is not constant. Such cases depend for whatever bearing they may have upon the question under discussion, entirely upon the truth and universality of the law announced by Louis, with respect to the existence of tubercle in the lungs whenever it is found in any other organ. It is now, however, generally acknowledged that, even with the modification as to age subsequently introduced into the terms of his original proposition by that eminent pathologist, that law was stated in far too absolute a manner, and that experience demands its yet further and considerable modification. In the work before us, M. Legrand cites a case (p. 21) of tuberculated mesenteric glands, with scrofulous subcutaneous glands in the neck, without any tubercles in the lungs, even though pleuropneumonia was present; the age of this patient is, however, not stated. At p. 57 he gives the case of a female, æt. 16, in which there was *tubercle* in the cerebellum, the mesentery, fallopian tubes, and uterus, with *scrofulous* caries in the bones of the hand, and yet the lungs free. Cases of tuberculated peritoneum in persons beyond the age of 15 are cited by Drs. Carswell, Watson, and other writers; and our own experience would supply additional and striking instances, up to the age of 35, in confirmation of this. If then tubercle, in the more special sense of the word, is ascertained to exist not unfrequently in some organ or organs, even in those who have passed their 15th year, without any similar deposit in the lungs, it need not affect our belief in the common nature of scrofula and tubercle, to find cases in which the tumid subcutaneous glands of the former are unaccompanied by any internal deposit of the latter. That such isolation of the morbid product should exist frequently in infancy and childhood, is no more than would be expected by those acquainted with the results of researches undertaken with a view of ascertaining the proportion of cases, during this period of life, in which tubercular affections of other parts of the system are uncomplicated by pulmonary deposit. For instance, Rilliet and Barthez state, as the result of their investigations, that out of 112 infants, subjects of tubercular disease, the lungs were free in 47; and M. Lombard found as large a proportion as 27 cases out of 100, in which the same immunity was exhibited by the lungs. It should be carefully borne in mind, too, in observations on the association of scrofulous cervical glands and pulmonary tubercle, that not every case of simply tumid gland is a case of scrofula; and that though the majority of such swellings do own a scrofulous parentage, yet if the affection do not proceed beyond the congestive stage, which, in by far the largest number of instances, precedes the deposition of scrofulous matter, and from which the glands may return to their usual condition, such cases should be taken no account of. And if both these descriptions of cervical glandular enlargements be duly eliminated, we apprehend that there will remain but a very moderate proportion, in which tubercle in some internal organ, and most especially in the lungs, will not sooner or later be found to have accompanied or speedily followed the external deposit. We know indeed, even now, but little of the conditions,—whether appertaining to age, sex, individual constitution, degree of general predisposition, functional activity of particular organs, or to circumstances of simply accidental nature,—by which the manifestation of the morbid diathesis may be determined to particular points in the economy; but it seems conformable to reason and to analogy to suppose that there may be, in some or other of the particulars just hinted at, sufficient to explain why the effects are sometimes limited to the lymphatic system, and their usual preference for the lungs overcome. Into this point, however, it is not necessary for our present purpose that we should enter. We will only just observe, with respect to the three cases brought forward by M. Legrand, in illustration of the independent occurrence of scrofulous enlargement of the cervical glands, that the first two (of the ages respectively of 65 and 56) appear to us to admit of great question as to whether they ought to be regarded as scrofulous at all. The previous good health of the patients, the rapid progress of the local affection to suppuration and afterwards to healing, and the character of due elaboration given to the pus, seem fully to justify this doubt: while the third case we think wears more of the appearance of one of cancer than of scrofula.

But, though no inference can fairly be drawn from the occurrence of scrofulous glandular swellings without internal tubercle, in favour of the essential diversity of these two affections, which cannot be easily set aside by arguments telling in the opposite direction, it may be expedient to inquire whether the determination of the question is otherwise affected by an observation of what happens in another form of scrofula, of which also M. Legrand gives examples, unaccompanied by tubercle—that of scrofulous caries.

Here it may be noticed that, just as he had previously done with respect to glandular enlargements, M. Legrand has preceded his accounts of uncomplicated scrofula in bone, by numerous cases of caries and white swelling; in which, though he persists in calling them non-tuberculous, internal tubercle made its appearance, and which he consequently describes as developed "under the influence of the tuberculous principle." The ground on which these cases are adjudged to be non-tuberculous is simply the absence of the corpuscle, which, as we have seen, he considers essentially characteristic of tubercular matter. It is hardly necessary for us to say that, in our view, this negative sign is not sufficient to remove these cases from the category to which everything else in their history would assign them. And when we add, that our author avows that by far the larger number of cases of disease in the bones and their annexed tissues, including both white swelling and caries, which were formerly considered scrofulous, are now rightly regarded as tuberculous, we shall not be thought too stringent, if we require a very rigorous proof that the small residuum differs essentially in nature from those with which it has been heretofore associated. The instances cited by M. Legrand of "caries essentielles dépendant sans doute de l'action délétère du principe scrofuleux, mais qu'on ne peut bien certainement rapporter à aucun principe tuberculeux," are five in number. Now the ages of two of these are not given; but these two would appear to have been the only adults out of the five, the remaining three cases having the ages respectively of 9, 16, and 11. To these last we might apply the observations offered above with respect to cases of uncomplicated cervical swellings occurring before that age, at which Louis' law becomes the general rule. And of the other two cases we might dispute the scrofulous nature of the first. But as death happened in all these cases, and, in the four unequivocal instances, appears to have resulted simply from the exhaustion and other effects of the scrofulous disease, which was extensive in its ravages, while one of them, a girl of nine years of age, exhibited every condition usually considered the most favorable to the development of tubercle in the lungs, we are not satisfied to pass them over without further remark. Such cases then as these,—where the scrofulous cachexia is so severe as to affect the osseous system at many points, and ultimately to prove fatal,—and more especially where, as in the instance particularly referred to, there is not only every mark of the scrofulous constitution, but post-mortem evidence (in the existence of pleural adhesions) of an inflammatory attack having occupied the immediate vicinity of the lungs,—and yet neither in the last or in the other cases is any tubercle found either in the lungs or elsewhere,—when contemplated alone, are calculated, we must allow, to raise a strong *primâ facie* presumption against the supposition of a common origin for both forms of disease. But do they not justify an equally strong presumption against the identity of the morbid principle or diathesis, which has here confined its ravages to the osseous system, though proceeding to fatal results, with that which (supposing it to be distinct in its nature from the tubercular diathesis) would seem to exercise ordinarily a selective preference for the subcutaneous glands of the cervical region? For, in the worst of the cases referred to, it is expressly stated that there was no glandular enlargement, even though in one of them the bones of the face, the left hand and foot, and the right scapula, thumb, and elbow, were softened, disorganized, and reduced to putrilage, with partial or entire destruction of the cartilages. And yet who, on the ground of such instances, is prepared to question the identity of origin and nature in scrofulous caries and scrofulous lymphatic glands? As was said above, we do not know what may determine the localization of the disease in particular cases; but it is very conceivable that when the diffused morbid tendency has once fastened upon a particular tissue, and that to any considerable extent, this very fact may have a tendency to determine its subsequent manifestations exclusively to that tissue, and thus may in some cases suffice to protect other and more important organs, even though they be the usually preferred seats of the morbid deposit, from invasion. There is an opinion popularly entertained that external deposits of scrofulous matter prevent the disease from "falling on the lungs;" and this common opinion, like most others, has much to justify it, especially the often observed fact of the rapid appearance of tubercular phthisis after the evacuation and healing of scrofulous abscesses in the neck, or the amputation of limbs affected with scrofulous caries or white swelling. And while these facts are among the strongest evidences in favour of the essential identity of scrofula and tubercle, they seem, in the way indicated above, to suggest an explanation of the exclusive manifestation of the effects commonly designated by the one or the other name. For, if this explanation suffices for cases of uncomplicated scrofula, it is equally good for the only remaining class we have to notice in this point of view, and to which M. Legrand draws attention, those namely in which internal tubercle exists without any external scrofulous affection. It is true that when, as in two of M. Legrand's cases, the deposit of tubercle is extensively diffused among the internal organs, yet without any external scrofulous affection, this explanation is inadequate; but then it is probable that in such instances there is *simultaneous* deposit in various organs, so that the principle implied in it cannot come into play; and the selection of the organs affected may be, as it often evidently is, susceptible of explanation by reference to some of the considerations we have before indicated. At the least we may safely affirm that, under the supposition of the identity of scrofula and tubercle, such cases present no greater difficulties than are presented upon either theory by cases like that we have already cited from M. Legrand's memoir, in which there was tubercle in the cerebellum, mesentery, fallopian tubes, and uterus, and yet none in the lungs; or like one he quotes at p. 116, where, in a patient aged 55, tubercle existed in the *bronchial glands*, pericardium, pancreas, and peritoneum, but without a trace in the lungs; or like that of an adult which came under our own observation, presenting the most extraordinary amount of tubercles we recollect to have ever witnessed in the peritoneum, liver, pleura, and pericardium, yet with the lungs perfectly free.

We may now leave our readers to judge for themselves of the inference drawn by M. Legrand from all the cases, of which we have analysed for them the leading features, and from the observations founded upon them, which we have made the subject of our remarks, only premising that M. Legrand's notions of a rigid proof differ very widely from our own.

> "From all that precedes, are we not already entitled to draw the rigorous deduction that *tubercle* and *scrofula*, though manifesting themselves by symptoms which are sometimes analogous, are two morbid principles essentially distinct; since we never recognise in the bodies of those who have sunk under scrofulous disease those pathological lesions which are always met with, on the contrary, in those who have fallen victims to tubercle?" (p. 105.)

The sequel of M. Legrand's work is devoted to an account of the physical and chemical characters, the seat and causes of tubercle; next, to a similar account of scrofula; and lastly, to a comparative examination of the two forms of disease, under the following aspects: the manner in which they are respectively influenced by the exanthemata and pregnancy, their course and termination, the respective liabilities of the scrofulous and tuberculous to other diseases, and the states of the blood and urine in each. In much of this we need not follow him; some particulars, indeed, we have anticipated. But we shall endeavour to extract, as the subject for further remark, such points as are made use of by M. Legrand in support of the general conclusion at which he arrives, and which are formally enumerated at the end of the work, respecting the essential marks of difference between the two forms of disease. And the first thing of interest that strikes us, is the account given, severally under the heads of seat of tubercle and of scrofula, of what is regarded as the distinct manifestation of these two principles in the osseous system. Tubercle in bones, it is said, is not to be confounded with scrofulous caries; and then follows an extract from the "Leçons orales" of M. Bérard, published in the 'Gaz. des Hôp.,' giving a minute description of five different forms of the true tubercular affections of bones, which, though interesting as descriptions, our space will not allow us to transcribe. The chief point, with relation to our present object, is that in all these forms the deposition of tubercular matter commences in the interior of the osseous tissue; either in small spherical masses, which become encysted, or in granulations infiltrated throughout the areolæ of the spongy tissue; and that in its subsequent progress it spreads from within outwards. It is in such forms of true tubercular disease that Pott's disease consists; and rachitis, when the term is confined to a disease of the spinal column, is also an exemplification of one of these forms (that of tubercular infiltration), or of the mere influence of the tuberculous principle, "qui entache l'économie, et porte alors son influence désorganisatrice sur le système osseux,"—by which we understand a reference to cases in which, from the coexistence of unequivocal tuberculosis elsewhere, the spinal curvature is to be considered as a tuberculous affection, albeit the diagnostic, to which M. Legrand's faith in the tubercular corpuscle should confine him, be absent. These forms of disease are, moreover, asserted to be *always* accompanied by tubercle elsewhere, and chiefly in the lungs; and cases are given which show that they certainly are so *sometimes*. Turning to the description of "scrofula of the bones" (at p. 258), we find it stated that the disease makes its first appearance always, or nearly so, in the periosteum, and its ravages are then always from the exterior to the interior; a circumstance to which much importance is attached as a characteristic point of difference between it and tubercle, and which is, indeed, referred to as an illustration of a law, applying to the whole subject, and stated at the conclusion of the work in the following terms:

> "*Tubercle* has its principal, almost its only source, in the internal organs; and its external manifestations only proceed from the *irradiation* of the morbid principle from the centre to the circumference.
>
> "Scrofula, on the other hand, extends itself on precisely an opposite plan; it manifests itself in the skin and periosteum, and then radiates towards the internal organisms, which, however, are never disorganized by it after the same fashion as by tubercle." (pp. 400-1.)

We do not find any attempt to establish by proofs this "irradiation" of scrofula towards the interior organs, unless the occurrence of *tubercle* in internal organs, consecutively to the manifestation of scrofula in the skin, subcutaneous glands or bones, be regarded in this light. But this would be allowing the convertibility of scrofula into tubercle; which is very much like an acknowledgment of their identity. As for the contrast drawn between the progress of the two affections within the limits of the organs in which they may be severally developed, we confess that we are wholly at a loss to see how this can be drawn into the light of a character or evidence of essential difference. For taking the facts as given by M. Legrand with respect to bones, and assuming that in caries the disease begins in the periosteum, it is clear that, whether scrofulous or tuberculous, there is but one direction in which its subsequent extension in the bony tissue (except laterally, which is not excluded) could by possibility take place, viz. from the exterior to the interior. And it is no less clear that if a morbid deposit occupy, in the first place, the central cavity of a bone, its subsequent extension in the bony tissue must be in the contrary direction, or, if infiltrated at first through portions of the cancellous structure, that the subsequent additions to this infiltration must also bring the diseased product nearer to the surface.

Is it not obvious, then, that this supposed characteristic "irradiation" of the two affections merges altogether in differences of original seat and form,—the direction being from without inwards, in the one case, *because* the disease commences in the periosteum and surface of the bones, and from within outwards, in the other, *because* the interior of the bones is first affected? And as the direction, so too may other noticeable points of difference receive explanation from this consideration of seat; especially the absence, in the case of caries, of that material which is held to be characteristic of tubercle. For as diseased action extends in these cases from the bones to the soft parts intervening between them and the surface (so far absolutely contradicting the law of M. Legrand), and a low inflammation with ulceration and suppuration is set up, even if matter containing the tubercular corpuscle were originally deposited, there is no doubt it would become so far altered, in consequence of these surrounding changes, as no longer to be capable of recognition. And thus the mere absence of the microscopic tubercular element from the discharge of a sinus leading to a carious bone, is no manner of evidence on one side or the other. But it is more probable that such matter is not effused in these cases; and here again the difference of seat seems a sufficient reason for its absence; the density of tissue in the periosteum and external surface of the bones interposing an obstacle to its deposition, while in the loose spongy tissue of their interior, just as in the parenchyma of the lungs or other organs, there is every facility for its accumulation. There appears to us, then, no ground whatever for the hypothesis of a specific centripetal and centrifugal tendency, severally characterising a correspondingly distinct morbid principle; and there is no more reason, on the score of the local appearances, for dissociating tubercular disease in the cancellous structure from superficial caries of the bones, than there is for separating scrofulous infiltration of the subcutaneous glands from those ulcerations of the skin which are believed to own an identity of parentage with them.

We pass over the observations of M. Legrand on the influence of the exanthemata upon scrofula and tubercle, as containing nothing that ingenuity can interpret into a passable evidence of essential difference between them. That measles and cowpock have, each of them, a perhaps somewhat beneficial influence on scrofula, we certainly do not think our readers will expect us to be at the trouble of disputing. The generally prejudicial effect of these, in common with the other exanthems, upon internal tubercle, is acquiesced in by our author, and exalted into a point of distinction. Another distinction he derives from the comparative influence of pregnancy upon the two affections. Scrofula he asserts to be often beneficially influenced by this condition; but he cites no proof of this beyond a case which appears in an earlier part of the work, and which seems to us a strange foundation—independently of the inadequacy of any single or of a few cases—for such an inference. The case in point, which is given at p. 156, is one of scrofulous caries of three vertebræ, proving fatal, in a woman aged about 50, by consecutive effects within the vertebral canal; the woman having borne several children, though there was evidence, in the state of parts, that the curvature must have commenced early in life. Upon the development of tubercle, on the other hand, M. Legrand agrees with M. Rostan in thinking, in opposition to the popular opinion, that pregnancy exerts a decidedly disastrous and accelerating influence. M. Louis, as our readers are aware, had previously expressed himself doubtfully upon the point; but the cases to which he refers, collected by Andral, show clearly enough that it is one not to be decided by even a considerable number of instances, in which the event may appear to favour either side of the question. The two or three cases cited by M. Legrand can only be taken as a contribution towards its settlement, and by no means as a sufficient justification of the very positive statement he makes, still less for the erecting this into a sign of essential difference between scrofula and tubercle. Supposing it to be hereafter shown, that in the large majority of instances, tubercle, even in its early stages, is materially accelerated by pregnancy, the fact that many instances can be adduced, in which no such result has followed, would be sufficient to deprive the former circumstance of all diagnostic value; especially when it is further taken into consideration that the scrofulous forms of disease are most common at a period of life antecedent to the age at which pregnancy becomes most frequent.

M. Legrand compares the course and termination of the two affections, and asserts that there is nothing in the history of scrofulous cases corresponding to the latent form of tubercle; which, excepting so far as the latency of the latter depends upon the seat of the deposit being wholly out of sight, we take upon ourselves to deny. Another assertion, that there is no acute scrofula, as a correspondent to galloping consumption, we must meet in the same manner; with a caveat only, founded on the widely different position of subcutaneous lymphatic glands and the lungs in the scale of vital importance, as to the difference in the constitutional or general state and in the ultimate result. And this remark applies to another of M. Legrand's notes of distinction, viz. that death is the rule in tubercle, the exception in scrofula. It cannot be necessary to point out to how great an extent this is dependent upon seat. A deposit of strumous matter in a gland or a chain of glands may take place, may excite those actions which lead to softening and evacuation, and the local disease may thus be got rid of without much affection of the general health. The matter is one of not quite so easy attainment when bone is affected; and scrofula here is sometimes fatal. How much less likely is it to be achieved when an organ, having such a direct and momentous relation to the life of the whole body as the lung, becomes the seat of deposit. And yet even here we find evidences—evidences sufficient to satisfy M. Legrand*—that a similar result occasionally, and perhaps more frequently than is commonly supposed, is achieved. Not that we deny—on the contrary, we are quite prepared to admit—that there is probably a marked difference of *degree* in the constitutional affection or taint, which shows itself respectively in tubercle of the internal organs, and in scrofula upon the periphery. Nor does it seem at all unreasonable to suppose, that such mere difference of degree in the impregnation of different systems with the morbid principle, may have much to do with the determination of the effects to one set of parts rather than another, and thus afford a solution of many apparently fundamental points of difference between tubercle and scrofula.

But to return to M. Legrand's distinctive features:—He asserts that the tuberculous are little subject to skin diseases, while the scrofulous are eminently so. Is he not likely here to be mistaking effect for cause? There is no question that, quite apart from scrofulous and tuberculous influences, the proneness of individuals to common affections of the skin varies widely, just as to common disorders of other tissues and organs; and there is as little doubt that such proclivity to cutaneous disorder would determine a dormant scrofulous diathesis to manifest itself in those forms of eruption and superficial ulceration, which are generally enumerated amongst its outward effects. It should be recollected, also, that the period of life during which the skin is most susceptible, is also that at which scrofulous affections are most frequent. And, so far as the alleged liability is true, we think it may be thus explained. At the same time we must except against the marked contrast drawn by M. Legrand; for it is not only far from uncommon to find a beautiful skin, unblemished by any eruptive disease, in children and young persons exhibiting some of the recognised effects of scrofula; but the reverse also may be frequently seen in the victims of tubercle. A difference in *general* liability to disease, and to be severely affected by it, is also said by M. Legrand to characterise the two classes of individuals: the scrofulous enjoying a marked immunity in these respects, as compared with the tuberculous. Now as this comparison can only be made between those in whom the diathesis has already manifested itself,—for we do not suspect M. Legrand himself would venture to assign characteristics by which, while yet dormant, the tuberculous could be distinguished from the scrofulous diathesis,—this difference, so far as it really exists, would resolve itself into such self-evident facts as—that lungs which are already the seat of a morbid deposit are more likely to become affected with pneumonia or bronchitis than those previously sound; and that these affections would, under such circumstances, be worse, and less likely to be recovered from than under the opposite conditions;—or that fevers would be likely to leave worse effects behind them, or to proceed more frequently to a fatal issue, in those whose vital organs were already in a damaged condition, than in those who had only some of their lymphatic glands in a state of disease. To one of M. Legrand's statements in illustration of this asserted difference, we can only oppose our very positive conviction of the contrary,—a conviction which, we doubt not, will be shared by all those of our readers who have kept themselves *au niveau* with the progress of our knowledge, as respects the relations of the disease referred to. This statement is, that albuminuria, which is a frequent complication of tubercle, is never met with in the scrofulous. Without doubt, the association of one form of Bright's kidney with tubercular phthisis is well known; but we are sure we have seen the same form of disease in scrofulous subjects who presented no development of tubercle whatever. We do not think it necessary to notice any other of the distinctions which M. Legrand seeks to derive from the respective complications of scrofulous and tuberculous affections; but shall bring this part of our subject to a close with a few words respecting the changes described by him in the blood and urine.

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* Vide cases quoted by him at pp 384 et seq.

The alterations observed in these fluids in both forms of disease are, indeed, spoken of as secondary only; yet the two forms are contrasted in respect of these alterations. Both tubercle and scrofula agree in showing increased fluidity of the blood; and there are also observed in both, diminution in the amount of the red corpuscles, together with certain alterations in their figure and structure, diminution in the proportion of the solid elements of the serum, and an increase in the entire quantity of water. The albumen of the serum appears sometimes above, sometimes below, the average amount, in tubercle; and, according to the observations of M. F. Dubois, quoted by M. Legrand, is more uniformly, though only to a very trifling extent, depreciated in scrofula. To this we cannot attach the least importance, though M. Legrand appears inclined to do so; especially as the statement is not supported by other analyses. The foregoing results are, however, in the main, confirmed by a comparison of the best analyses of which we are in possession. But there is further observed a difference in the mean relative proportion of fibrine in scrofulous and tuberculous blood; in the former it is diminished, in the latter increased; and this is the point to which M. Legrand draws attention, as illustrating an essential distinction between the two diseases. He speaks of the depression of the fibrine in scrofula as showing that this disease "annihilates the inflammatory element," and as explaining the rare occurrence of inflammatory complications in scrofula; while, on the other hand, tubercle, notwithstanding its general impoverishing effects upon the blood, does not, at least in its early stages, destroy the inflammatory element, and, accordingly, inflammatory complications are here both frequent and pernicious. Let us see how far these statements are borne out by the facts. That there is a marked elevation in the proportion of fibrine in phthisical blood, undoubtedly comes out, as the *mean* result of all analyses. Yet when we find two out of the twenty-one cases examined by MM. Andral and Gavarret, showing a lower amount of this element than the physiological mean established by their researches (2·10 per 1000 in one case, and 2·90 falling, at the second observation, to 2·70 in another), there may be some doubt whether the observed augmentation is an *essential* characteristic of the disease. On the other hand, in a table extracted by M. Legrand from the 'Gaz. des Hôpit.,' and there copied from an English journal, which exhibits the results of twelve analyses of the blood of *scrofulous* patients by a Dr. Nicholson, we find that though the proportion of fibrine falls as low, in one case, as 1·20 per 1000, yet in two instances it is at the physiological mean (3). And, on further consulting Dr. Glover's analyses of the blood drawn from patients affected by external scrofula,—which were conducted on the method employed by Andral and Gavarret,—we are struck with the fact that in the cases of eleven males, the fibrine reached a *mean* of 3·132, and in six females, a mean of 3·585. Now does not a candid consideration of these facts, taken in conjunction with the well-known and easily explained occurrence of bronchitis, and of circumscribed inflammation around the tubercular deposits, in phthisis, tend to this inference;—that both forms of disorder are *essentially* characterised by an impoverishment of the blood—by a spanæmia, which includes the fibrine as well as all the other solid constituents of this fluid, until some intercurrent inflammation, so much more prone to occur in a vital organ than in the seats of external scrofula, exerting its characteristic effect, raises the proportion of fibrine up to and above the normal standard? That the spanæmia in which the two orders of affection agree, is the only *essential* change in the blood, and that the fluctuating proportions of fibrine in which they differ, are contingent upon accidental circumstances, is further confirmed by the fact, that in the pneumonia and bronchitis of phthisical patients, the fibrine of the blood does not attain so high a figure as in cases happening to those who were previously in good health, and that when inflammation attacks a patient affected by scrofula, this element undergoes an augmentation which, allowing for its previous depression, is equally proportioned to the seat and severity of the attack. It should be remembered also that the fibrine effused in the inflammations both of scrofulous and tuberculous subjects, manifests a similarly defective elaboration and plasticity. For ourselves, we cannot see any reasons for attributing an essential defibrinating influence to scrofula, which do not equally apply to tubercle; there are no facts within our knowledge which go to show any differences in the amount of fibrine in the blood of the scrofulous, and of those having tubercles *in a quiescent condition;* and the differences, in this respect, which afterwards make their appearance, seem susceptible of an easy and satisfactory explanation, independently of any real or supposed difference in the essential tendencies of the two affections.

We need scarcely prolong this discussion by dwelling upon the indications presented by the urine. M. Legrand allows that the changes which occur in this fluid are altogether secondary, and chiefly expressive of those complications, inflammatory and febrile, and of that general waste, which attend tubercular disease in its progress, or of the anæmic state which exists both in the scrofulous and tuberculous. The only particular to which he points special attention, is the frequent presence of albumen in the urine of scrofulous patients, and its "very rare occurrence in the tuberculous" (p. 398): a circumstance which in his view explains the almost constant diminution of albumen in scrofulous blood, and the frequent occurrence of dropsy in that form of disease, and the rarity of both these states in the tuberculous. But the grounds on which he considers this albuminous condition of the urine to be so common in scrofula, appear very defective. Reference is only made to the analyses of Becquerel, who speaks of the presence of albumen in the urine of the scrofulous as merely an accidental circumstance. The uniform deficiency of albumen in the blood, on the other hand, we have already stated, was asserted on no less questionable data. We have also had occasion to dispute M. Legrand's former assertions,—strangely at variance, as they seem to us, with what he here says of the urinary changes in scrofula and tubercle,—respecting the association of albuminuria with these affections. We have no doubt that albuminuria occurs as a complication both of the external forms of scrofula and of internal tubercle; or, we should rather say, that it is itself, in one of its forms, a manifestation, like tubercle in the lungs or caseous deposit in the cervical glands, of one and the same diathesis. And, doubtless, whenever it occurs with either one or the other of these affections, it will be likely to be attended with a diminution of albumen in the blood-serum. But as to any such regular and characteristic occurrence of correlative changes in the blood and urine as M. Legrand assigns respectively to scrofula and tubercle, we cannot, upon such evidence as he adduces, accept them as by any means proved, or even rendered probable.

We have now brought under the consideration of our readers all those marks of difference between scrofula and tubercle, which M. Legrand puts forward as of essential significance. And in examining them, one by one, with reference to the hypothesis they are supposed to illustrate, we have shown, as we hope, how little weight they are entitled to, as evidences of generic diversity; so that whether we investigate the structural characters of the matters deposited in scrofula and tubercle,—or take into consideration the occurrence of glandular enlargement without internal tubercle, or of internal tubercle unattended by glandular enlargement,—or compare the history of scrofulous affections of the bones with those ascribed distinctively to tubercle,—or examine the reactions of scrofula and tubercle upon other diseases, or of other diseases and of the state of pregnancy upon them,—or inquire into the differences which mark their progress and termination respectively,—or into the morbid liabilities of the scrofulous and tuberculous,—or, lastly, take cognisance of the changes wrought in the blood and urine of those who are affected with these disorders,—we find nothing that is not, to say the least of it, equally explicable on the opposite theory of generic identity. And we fall back upon this theory, as the only one which is adequate to explain such remarkable facts as are recorded by M. Legrand himself, and by which the most striking analogy is shown to exist between the two series of phenomena;—such facts as their occurrence in the same habit of body; their being preceded by a cachexia in no respect distinguishable for the one and for the other; their coexistence in the same individual; the occurrence of one series in certain individuals of a family, of the other in other members of the same family; their common transmissibility, including that of the cachexia or diathesis; the apparently indifferent development of either amongst the offspring of parents, one or both of whom may have manifested one only of the two forms of affection. Facts like these, some of which are strikingly illustrated by histories reported in M. Legrand's work (as e. g. by cases 52 and 53), and of which we are persuaded the personal experience of all our practical readers will have furnished them with instances, appear to us to demand, that far more substantial arguments than have hitherto been brought forward, whether of statistics or of logic, shall be adduced, before the common nature and origin of tubercle and scrofula, to which they so strongly and, in our apprehension, so conclusively point, may be considered as disproved. A belief in this generic identity we conceive to be still the prevailing one amongst our countrymen; nor should we, even at the instigation of a work honoured by the approbation of the French Academy of Medicine, have been disposed to give so much space to an examination of the arguments brought against it in that production, had we not been mindful of the powerful influence likely to have been derived to the contrary belief by the statements of Mr. Phillips, whose work, as a whole, deserves to be regarded as the most complete and valuable monograph on the subject of Scrofula in our own, or probably in any other, language. The arguments of this able writer were principally statistical, and were examined, and we think satisfactorily disposed of, in the pages of one of our predecessors; they are also met by very pertinent objections in the little work of Dr. Madden which stands at the head of this article; and we have no intention of alluding to them any further. But believing as we do, that the pathology of tubercular disease cannot be advantageously investigated, apart from that comprehensive view of its phenomena which depends upon an admission of its essential identity with scrofula, we have thought it desirable to defend this position from the more recent attacks upon it contained in M. Legrand's pages.

ART. XIV.

1. *Obstetrics: the Science and the Art.* By CHARLES D. MEIGS, M.D., Professor of Midwifery in the Jefferson Medical College at Philadelphia, &c. &c. With 121 Illustrations on Wood.—*Philadelphia,* 1849. 8vo, pp. 686.
2. *Parturition, and the Principles and Practice of Obstetrics.* By W. TYLER SMITH, M.D. Lond., Lecturer on Obstetrics in the Hunterian School of Medicine.—*London,* 1849. Fcap. 8vo, pp. 396.

IN continuation of our review of these works, we have to notice particularly the more practical subjects which are discussed in them. And in doing so, we shall chiefly be engaged with Dr. Meigs; not only on account of his acknowledged experience as an obstetric practitioner, but also because the scope and construction of his treatise require it. We have already devoted much space to a critical analysis of Dr. T. Smith's subordinate to the physiological, we shall not at any length allude to it.

Dr. Meigs has illustrated many parts of his work with diagrams, most of them simple outlines like M. Chailly's; and in general they are well selected and instructive. In connexion with the planes of the pelvis, he has added four useful outline figures from Wigand, showing different inclinations of the superior strait of the pelvis. His side view of the pelvis has an anatomical error, which we have seen in several other works: the anterior lip of the os uteri is made shorter than the posterior lip; this should be reversed. We doubt, too, the propriety of representing the vagina as an open cylindrical canal, because in truth, during life, the two walls closely approximate, and form an important column of support to the uterus. To watch through the aperture of a speculum the way in which the vagina contracts as the instrument is withdrawn, affords a sufficient demonstration of this fact.

The mechanism of the different cranial positions, of which Dr. Meigs enumerates six, is divided, after the manner of the French, into a series of movements of the fœtal head, viz. flexion, rotation, extension, and restitution. This is a clear way of describing the passage of the head in childbirth, and one which is readily appreciated by students. In the fourth position, where the occiput is directed to the right sacro-iliac synchondrosis, Dr. Meigs accords with Professor Naegele, and all well-informed accoucheurs, that the occiput moves round to the right acetabulum spontaneously; although this movement of internal rotation sometimes requires, according to Dr. Meigs, to be aided by the accoucheur's hand, or even by an instrument. The way in which the child is born when this movement is not accomplished, is shown by a diagram; and we confess in our own experience, that it is far better to leave the selection of the two to nature, than to make any attempts to rectify it. We doubt very much whether the hand alone can ever accomplish it; and it may well be questioned whether the movement, in cases where this kind of assistance has been given, would not have been accomplished without it. It is, we believe, a judicious practice, if instruments are employed at all, to be content with supplying the force, in accordance, of course, with the proper axes of the pelvis, and to leave the way in which the child's head will pass out entirely to nature. If the large movement of rotation can be made, it will take place of itself; and if it is not spontaneously effected, it is better to deliver with the forehead first. Our own experience, at least, has led us to this conclusion. Dr. Meigs agrees with Dr. Naegele and others, in considering their fourth position as the next in frequency to the first, viz. where the occiput is forward, and to the left. He states this as the result of his registry of presentations and general clinical experience, but without giving the relative number.

Abortion. The chapter on Abortion in Dr. Meigs's work is short and defective, and much in contrast with the elaborate lectures on this subject by Dr. Tyler Smith. The influence of the excito-motor power in conveying impressions from different parts of the system, through the spinal cord to the uterus, and inducing abortion, is here well illustrated. Without recognising, says Dr. T. Smith, the paramount influence of the nervous system in the causation of abortion, we deal only with surface physiology. Hence he has simplified the large and diversified subject of the

causes of abortion, by ranging them under two hea
causes which act by irritating the extremities of th
2, the centric causes in which the spinal centre is n

Among the *excentric causes* we find irritation of tl
vesical, ovarian, rectal, vaginal, and uterine nerves ;
catalogue we find no more than what are usually incl
causes of abortion, yet Dr. Smith's mode of describi
illustrations of the excito-motor theory, is novel and

Among the *centric* causes are blood-poisons, the e
scrofula, mercurialization, carbonic acid, specific u
emotion.

We do not propose to follow Dr. Smith into hi
operation of these various causes ; but we notice tha
cursory manner of the diseases of the os and cerv
indeed, at their having been forced into a too pr
though they were the main and almost the exclusiv
Dr. Meigs does not even mention them ; and in spe
ment in the prevention of abortion, Dr. T. Smith o
sufficiently simple, consisting of local abstraction of b
cauterization of the diseased sites ; every possible car
vent local and constitutional disturbance following up
Indeed, if we were to judge of the relative import
causes of abortion by the care and pains with which
we should say that Dr. T. Smith attributed greater i
tion of the mammary and trifacial nerves, than to the
which is perhaps to be accounted for from their affordi
his physiological views of the synergic relation betwee
Perhaps the rarest cause of abortion is irritation of
and about the next is mammary irritation. And ye
shown a good deal of forecast in the following stateme

> "I ought to state, that while I do not doubt the frequen
> matory disorder, and its consequences in the os and cervix u
> the organ has so recently become the subject of common vis
> I do not think the variations of colour, size, hardness, and th
> tion which may be consistent with moderate health in differe
> as yet satisfactorily made out ; and thus many cases may b
> which are not truly and decidedly morbid ; and in some cases
> has been positively excited by the treatment pursued to preve

Our own experience very much coincides with this ;
Dr. T. Smith ought to have shown far greater cautio
this opinion of his, than by recommending, in a loose a
" the local abstraction of blood, and occasional cauteriz
way of treating these cases. We believe that a soft
around the os uteri produces very little irritation inde
left alone, would rarely, if ever, cause abortion. If, h
tissues of the canal of the cervix are inflamed, then f
is occasioned ; but then it must be noticed, that as lon
pregnancy very rarely occurs. We believe we are enu
fact when we say that such a condition of the os and
induration or ulceration (as a simple granulating s
called, and familiarly spoken of to patients), as does n

will rarely cause abortion; provided always, that the disease is not mixed up with some syphilitic or cancerous taint. The great danger is in over-treatment.

We cannot but observe, too, that syphilis, as a cause of abortion, has not received from either author a sufficient degree of attention. It is of little practical value to speak of such a disease as syphilis, in general terms, as causing abortion; and our present knowledge of the disease, in this relation, requires a more minute description of it. Every one understands that if a woman, having had a primary sore, retains so much of the poison as to exhibit any of the recognised forms of secondary or tertiary signs, her offspring will be either killed by it in utero, or born with marks of it. This is an elementary fact. But supposing a man to have been cured of primary sores, and have only a very slight appearance of secondary symptoms,—say a little discoloured spot or two on the inside of the hand or edge of the scalp,—can he transmit the poison to the ovum, through the semen, without the woman having any known marks of syphilis; and can the taint then remain so long, as to cause a succession of abortions? Again, supposing the husband to be cured of his secondary symptoms, can he still retain so much of the poison as shall infect the ovum, though no trace of it shall be apparent on him or his wife; and may the death and abortion of the early fœtus ensue? We might refine on this inquiry, and still not overstep the limits within which this poison may be said to range in destroying the early ovum. The character of the ovum, under these circumstances, is an important part of intra-uterine pathology. There is oftentimes a well-marked change on the surface of the placenta; thick white patches, which we believe to be a primary disease of the placental decidua, and particularly of the cells of which it is composed. The ovum undergoes a slow death; the fœtus, at an early period, is frequently wanting when the amnial sac is opened; it appears to have broken up, and disappeared by liquefaction. The preventive treatment of abortion under the more disguised states, is full of clinical interest. Ought one or both parents to undergo mercurialization? and how long ought they to be separated, and pregnancy prevented? We cannot, of course, do more than hint at this inquiry; but we feel that our authors have virtually omitted one of the most widely diffused causes of abortion, in not having critically and at length examined this subject.

The use of instrumental aid in withdrawing the ovum is referred to by Dr. Meigs, who speaks favorably of Dr. Dewees's hook and of Dr. Henry Bond's forceps. He wisely enough limits their application in the following remarks:

> "While I lay before the student these instruments for the extraction of the dead ovum, I ought to warn him against too facile a disposition as to the employment of them, and to assure him they will often disappoint his expectations, and sometimes, where they do succeed, lead to evil consequences as to the mother. The ovum, in abortions, inhabits the body and fundus uteri. The cervix stands guardian as the *facultas retentrix* over the deposit, and reluctantly yields it a passage. In doing so, the conical neck of the womb must become a cylindrical canal, into which the fundus and corpus uteri thrust their intolerable burden. When this cylindrical canal hath received into its calibre a small ovum, or the remains of one, it has, of itself, little or no power of expulsion, but merely grasps the ovum and holds it fast. It holds it sometimes for many days. I have found it to hold the ovum in this manner for many consecutive days, because the very os uteri would not let it

escape, refusing to yield, chiefly, perhaps, because no dilati
In the long run it yields, the os tincæ becomes wide ope
down effort, a fit of coughing, or straining at stool or urine
vagina. Now, until the canal has become truly cylindrical,
Dr. Dewees's hook are not to be employed without much ca
the most part it is better to wait until all is prepared, and
with the index-finger." (pp. 215-16.)

In those tedious and dangerous cases of aborti
membranes lacerate early and the decidua peels off
fetid discharges and recurring hemorrhages, we ha
tepid water thrown into the uterus very useful, not
cavity of the womb itself from the decomposing matt
source of disease, but also in favouring the separation
collapsed membranes. Cases of this kind extend ov
or more, before the uterus is cleared; and the patie
bilitated. Hooks or forceps are of no use in such in

Conduct of labour. Under this title Dr. Meigs
hensive summary of what may be required of the ac
natural but in many cases of difficult labour. "The
he says, "might refer to any, and so to all possible
for midwifery after all is but the conduct of labours."
in separate paragraphs gives the general rules and pre
ment of labour,—such as attention to the bowels, p
the support of the perineum, &c. &c.,—but he consid
in cases of obliquity of the uterus, the effects of a "bad
pubis," and the management of the placenta when r
There is of necessity little that is novel in these matt
the sound and judicious principles which have guide
should be here adopted: but we willingly acknowl
style in their description, which is in general free fro
conceits, and retains all his raciness and apt illustratio
way with him, when speaking of the most eleme
fixes the attention and adds much value to the wo
students; indeed it is obviously the result of much
with the art of teaching.

There is only one subject contained in this ch
necessary to allude to, and that is the use of eth
midwifery.

Anæsthesia in midwifery. It is now more than
since (January 1847) Professor Simpson, of Edin
the vapour of ether in a case of delivery by turning
deformed; and the result of this was, that the
without the woman feeling pain, without impairing
of the contractions of the womb, or the consentane
abdominal muscles, either during the labour or after
to the child. Before the year was out, Dr. Simpson
of this mode of saving women the pain of labour
stetric practitioners to employ it; its use was not
instrumental cases, but it was exhibited in natur
series of experiments on other anæsthetic agents,
with the greatest perseverance and at some person

discovered that chloroform was a more available, and in most, if not all respects, a more valuable, agent for the induction of anæsthesia. Indeed the superiority of chloroform over sulphuric ether was found in practice to be so very great, that the latter was abandoned; and since then the vapour of chloroform has been the sole agent used throughout the scientific world, both in the practice of surgery and in obstetrics. The increased amount of benefit derived from it in addition to that above stated, as the result of Dr. Simpson's additional experience, may in general terms be stated to be, that its influence may be kept up for hours—six, eight, or more,—without any harm; that the minds of women are relieved from the distressing forebodings connected with labour; that the soft parts are relaxed; and further, that the chances of a rapid and more healthy convalescence are increased. With all this alleged good there is little or no evil, none at least but what a careful administration of well-prepared chloroform may be expected to avert. Whatever our opinions may be on the use of anæsthetics in midwifery, we cordially offer our congratulations to Dr. Simpson on his discovery of chloroform, and our admiration of the enterprise and energy with which he has confirmed and extended the use of anæsthesia in painful diseases and operations; and we have, too, the pleasing reserve, that this great boon to the human race is inseparably associated with an obstetric physician.

Notwithstanding, however, the good report of chloroform in midwifery, our two authors are decidedly opposed to it. Dr. T. Smith condemns it, without, however, giving any reasons in detail why he does so.

> "A year ago, and all women were promised safe and perfect exemption from the pains of natural labour. Now, however, the anæsthetic Elixir proves to be but a cup of Circe, instead of the true birth-help. At the present time, when the febrile excitement of the medical mind upon this subject is rapidly passing away under the influence of the numerous fatal cases which have occurred from the use of anæsthetic agents, there seems a more than ordinary necessity for dwelling upon the manifold wisdom with which the act of parturition is surrounded. Woman will certainly derive truer comfort, and a greater measure of safety and freedom from unnecessary suffering, from physiology, than from a wild therapeutics, which, in her hour of trial, only offers a choice betwixt poison and pain." (p. 168.)

This is severe but undiscriminating; and we cannot but remark that the talk about a "wild therapeutics" comes with a bad grace from the advocate of catheterization of the fallopian tubes. Dr. Meigs, however, has considered this subject far more at length, under the title of etherization, and in answer also to a letter from Dr. Simpson, which is given. He does not "deny that there may be instances of severe suffering for women in labour, that ought to be mitigated or even wholly obviated by casting the woman into the profound anæsthesia of chloroformization. But what he desires to say is this;—that having carefully studied the reports upon etherization and chloroformization, whether of America or of Europe, he yet remains unconvinced, either of the necessity of the method or of its propriety." His first argument against its necessity is derived from the average duration of labour, which is computed to be four hours; and taking the number of labour-pains at about fifty, and their average length at about thirty seconds, the woman really suffers about fifteen or sixteen minutes. "I should find," says Dr. Meigs, "the objection to it less, and the inducement greater, were the fifteen minutes of pain to be always fifteen consecutive

minutes. When they are distributed through two
minutes, or four hours, I look upon the exhibition
uncalled for." Dr. Meigs thinks, too, that the har
nected with childbirth has been greatly exaggerated
anæsthesia; and that the lying-in room is more a s
and gaiety, than of the shrieks, and anguish, and desp
so forcibly portrayed. The hazards, too, of anæsth
great. He fears, from the result of experiments, i
medulla oblongata; and he says: "Many and but too m
power of these tremendous agents to overthrow, al
force resident in the medulla oblongata, are spread
medicine for the last two years."

The arguments which our author furnishes of
anæsthesia, we give in his own forcible words:

> "I cannot avoid the feeling of astonishment which seizes
> the details of cases of midwifery that have been treated
> *Drunkenness* of etherization. To be insensible from whisky,
> and wine, and beer, and ether, and chloroform, is to be what
> dead-drunk. No reasoning—no argumentation is strong en
> ninth part of a hair's discrimination between them—excep
> one of the agents or its diffusibility as a stimulant narcoti
> produce its intoxicating effect, which is sooner recovered fro
>
> "I have by no means said what I am inclined to say as to
> any processes that the physician sets up, to contravene the ope
> and physiological forces that the Divinity has ordained us
> The question is often propounded as to the Beneficence that
> sorrow and pain of them that travail in childbirth. It ought
> without any the least disposition to what is called *canting,*
> connexion exists betwixt the power and the pain of labours.
> may assume the privilege to control, check, and diminish the
> ever they become so great as to be properly deemed pathol
> have the professional right, in order to prevent or obviate t
> of women on the hazard of that *progress* of anæsthesia, wh
> probably can never be ascertained so as to be foreknown." (

Dr. Meigs recapitulates these opinions in a letter to
in addition, he has used some very forcible expression
of chloroform in natural and instrumental midwifery.
the latter, he holds that, in a forceps case, to place a p
cumstances that she cannot answer yes or no to the q
is hurt or not, is to cast away our safest and most tr
for the questionable equivalent of ten minutes' exe
which even in this case is physiological. In relatio
cannot do better than transcribe his own words:

> "I readily hear, before your voice can reach me ac
> triumphant reply that an hundred thousand have taken it
> a witness that it is attended with alarming accidents, howev
> I exhibit the remedy for pain to a thousand patients in lab
> the physiological pain, and for no other motive—and if I
> destroy only one of them, I should feel disposed to clothe
> ashes on my head for the remainder of my days. What suf
> risk the life or the death of one in a thousand, in a question
> one of the general conditions of man?
>
> "As to the uses of chloroform in the medical or therape

the question changes. There is no reasonable therapeia of health. Hygienical processes are good and valid. The sick need a physician, not they that are well. To be in natural labour, is the culminating point of the female somatic forces. There is, in natural labour, no element of disease—and, therefore, the good old writers have said nothing truer nor wiser than their old saying, that '*a meddlesome midwifery is bad.*' Is chloroformization meddlesome?" (p. 325.)

We are bound to notice that Dr. Simpson has replied to these observations of Dr. Meigs, and other objectors to anæsthetics in midwifery, in a Report on its early History and Progress;* and to this we must refer our readers, until we take up the subject in more detail.

We own ourselves amongst those who are not convinced by his arguments, although we readily acknowledge their shrewdness and ability. In practice our objection is to its employment in simple natural labour. We have used it, however, with great advantage in the intense suffering which accompanies the irregular contractions of the first stage of labour in some hysterical women; also for a time in the second stage of labour, when it is prolonged, and the patient's powers become depressed, and her mind desponding. It is particularly indicated in that agonising form of precipitate labour, where the force and frequency of the pains are vehement and rapid; but with the exception of a single case of version, we have never employed it in instrumental midwifery. We believe that the judgment of the majority of the accoucheurs of any celebrity in London is adverse to its extended employment; and we think also that it has not found that favour with our countrywomen which its zealous advocates anticipated.

Preternatural labour. Amongst the various subjects which come under this division, Dr. Meigs includes the different forms of hemorrhage; which are comprised also in the last two lectures of Dr. T. Smith. The latter gives a very interesting exposition of the physiological modes of arresting uterine hemorrhage; without, however, in any material way increasing our resources for this purpose. Dr. Meigs refers to the practice recommended by Drs. Radford and Simpson, of detaching the placenta in some cases of placenta prævia. He argues theoretically, and under what we believe to be perfectly mistaken notions of the anatomy of the utero-placental circulation, that too much confidence has been reposed in this means of arresting hemorrhage. There is so much concurrent testimony, however, that when the placenta is separated, the bleeding ceases, that this fact alone is a sufficient warrant for the practice, provided it does not involve other invalidating objections. The necessary death of the fœtus is one of them; and in Dr. Meigs's opinion, such a dilatation of the os uteri as would admit of the perfect separation of the placenta, would also admit of the operation of turning. But this is just the question. It is certain that the os uteri will in some cases, in spite of copious hemorrhage, remain a firm, solid, undilatable ring, not admitting the introduction of the hand. Dr. T. Smith considers this condition, although in our minds without any show of practical truth, an active contraction of the cervix uteri, not a rigid unyielding structure. It is in this complication, one of a *very rare* description, that it is considered feasible to effect by the sweep of the fingers, an entire separation of the placenta. In some cases, where the placenta is small and not morbidly adherent, there can be no doubt that it may be

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* Anæsthetic Midwifery; Report on its Early History and Progress. 1848.

accomplished. But if there is but little liquor amnii, ɛ
fœtus during pregnancy has rested on the concave surf
it happens that the placenta is thinned and extende
herent. Such a complication we have met with; a
separate such a placenta with the imperfect means at o
operation which we feel persuaded would signally fail.
of this practice as becoming "very popular." If s
suppose that cases are treated by this method, which ou
by version. A man may have the greatest opportuniti
consultation practice, and he will very rarely indeed n
placenta prævia which would justify the artificial separa
So far as our own observation goes, the error is in
enough, and in looking rather to the positive extent o
os uteri, instead of to its dilatability.

We must refer our readers to a comprehensive discu
of puerperal convulsions, by Dr. Tyler Smith. The
treated it, involves so many physiological consideratio
beside our present purpose to analyse or criticise it
Dr. Meigs has given, in our minds, sufficient importan
of the blood which precedes eclampsia, and which
albuminuria. Dr. Meigs, indeed, does not even me
T. Smith alludes to the albuminuria as sometimes accon
convulsions, and as always caused by pressure on the ki
vessels and nerves, accumulating noxious elements i
believe that Dr. T. Smith will find the condition of bl
rated at the latter periods of gestation, and most
pregnancies, and which is accompanied with anasarca
of the body and albuminuria, to be the great cause of p
and to be of infinitely greater moment, in a practical
all the excentric causes put together. What this con
and how it is occasioned, are the pathological problem
to pressure on the large abdominal vessels, whether
being the cause, we know of no facts on which this s
What are the noxious elements? Urea, we know, is pr
quantity as not to account for it. Again, in some case
Bright's disease, which we know of, convulsions were
must look to animal chemistry for an interpretation of

Dr. Meigs records a very interesting case of labour,
the posterior lip of the uterus. The patient was thirty
and had been sterile for fourteen years; but before
children. The pregnancy had been more than usually
vaginal discharges. The labour was attended with in
at length the lower segment of the uterus, with the ma
separated and cast off, after which a full-sized child wa
died on the following day. There are three figures ill

Smallpox. Dr. Meigs devotes a paragraph to sm
with pregnancy and labour; in which he speaks of
attending it. He cautions the medical man against
patient to be exposed to the infection of variola, or to
or immediately after her pregnancy. He speaks c
shocking spectacles of distress from the vaccination o

and in an epidemic of smallpox, he would leave his pregnant patients to the chance of a natural infection, rather than bring them within the scope of its virulent power by a vaccine inoculation. "I have seen," says he, "pregnant women very nigh to term unnecessarily revaccinated, with consequences so terrific, that I think I would not, for a thousand golden crowns, vaccinate any woman knowing her to be pregnant." His opinion of the danger attending smallpox, and he might have added scarlet-fever, is thus expressed:

> "I will not say that every woman who gives birth to a child while labouring under smallpox must inevitably perish, since my clinical experience has shown me that the contrary may, rarely, be the case. But I do hold to the opinion, that a pregnant woman, labouring under a considerable attack of smallpox, is far more likely to be lost than saved, whether she miscarries, whether she be prematurely confined, or whether she give birth to her child at the full term of utero-gestation. A pregnant woman may be attacked with smallpox even in its most direful confluent form, and yet recover well, provided labour does not come on in the course of the disease; but, if she be confined or suffer abortion, she shall hardly escape death by hemorrhage from the womb, or by metro-phlebitis coming on early after the detachment and expulsion of the placenta." (p. 431.)

Embryotomy. The history of a very difficult delivery in a patient whose conjugate diameter of the pelvis measured only two inches, is recorded at great length. In conducting the delivery of this case, Dr. Meigs found the crotchet and Dr. Davis's osteotomist fail him in breaking up the cranial bones; and it was not until putrefaction had done its work, that the bones could be removed, and the head so reduced as to be drawn through the narrow strait, the woman being nearly dead from exhaustion. In a subsequent confinement, at full term, the patient not consenting to the induction of premature labour, Dr. Meigs first drilled a hole in the fœtal head with a convenient instrument made for the purpose; and then with two pair of pliers, the one straight and the other curved, he broke away the posterior and the front parts of the bones of the head, leaving only the bone to be withdrawn. Dr. Meigs figures these instruments, which are known in America as Dr. Meigs's Embryotomy Instruments; and he considers them an important contribution to the operation. In two subsequent pregnancies this patient underwent the Cæsarean section, the operation on both occasions being successfully performed by Dr. Gibson of the University of Pennsylvania. The children were saved and the mother recovered well. The particulars of the last operation are given in detail.

Puerperal fever. It is somewhat curious to see how uncompromising an advocate Dr. Meigs is of the treatment of this disease by bold venesection. He is an ardent admirer of Dr. Gordon's plan of depletion. "I scarcely can find words," says he, "to express my admiration of Dr. Gordon's work upon Puerperal Fever." Again he regrets the treatment recommended by other late writers, who, in general, are unfavorable to depletory measures. He does not believe in its being contagious. Our inference can only be, that he does not see the same disease in Philadelphia that we do in London or Dublin. As for bleeding in the epidemics we have witnessed here, whether in hospital or private cases, it is quite out of the question; it cannot be borne. The patients are struck with death by the puerperal poison; and to depress their powers by bleeding, is only to hasten the fatal end. A correspondence which we notice in another part of this Number, between Dr. Joseph Clarke of Dublin, and Sir Charles M. Clarke, will throw some light on this subject.

Our last word with Dr. Meigs is in justification of ourselves. He takes us to task for an uncalled for severity towards Professor Simpson, on account of his uterine sound, which Dr. Meigs praises most highly. It is rather a failing with the Doctor to be strong in his eulogy; he has an amiable way with him of becoming warm and glowing in his laudatory testimonials; and we should have paid more attention to his short criticism on ourselves, only that we see he has been premature in his judgment. He wants experience in the uterine sound. His work on Females and their Diseases, is dated 1848; and in it there is no mention of the uterine sound. His pleasing conversations with the ladies, with his drawings of the womb retroverted, did not contain the sound in the act of turning it forward and backward, and backward and forward; making and unmaking the displacement. We are now in 1849. Let him wait and try it, and let be seen in general practice; and we are well confident that our estimate of it will be verified. Our observations in the review referred to (January) on the uterine sound, and the uterine stem pessary, were founded on an abundant experience of these instruments; and we adhere, without a tittle's abatement, to the practical truth of our remarks.

ART. XV.

A Physician's Holiday; or a Month in Switzerland in the Summer of 1848. By JOHN FORBES, M.D., F.R.S. With a Map and Illustrations. —*London*, 1849. Post 8vo, pp. 520.

IT is refreshing to have a legitimate opportunity now and then permitted to us, of diverging from the beaten track of professional criticism, into the more verdant paths of general literature; such opportunities, however, are but too seldom presented to us. Considering the high mental accomplishment which exists in the ranks of our profession, it seems to us that there is too great a fear of allowing the public to know it, and that in consequence our body contributes proportionally less to the mental food of the present generation, than it has done in past times. We think that this is a matter of regret on several accounts; and not least because it tends to discourage that general cultivation of mind, on the part of the younger members of our profession, which is requisite, not merely to enable them to take that social position at which they ought to aim, but also to give them a comprehensive view of the objects of their professional study. If the men in highest repute among us for their practical skill, and at the same time the most distinguished for their ability as thinkers and writers, shrink from ever committing themselves to publication except on topics purely professional, their silence may well discourage younger men who have yet their position to gain, and who too often feel themselves compelled, not merely to avoid the ostensible display of the accomplishments they may possess, but even to the positive concealment of them.

On these grounds, then, even if on no others, we should welcome the appearance of the 'Physician's Holiday,' as an agreeable indication that taste and ability as a writer on a non-professional subject are by no means our body from the imputation of being good for nothing else than "doctoring." But the book has claims of its own on our friendly regard, which our readers will best appreciate after perusing a few samples of its contents, which we shall now place before them. The first chapter contains an amusing account, drawn from the life, of the various modes of spending the "holiday" which it is the custom of London physicians to take some time in the August or September of every year, that arise from the diversity of individual tastes:—some going to their country houses, to farm or garden; others to sport on hill or moor, or to fish in some out-of-the-way salmon-stream; others joining a yachting party, or circumnavigating our islands in a steam-boat; and others being obliged to content themselves with an occasional breath of country air, obtained by a hebdomadal trip to their families or friends out of town between Saturday afternoon and Monday morning.

> "A philosophical friend, whose active brain will not allow him to desert his books and his apparatus, even for the woods and fields which he loves so much, takes his holiday sometimes in quite a different style: he sends his horses to grass, shuts up his front-windows, retires to his library in the rear, and leaves strict injunctions with the footman to inform all inquirers, patients especially, that he has gone on his annual holiday." (p. 3.)

The travelling trip, however, seems the method of spending the holiday most approved by our author, as combining the most complete change with the most healthful influences; but in the genus traveller there is wide range of specific differences: one set thinking it best, as Dr. Forbes happily expresses it, "to let the written troubles of the brain be effaced spontaneously by time alone; whilst another prefers driving the old notions out by forcing in new ones, on the principle of the pop-gun." Of these two methods he gives us the following examples; the individuality of the second of which is well known, we doubt not, to most of our readers.

> "Of the former class of travellers I know one who will hardly accomplish his hundred miles of distance in half as many days, and whose delight it is to interpose a week between each successive stage of his progress, such week being mainly spent in a gentle daily walk to some easy height in the neighbourhood, which may command a quiet view of the open sea or secluded valley, and where he may stretch himself unmolested on the green turf until the hour arrives for his return to his gay hotel or village inn, as the case may be.
>
> "Of the latter class, the most prominent member is one who is among the very first, as well in professional station as in intellectual endowment and varied knowledge, and whose annual delight is to speed to the uttermost end of the earth that can be reached and returned from, within the period of the inexorable holiday. Year after year has he thus visited, in succession, not merely all the nearer countries of Europe, but the most remote; and has repeatedly gone beyond these, to the east, to the south, and to the west. Norway, Russia, Asia Minor, Egypt, Algeria, Greece, Spain, Canada, the United States of America, have been thus swept over—if with the wing, also with the eye, of the eagle—and yet the old Library in the West End is infallibly regained on the very day fixed on for the termination of the holiday ere yet it had begun. Could my Publisher prevail on *him* to write out his 'Holidays,' there would be then something of a stamp that would enable many, for a time at least, to make holiday at home." (pp. 4-5.)

By way of apology for adding another to the many records of travel

over ground of which the greater part is already famili
that on returning from his trip, he felt himself so
strength, and spirits, and retained such a vivid impre
pleasures enjoyed during its continuance, that he cc
disposition to relieve his own "stuffed bosom" b
others something of what he saw, did, and felt, with
that by so doing he might induce some of his town-tie
had done, and to enjoy as he enjoyed. Those who, lil
when fresh from his intercourse with Nature, under
and most beautiful aspects, witnessed the enthusias
scribed some of the most striking features of these s
which they had produced on his mind at the time,
earnest recommendation to his friends to "go an
easily understand how this account of his experien
before the public. He makes "no pretensions to t
give to familiar things the gloss of novelty, or make
the rose,"—nor has he laid himself out for the collec
formation which interests the grave matter-of-fact r
anything to excite in the personal incidents which th
he has great faith in the merits of his subject, and c
to believe that *any* attempt to delineate its beauties
uninteresting, so long as it keeps nature and truth
who have the privilege of familiar intercourse with t
will have a peculiar charm as a record of his individu
to those who have not, we can strongly recommen
valuable guide in the accomplishment of any similar de
of the scenes it describes, reflected by a mind distin
cultivation, for its ardent admiration of nature, and
thorough geniality of temperament, which leads to a c
every healthful form of pleasurable excitement, and d
sible upon sources of annoyance or discomfort. Perh
cipation from those editorial chains which he bore s
long years, added not a little to the zest of his en
they do not seem to have cramped the vigour of o
narian," whose pedestrian performances, we suspect,
of his brethren whose legs seem as if only fit for
carriages into the houses of their patients.

In his Second Chapter, he gives some valuabl
Travellers,—as to "what to eat, drink, and avoid;"
make a quotation in regard to the use of cold wat
which we believe that there is much needless and in
leading to unnecessary abstinence from one of the
invigorating of all beverages:

> "With the Alpine pedestrian, toiling hour after hour
> deep valley or up the mountain's steep sides, the supervent
> necessary result of the great loss of fluid by the skin and
> hint for the resupply of them. It would be strange, there
> tion of this natural and irresistible instinct were injurious
> it were highly dangerous, as it is generally considered to b
> of drinking cold water I believe to be an entire mistake;
> it as a thing to be forbidden to the heated pedestrian, I
> less wholesome than it is delicious.

> "I am well aware of the fact that dangerous and fatal results have followed the sudden ingestion of cold water by travellers and others who had been undergoing great bodily exertion in hot weather. In recommending the use of it to the Alpine traveller, I must, therefore, guard myself against all risk of leading him into danger. It is its moderate and rational use I sanction and advise, not its immoderate and irrational use. The circumstances and bodily condition under which dangerous consequences have resulted, or are likely to result, from drinking cold water, seem to be the following: first, the exhaustion of the strength from previous over-exertion and consequent depression of the heat-producing and cold-resisting powers; second, the sudden application to the stomach of a large quantity of very cold water, when the system is in this state. And so long as the pedestrian eschews this combination of circumstances, I believe he may freely indulge his taste by filling his drinking-cup at every spring he passes in his way. So far from the simply heated state of the body being here an element of danger, I believe the hotter the individual is, provided he is not exhausted, and provided he does not drink an excessive amount of water, the safer is the practice. But he should content himself with a small cupful at a time, should drink this slowly, and, as a rule, rather drink often than much. This is more especially proper when the water is very cold; and for this reason the pedestrian should always consider this quality of coldness when he is indulging his thirst, the rather as there is considerable variety in the degree of temperature of the springs he meets with.
>
> "When no other supply is at hand, the traveller may, if in the condition of vigour above mentioned, drink with perfect safety of water just melted from the glacier, as I have myself repeatedly done; but in this case he should drink very slowly, sipping rather than drinking, so as not to chill the stomach too rapidly, and not in large quantity, rather renewing his draught after a time, than quite satiating his thirst at once. In drinking water of the temperature of 33° under such circumstances of extreme bodily heat, I have been surprised to find how very little its extreme coldness was felt either in the mouth or stomach; it seemed to the sensations hardly to exceed the ordinary temperature of summer water. . . . .
>
> "In regard to this matter of drinking cold fluids when the body is heated, I should be sorry to draw positive conclusions on a matter of this importance, either from theory or from my own limited experience; I think it right, therefore, to corroborate my opinion by the practice of the guides: each of these carries his leather or wooden drinking-cup in his pocket, and never hesitates to take a moderate draught from the passing springs as his thirst prompts him, and without any injurious result, as far as I know." (pp. 22-5.)

Dr. Forbes's previous convictions in regard to the beneficial results of the habit of total abstinence from fermented liquors, were fully borne out by his personal experience on this journey; and he found the guides of Chamouni and the Oberland unanimously of opinion that, in their winter excursions among the snow, spirits are injurious; and, although they regarded the moderate use of wine as beneficial, it must be remembered that the ordinary wines of Switzerland are very weak.—Among the great advantages of travelling to the dyspeptic or bilious invalid, Dr. Forbes enumerates the emancipation from the trammels of systematic *dieting*. "Instead of enjoying real, vigorous, independent health," he truly remarks, "the votaries and victims of such a system can only be said to live a sort of negative, artificial life, as if by Nature's sufferance, not her sanction—and, for a man's life, one surely both afflicting and degrading." Out of such a thraldom it is barely possible for an invalid to escape at home: the chain of associations, and the feeling of dependence, are too strong for the will weakened by protracted indisposition; but with an entire change of circumstances, the spirits and stomach are so altered, that the plain or even coarse food which would have been previously felt

like poison, becomes not only harmless but wholeso
is, that the invalid must "seek for and see the sig
all the exercise his strength will admit of, and rem
of *moderation* at all times, but more especially in
his emancipation." The use of the Alpenstock ha
ficial effect on the digestive powers, with the water-
the bracing breezes of Graefenberg or Malvern.
tute action for inaction, the toil of muscle for the t
for coddling; the roughness of the ruder times an
the luxuries and refinements of an advanced civiliz
to a natural condition of the system, that is, t
sequence."

We have been thus tempted back by the sound
introductory remarks, into that very beaten track fr
just now rejoicing in our temporary escape. We m
taste of a purer atmosphere and the sight of a brigl
passage we shall cite by way of sample, is Dr. Forb
pression produced upon him by the oft-described F

"Later in the evening we walked out on the terrace
enjoy the view of the Falls by moonlight. The evening
had been; warm, cloudless, and without a breath of winc
of tumbling foam lay straight before us, the only bright
landscape, and attracting and fixing the eye exclusively
heard but the one continuous roar of the water, soften
seeming to fill the whole air, like the moonshine itself. T
wild and delightful in the hour and its accompaniments. T
to the impressions made on the senses. A host of half-for
thoughts crowded into it at the same time, giving rise
and melancholy, accompanied with a sort of objectless sy
something unknown. The ideas and emotions most de
those of Power and Perpetuity, Wonder and Awe. Wha
senses and the mind seemed a part of something infinite
comprehend nor shake off: the same mass, the same rus
night, year after year, age after age, now and for ever!
the Hartley school might explain all this by supposing tl
—that of *Eternity* to wit—had got into some cranny of
weak either to contain or sustain it, and had thus given r
Fall, just as has here betid the Rhine, struggling in its r
ing its broken barriers. Hence, in both cases, the whirl
and hurlyburly.............

"In regard to the general effect of these Falls on the
that they impressed the intellect much less than the fee
somewhat disappointing, particularly as to the dimensi
breadth and height; and as I gazed I felt a sort of criti
within me; but this was speedily subdued by somethin
reasoning, and there only remained behind, such ideas an
attempted to describe. Milton makes his Adam and E
they are happier than they know:' the spectator of th
grander than he thinks." (pp. 69-73.)

Our next tableau shall be of a purely Alpine sc
Sea, on the flanks of the Schreckhorn.

"As we looked up to it from this position, the view
grand. It had exactly the aspect, and excited in th

feeling—as if a gigantic river, a hundred times as broad as the Rhine at Schaffhausen, and falling over a precipice proportionally high, had been instantaneously frozen, and fixed in all its fury and turbulence—a Titanic waterfall, white and foamy, but eternally silent. So strong was its resemblance to a waterfall, or, perhaps, I should rather say, so strongly did its appearance excite the idea of a waterfall, that one almost required the aid of hearing to be convinced that the huge icy mass above him was not in reality a tremendous rapid rushing down from some unseen and unknown lake in the regions above. The eyes were fixed on it by a sort of fascinating terror as well as wonder: the idea of danger, as from a torrent about to overwhelm you, forced itself into the mind in spite of your conviction; the very silence of the scene seemed unnatural and awful.

"The above statement may seem exaggerated; but it is really not so, either as regards the scene itself or the ideas and feelings excited by it; and I am of opinion that few persons, possessed of only a moderate share of imagination, could be brought suddenly and unexpectedly, and without previous warning or expectation, into the presence of such a spectacle (as was the case with myself), without experiencing all that has been described—and more. The comparison of a glacier to a frozen river or torrent or cataract, is one likely to occur to any beholder; it, however, had not suggested itself to me when looking on those I had already visited or seen at a distance: and although I must have formerly seen this comparison both in Coleridge and Byron, I fortunately had entirely forgot it; so that the spontaneous excitation of the idea on the present occasion was accompanied by all the additional force which perfect novelty gives to any mental impression. On referring to the poets since my return home, I was delighted to find the forgotten idea expressed in their own glorious language:

'Ye ice-falls, ye that from the mountain's brow
Adown enormous ravines slope amain—
Torrents, methinks, that heard a mighty voice,
And stopped at once amid their maddest plunge!
Motionless torrents! silent cataracts!'*

'————— O'er the savage sea,
The glassy ocean of the mountain ice,
We skim its rugged breakers, which put on
The aspect of a tumbling tempest's foam
Frozen in a moment.' "† (pp. 224-6.)

This great sight was speedily followed by a revelation still more glorious.

"Hitherto all the great mountains surrounding us had remained covered with an impenetrable veil of mist, so that we could see nothing but the base and brows of those nearest to us, and we had almost given up all hope of seeing anything of their loftier regions, when, all at once, the clouds on our left hand began to exhibit in one spot some tints of sunlight, and then opening out their folds, one after another, displayed to us the very topmost peaks of the Schreckhorn in all their glory. For some time I could scarcely believe that what we saw was anything but illuminated clouds of singular shape; so entirely beyond the point where I should have thought of looking for a mountain, were the snowy peaks that now appeared; and when some of my companions shouted out 'The Schreckhorn!' I began to look for it much lower down. The Schreckhorn, however, it was; and it remained long enough uncovered, or rather it momentarily uncovered itself sufficiently often, not only to leave no doubt as to its identity, but to show itself in as striking a point of view as, perhaps, a mountain-peak can ever be seen in.

"The whole mountain to its very base was wrapped in a dense, whitish mist, so as to give no indication of its outline; nothing, indeed, being visible in that direction, but a continuous wall-like mass, extending from the earth upwards; well-

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* Hymn on Chamouni. † Manfred.

defined at its base, but gradually lost in the general body c
upper sky. There was, consequently, nothing left whereb
and when the snowy summit broke through its covering, it
greater than if the whole mountain had been exposed. Not
grander or more sublime than this view, more especially wh
maining stationary for a moment, let us see the peaks set,
work of clouds, their interior edges rounded and curling, a
the same unseen sun which illumined the yet brighter peak

We should gladly quote, did our limits permit, the
account of his visit to Dr. Guggenbühl's Cretin Establis
but we must content ourselves with extracting th
interest.

"In descending towards the house, I encountered midwa
some twenty of Dr. Guggenbühl's patients or pupils, cli
exercise, and amusement—all combined—under the supe
dressed young man and two of the sisters of charity who
ment. They were all children, from the age of twelve o
three or four: one was carried by a servant, being incapa
were running and waddling and tumbling on the grass, a
way, with the servants, with one another, and with a fine
made one of the party, and who was probably of nearly the
as some of his poor biped companions.

"This little exhibition at once satisfied me of the enlig
Guggenbühl's views; and I felt much greater pleasure in
amining the poor objects of his benevolent care, amid th
and as it were in Nature's own presence, than if I had see
ward or schoolroom, under restrictions which they probab
stand nor well brook............

"Dr. Guggenbühl justly considers cretinism as a physica
an imperfect development of most of the bodily organs, a
ticular, on the imperfection of which latter organ all the m
Whatever be the special cause of the affection, he conclud
proving the bodily health generally, by strengthening a
developing to a higher degree of functional activity—all th
and among the rest, and in an especial manner, the brain, t
benefit can be founded. It was therefore a beautiful and
ciple which he adopted as the indispensable basis of all his
of having the infant-cretin removed from the low, close vall
generally originates, to the free, dry, cool, bracing air of the
sheltered and sunny slopes of the Abendberg. In such a
is powerfully restorative, all the most effectual means of i
applied with the best prospect of success; while they
failed so long as the deteriorating causes were in full opera

"Heroic doctors, in their ignorance of the way in whi
may attempt to cure a chronic disease by a *coup de main;*
porary relief for real cure, may themselves suppose, or be
have done so; but every physiological physician knows
dition which may have been months or years in forming, c
permanently removed by means which act slowly and for
one part only, but, more or less, on the whole system. A
certainly, in the cases now under consideration; in the
which nothing is to be neglected that can help to waken
in that gentle and imperceptible but uninterrupted mode i
all her great and permanent changes in the organic world o
no less than in the mere physical portion of her domains..

"Dr. Guggenbühl was so kind as to examine, in my presence, three or four of his more advanced pupils in this species of instruction; and it was delightful at once to see the amount of real knowledge that had been thus acquired, and the gratification which its acquisition and the conscious possession of it evidently conferred on the respective pupils. Not that the poor children knew much or could do much; far from it; but what they did, sufficed to show that the instrument was capable of actiôn, and left no grounds for doubting that perseverance in the same course would lead to something still better. At the very least, the actual result showed the existence in the poor children of the quality of *teachableness;* and this quality can be made subservient, in many ways, to the acquisition of *habits* which cannot fail to add to the comfort, health, and happiness of themselves as well as of their relations. This has been sufficiently manifested by many cases at present in the establishment, and by others that have left it. And, indeed, when we merely see that those who could neither walk, nor talk, nor feed themselves, have learned to do so by instruction, we need not doubt that improvement may take place in matters involving only a little more cerebral or mental action." (pp. 263-70.)

Dr. Forbes is inclined to think that many of the inmates of the Abendberg establishment, reputed to be cretins, differ in no essential particular from the ordinary idiots of our own or other countries; and infers from the inquiries he has made, that all Swiss idiots are classed as cretins. He does not consider it as proved, that cretinism has any essential connexion with goitre, beyond the general fact that they both commonly prevail in the same localities; innumerable goitrous subjects having no taint of cretinism, and many cretins having no goitre. But still he believes that they may both be owing to the same cause, or to a modification of the same cause; and regards that cause as of the nature of miasmata or malaria. There is an influence existing in certain localities, sufficient to produce it in families who settle within its sphere; whilst, on the other hand, removal from its source is the first and most essential step towards the eradication of the morbid condition. On the whole, it appears certain that cretinism is on the decline; and this diminution seems traceable to an improvement in the habits of the people, who pay more attention than formerly to cleanliness, both of their persons and dwellings, and who send their children much more out of the valleys into the mountains, in consequence of the multiplication of alpine chalets for summer residence.

Our next extract relates to the remarkable "hot water cure" practised at Leukerbad, from whose hot springs, of the temperature of 115°, 120°, and 124°, a constant supply is obtained so as to keep up the temperature of the bath at or near to that of the human body.

"In making use of the baths, the usual course followed is to go into the water at four or five o'clock in the morning, to remain in it from one or two to five hours, and to go into it a second time in the afternoon, and remain from one to three hours; making the daily stay in the bath from two to eight hours. The bathers undress and dress in heated rooms adjoining the baths, and always retire to a warm bed in their hotel, for half an hour or an hour, after bathing. They are clothed in very long dressing-gowns of flannel or thick linen, reaching from the throat to the feet. Thus dressed, both sexes occupy the same bath. Each person is provided with a small floating table, with a small basket for holding the handkerchief, snuff, books, &c., also for eating off, playing on, &c. At the time of our visit, none of the baths contained more than ten or twelve persons; but we saw and heard enough to enable us to judge of the strange scene they must present when they are quite full: as it was, there seemed great sociality and mirth among the company............

"The baths are employed for many chronic diseases, but
tion is in cutaneous diseases, scrofula, chronic rheumatisı
Of their great efficacy in many such cases, as well as in oth
scription, we have sufficient proof in actual experience; and
might be fairly expected from so powerful an agency as hot
in the manner it is applied here. Immersion in a fluid of a te
or exceeding that of the human blood, for a fourth or third
four hours, during the space of a month or two, *must* pr
modification in the actual condition of the animal functions;
if this modification were not sometimes beneficial as well a
It could be easily shown, on physiological grounds, how this
known by actual experiment to be so." (pp. 308-10.)

The town and baths of Leukerbad are situated at a
4,600 feet above the sea; and, as it were, in a basin
centre of a ridge of snowy mountains, with vertical
The climate is so extremely rigorous, for eight month
as almost to preclude access to it; but during the s
curious excavation seems almost a realization of t
Rasselas.—Proceed we now, however, to that which c
feature in Dr. Forbes's "Holiday," the view from
Riffelberg, which brings into one grand panorama m
mountain scenery of Switzerland. As before, we
description of the scene itself, but the record of the
left upon his mind.

"In looking back to this gratification, and attempting to
that it was compounded of many elements—all, no doubt, mo
some of them seeming of opposite, if not contradictory, q
forms or modes of the enjoyment varied greatly from time
describe what I felt, but I know I felt vividly and strongly.
able excitement, but only of that kind which arises from t
emotions. Though beautiful almost beyond conception, t
solemn.

"Sitting there, up in mid-heaven as it were, on the smo
rock; in one of the sunniest noons of a summer day; ami
vation and the perfect exposure to the most delicious tem
of the richest blue, and either cloudless, or only here and th
aerial and sunbright cloudlets which but enhance its depth;
vision, from the valley at our feet to the horizon, filled w
every variety of form, and of a purity and brilliancy of w
common whiteness dull;—we seemed to feel as if there cou
mood but that of an exquisite yet cheerful serenity—a sort
or absorption of our powers, in one grand, vague, yet most
Beauty and Loveliness.

"At another time—nay, it would almost seem at the sa
the alternation from mood to mood—the immeasurable vast
scene, the gigantic bulk of the individual mountains, the p
yond the level of our daily earth as to seem more belongin
our own elevated and isolated station, hemmed in on
wastes and impassable walls of snow; and, above all, the
absence of every indication of life and living things—sugge
the foot of man had never trodden, and never would tread
analogous ideas would excite a tone of mind entirely d
melancholy.

"Then would intervene thoughts and ideas still more fa

> the reason, while recognising their unreality, submitting without a struggle to their domination. Among these fancies there was one which haunted me for a space, and which, no doubt, was excited, though quite unconsciously at the time, by the suggestive forms of some of the mountains, as well as by the immobility and silence of everything connected with them. It was that of being environed and guarded, and as it were imprisoned, in an awful circle of Titanic Sphynxes, planted there from all time, in the dreadful silence of some mysterious watch. There they seemed to sit on their icy pedestals, side by side, in death-like repose, looking down on you with their fixed and melancholy eyes, breathing over the senses a sort of magnetic stupor, and chaining soul and body to the spot by an irresistible fascination.
>
> "Although the scene here attempted to be described needs no extraneous aid to enhance its qualities, I can hardly doubt that, in my own particular case, the impression made on the mind was in reality strengthened by surprise. I had had no distinct notice or indication of what I was to see from the Riffelberg. I had never heard the view named except by Dr. Hasse, and he had merely, in our hurried interview, advised us to visit it as something extremely well worth seeing, but without entering into any particulars: no one else who had seen it had mentioned it to me; I had read no account of it except that in our Guidebook, and this conveys no just idea of the thing; even our partial views of this morning, and while ascending the mountain itself, had in no manner prepared me for the overwhelming splendour of the sight which burst upon us on reaching the summit of the Rothe Kumm. I said at the time, and I still feel disposed to believe, that the whole earth has but few scenes that can excel it in grandeur, in beauty, and in wonderfulness of every kind. I thought then, and I here repeat my opinion in cool blood, that had I been brought hither blindfolded from London, had had my eyes opened but for a single hour on this astonishing panorama, and had been led back in darkness as I came, I should have considered the journey, with all its privations, well repaid by what I saw." (pp. 353-56.)

We must not altogether pass by those incidental notices of the people whom our "healthy sexagenarian" encountered in the course of his perigrinations, which reveal the character of the writer as much as of the subjects of his description. On their way to Schaffhausen, the party stopped at the small town of Säkingen, to refresh the horses; and after visiting the church, Dr. Forbes strolled into a cemetery at the further end of the town.

> "In a small open chapel at one end of it I found two men and three women at their prayers, kneeling before a large image of Christ. One of the men was reading the prayers aloud from a German prayer-book, with great correctness of pronunciation and emphasis, and with much apparent earnestness and devotion. The prayers, which were addressed alternately to the Virgin, Christ, and our Heavenly Father, and in the order of frequency here set down, seemed to possess the fine old nervous style of the English Liturgy. The people were all of the humbler class, very plainly yet neatly dressed; and the reader, I was informed, was a layman of the village, in the same rank of society. There was something very impressive in this exercise of devotion at such a time, in such a place, and by such persons; and it was impossible, while listening to the solemn and earnest words of the humble man who officiated, and the unaffected responses of his hearers, not to feel that the devotion could not be wrong in them, whatever opinions might be held respecting some of the objects of it." (pp. 66-7.)

In the Alpine valley of Urnerboden, he fell in with a good priest, who enlisted his professional sympathies in behalf of some members of his little community.

"I accompanied the good man to a cottage, a short distanc
visit a family who were sick. I found in one small room
windows closely shut, a mother and one child in one bed and t
a second, all of them labouring under dysentery. They were
woman and by the father and husband, a man upwards of six
tainly one of the finest men I ever saw. My prescription w
and windows, to allow the forbidden luxury of cold water and
food, and to apply fomentations to the affected region; and
full confidence of their recovery, though there was probably n
cine nearer than Altorf. I was much interested by this good
Urnerboden, who, in his humble and lonely station, seemed to
faith and unaffected simplicity, the sacred duties to which he
and when he took leave, after accompanying me some distanc
could not help feeling more warmly than the occasion might
thought to require. But the sources of sympathy had been o
and the imagination might easily be impressed both by the
stances of the meeting and the parting; and, besides,

'One touch of Nature makes the whole world kin'

Dr. Forbes seems to have been fortunate in his
clerical order. Here is another specimen, of whom o
under the influence of a most excellent breakfast, ex
any previous ceremony of introduction, as if the go
actually been the *hospitium* which his guests took o
served with his own hands, and compensated by the
francs, which was all he would accept for the whole p

"We were, of course, reluctant to be thus waited on by a
profession, and made many apologies; but the good man so
they were unnecessary, by the unaffected readiness and sir
served us. He seemed to act as if his present proceedings v
quence of his social position in this locality; yet in doing w
to be his duty, he betrayed not the slightest evidence that
fectly agreeable to him; nay, more, it seemed to me that w
all the functions of a servant, he still preserved the bearing
was no symptom either of real pride affecting humility fro
subserviency originating in a base nature and practised fo
thing with our good host seemed natural, simple, and true
to believe that he was acting from religious motives, I confe
all his proceedings, from first to last, I could not help rev
equality of the human brotherhood, which was both profess
founders of the religion of which he was a minister." (p. 3

In returning by the same route after his visit t
Forbes pays his clerical host (who proved to be a
visit, and has an equally propitious reception.

"We reached the hospitable mansion of our good Johann
about eleven o'clock, and again had the benefit of his frier
plentiful and luxurious breakfast. On this occasion we n
what with German and what with Latin, I contrived to
information from the worthy and kind-hearted pastor......
the good priest, which I did not do without very friendly
vail upon him to accept more than a five-franc piece for all
him to, and for the free expenditure of his worldly substan
for the guides were his guests as well as we. In final rep

to himself, if ever an opportunity should occur, the good man, showing the same kindly nature as in everything else, recommended to my sympathy any of his unhappy countrymen who might cross my path in their exile. And so we took our leave of St. Nicolas and its simple-minded and gentle priest." (pp. 374-5.)

We must conclude our extracts with a passage descriptive of the character and habits of the common people of Switzerland, and showing that genial appreciation of their best points which might be expected from the kindly disposition of the writer.

> "So far as we have hitherto had opportunities of judging, and speaking generally, I am bound to say that the common people of Switzerland are a fine race, physically and intellectually. Our intercourse with them was necessarily limited, and lay chiefly with that class of men who fill the station of husbandmen, guides, drivers, &c. With the few exceptions mentioned in their place, we found these persons remarkably intelligent and well-informed on all matters relating to the country. They seemed also straightforward men, without any servility, and yet with nothing of of that self-satisfied and obtrusive independence which has been said to mark the republican character. They spoke their minds freely but civilly, with unaffected and manly simplicity, showing only that degree of respect and deference which an honest nature spontaneously yields to superior knowledge. They were excellent companions, yet excellent servants; conversing with you without restraint, helping you without pretence; and annoying you neither by unmanly humility nor by ignorant impudence. They obviously felt that, in their station, they were as respectable as yourself, and they expected to be treated with that consideration which was their due: but their claim to such respect seemed to be founded on the conviction that they were fulfilling the duties belonging to their position in life, as you were fulfilling yours. It was always pleasant to travel with such men; and when the journey was over, it did the heart good to see them the first to hold out the hand, bidding you farewell with a cordial and honest shake, which seemed rather to express regard for the man than obligation to the employer. From what I have seen of men in the same class of life in our own country, I would compare my Swiss friends, more particularly in regard to intellectual development, with the miners of Cornwall, and with the shepherds and mountaineers of Cumberland and Scotland: they are greatly superior to the class of agricultural labourers and common artisans in England." (pp. 257-8.)

Our readers will have gathered, we trust, from our account of the book, that which it was our intention to convey:—namely, that whilst it will serve as a most useful guide to any who may desire to go over the same ground, it is also one of those highly graphic and lively narrations of the author's experiences, which forms the best compensation the stay-at-home traveller can have, for the want of opportunity to see Nature with his own eyes, or to extend his peregrinations beyond the limits imposed by his daily round of duties. And those who have been accustomed to think of Dr. Forbes only as the learned editor of the 'British and Foreign Medical Review,' or as the bold physician who has dared to tell his brethren how little they really know about the natural history of disease, and how empirical is their system of medication, will here meet him in a new character,—not new, however, to those who have enjoyed his intimacy,—that of the ardent lover of Nature and of his kind, the cordial, sympathetic, and true-hearted MAN.

ART. XVI.

A Dissertation upon Dislocations and Fractures of Shoulder-joint. Being the Jacksonian Prize Essa THOMAS CALLAWAY, Demonstrator of Anatomy t &c. &c.—*London,* 1849. pp. 178.

THE Jacksonian Prize for the year 1846 was awarde by the Council of the College of Surgeons, for an essay lished, and which recommends itself to us, as well from as from its intrinsic merits. The demonstrative part for example, which relates to the anatomical descriptic tremity,—is exceedingly clear, and does great credit powers as a teacher; we regret, however, that the sa maintained in other portions of it, which are occasi very often inelegant. We wonder that the baldness o as this—"Essay on Shoulder Joint"—which stands volume, did not strike the author, as much as we are the taste of most of his readers. It is, in fact, scarce however, are venial faults, that may be forgiven when and we shall therefore abstain from pointing out nur the kind, which a little more practice in writing woul

Mr. Callaway's mode of proceeding is this. He description of the bones, ligaments, muscles, nerves, shoulder; and next proceeds to consider the accident parts are liable. Fractures and dislocations of the cl the scapula,—dislocation of the shoulder-joint,—inj head of the biceps muscle,—and fractures of the hea successively engage his attention. We propose t Callaway's labours, and present to our readers, such servations as seem particularly worthy of notice.

In the chapter on the Anatomy of the Scapula, M the ingenious investigations of Mr. Ward, which ha covery of a law that is thus expressed: "Each fossa l opposite a certain central point, and consequently the muscle lies in that precise position in which it can o on the head of the humerus." (p. 11.) It is deli something like an approach to the philosophical stu must become endowed with an interest that it has ne the more earnestly our demonstrators and anatomica its investigation in this spirit. Similar commend following description of the tendon of the biceps mu

> "The long head of the biceps is of so great importance shoulder-joint, in directing the head of the humerus, that i considered as an inter-articular ligament by some anatom ever, its importance has been rather neglected or under thology will be spoken of hereafter. It arises from the u cavity (being most intimately connected, or rather identic ment itself), passes over the head of the humerus, and und being perfectly independent of either; it is entirely inves of its own, and holds much the same relation to the cavity

ligamentum teres does to that of the hip. Having arrived at the margin of the anatomical neck of the bone, it passes between the two tuberosities, and down the bicipital sulcus, to the belly of the muscle, whose great natural contractile power, being further stimulated by the constant action of the fore-arm, tends to keep the tendon tense, otherwise it would be constantly liable to be thrown into a fold, or, becoming lax, to impede the motions of the joint. It will thus be seen, that the tendon in some measure tends to render the capsule of the joint incomplete; it, however, forms about one third of a circle, which, with the concavity of the glenoid cavity, together make about two thirds of a circle, and compensate in no slight degree for the general shallowness of the cavity. Let us suppose that the deltoid be put into motion; the head of the humerus ought to be brought directly upwards, and would, without doubt, impinge against the under surface of the acromian arch. This, however, does not take place; the polished hemisphere of the humerus is carried against the under surface of the tendon, which, acting as a curved director, guides the rounded head of the bone inwards and backwards against the glenoid cavity." (p. 25.)

It is to Mr. Hilton, the Lecturer on Anatomy at Guy's Hospital, that we are indebted for this ingenious and plausible physiological explanation of the function of the biceps tendon.

We pass over the rest of the anatomy of the shoulder, to arrive at the following passage on *Fracture of the Clavicle;* in which the author, getting out of his accustomed beat, exhibits some of that faultiness of style which has before attracted our attention.

"I am not prepared with facts, to say to what amount the extent of cancellated structure in this bone may influence its repair, neither am I able to show, satisfactorily, at what *ages* its fractures occur most frequently; it certainly is very common among children, and those of tender years,—is oftentimes overlooked,—still the greater number of specimens to be met with in most museums show that the adult clavicle is very often broken, perhaps consequent upon its great exposure to circumstances likely to produce the accident. I therefore am inclined to receive anything but statistics with caution, in drawing a parallel between this and any other bone of the body, with reference to the ages at which it is most frequently broken." (p. 47.)

It is well known that, in almost all instances of fracture of the clavicle, the sternal end is the most elevated; but the causes of this peculiarity are not so clear. As usual, the truth seems to lie midway between two conflicting opinions. Neither extremity of the fractured bone is in its exact natural relation. The sternal end is elevated, probably by the action of the sterno-cleido-mastoid muscle; and Mr. Callaway has drawn attention to the fact that the extreme mobility of the sternal portion, after the clavicle has been divided in the dead subject, is evidence that it would be readily influenced by muscular action when this happens in the living. The acromial extremity of a fractured clavicle is depressed and drawn inwards, chiefly by the weight of the arm; for as every one must have observed, the depressed portion rises when the hand is placed under the elbow, and the shoulder elevated. On this account some surgeons consider it necessary merely to support the arm in a sling, by means of a handkerchief passed under the elbow, and tied over the opposite shoulder; and we have seen cases turn out very well, when the fracture has been treated in this simple way. Desault attributed, and probably correctly, the drawing-in of the fragment to the actions of the pectoralis and subclavius muscles.

Sometimes fracture of the clavicle occurs in young p
displacement, in consequence of the periosteal and car
covering the bone remaining entire; and it is thus n
to meet with children, in whom the gradual formation
clavicle from the effusion of callus is the first evidence
previous fracture. Such lumps have been mistaken fo

There is also another form of fractured clavicle in
overlapping; and depending, according to Dr. Smith o
the first to explain it, on the occurrence of the fracture
that the coraco-clavicular and coraco-acromial liga
portions of the bone in their natural situations. Such
least they did in the series of preparations which Dr.
right angles, and without much effusion of callus arou

The termination of cases of fractured clavicle is
favorable; there being very few cases extant in which
either by ligaments or with the intervention of a fal
rare for the surrounding vessels to be involved in the
times the brachial plexus of nerves is bruised, when o
great; and to the occurrence of such cases must be attri
—certainly only partially true,—that fractures of th
painful than similar injuries to other bones.

In considering the treatment of this injury, Mr. Call
"The reduction of the fractured ends of the bone, t
dition, if attempted, is exceedingly easy." It is diff
this point without *the attempt* is made.

The contrivances for keeping fractured clavicles i
reduced, are very numerous, and all more or less u
fact is, that, in accordance with the general law, the
called provisional callus effused around a fracture, is
movement which takes place between the fragment
this bone never unite without some deformity, which
interfere with the usefulness of the limb. Mr. Cal
correctness of the observation we made in our not
Minor Surgery, in the number of this Review for
figure-of-eight bandage affords results as satisfactor
does not, however, agree in our commendation of plas
applications; but seems to imply that he speaks w
their employment.

Dislocations of the clavicle. All forms of this a
Mr. Callaway does not add much to our previous kno
however, two interesting cases of dislocation backw
end of the clavicle. Both occurred at Guy's Hospit
Mr. Key; one from the kick of a horse, the other fro
The dislocation in the former case could not be red
was cured without deformity; and in neither was t
determining the nature of the accident.

Fractures of the scapula. These Mr. Callaway div
of the body, as supra- and infra-spinal fracture; 2d
as fracture of the anterior angle or neck of the bone
inferior angle; and, lastly, fracture of its process
body of the scapula are commonly transverse, an

violence; but our own opinion agrees with that of Mr. South, that the violence necessary to produce this injury has been overrated. Certainly we have met with several cases of the kind, in which there was very little bruising of the surrounding soft parts, and the detection of the accident was uncommonly easy. Our author subjoins an example of the injury caused by muscular action. The case is related by Dr. Heylen as occurring in the person of a labourer, who was suspended for some time by the left hand from the body of a cart, into which he was scrambling, when the horse drawing it took fright and ran away. The diagnosis was difficult, for there was little deformity, and no trace of external violence.

The following passage introduces the subject of fracture of the neck of the scapula.

> "The question of the existence of a fracture of the cervix scapulæ, or anterior angle of the bone, has for years, like that of a union of fracture of the cervix femoris, been debatable ground among English surgeons. At one time all injuries in and about the shoulder, which were at all obscure, and which baffled the acumen of the surgeon, were set down as fracture of the cervix scapulæ; if any accident was found to be neither a fracture of the head of the humerus, nor dislocation of that bone, then the scapula was at fault, and a fracture of its neck was diagnosed. The rarity, however, of any pathological specimens of this injured bone have gradually led many surgeons in the present day to suspect the frequency of its occurrence, and some to withhold altogether their belief as to its existence. Mr. South says, 'I believe there seems good reason for believing this accident never occurs. I believe there is not any existing specimen of fracture of the neck of the bladebone.'" (p. 90.)

There is much truth in these observations, which might suggest a homily on the utter futility of many professional contests. Sir James Stephen, in his essay on the Founders of Jesuitism, says: "If men will join in the war of words without the same common intuitions, they may discharge their dialectics against each other till the day of doom, without making any approach to the same common conclusions." The first point to determine is, what *is* the neck of the scapula. If the complete separation of the glenoid cavity entire from the rest of the bone is to be called a fracture of the neck of the scapula, most surgeons will take the liberty of doubting the existence of such an accident, until they see a preparation, which it will not be very easy to find. Of course a musket-ball may break up the glenoid cavity, and detach it more or less from the scapula; but it is a mere quibble to call such an accident a fracture of the neck of the scapula. The following case approaches nearer to a true fracture of the cervix scapulæ than any other we have met with.

> "A moderately tall man, aged 44, and apparently healthy, being in a state of intoxication, was thrown out of a cart, and he seems to have fallen on the back part of his head, and also on the right shoulder. His most serious injury was of the head. A transverse fracture of the scapula was discovered by Mr. Cock, of Guy's Hospital, with some difficulty; crepitus was not felt on pressing its processes and rotating the humerus, but the hand, on the inferior angle, detected grating. The roundness of the shoulder was *not lost*, but there was an incomplete dislocation of the outer end of the clavicle upwards. The figure-of-eight bandage was applied with pads in the axilla. He survived only eight days. The coraco-clavicular ligaments were not torn through; the injury to the scapula consisted of separations between the spine, with a considerable part of the supra-spinous fossa, and the neck, and the inferior portion of the bone. A line of fracture ran from the posterior costa along the upper side of the base of the spine, and another from the superior costa,

leaving a little of the supra-spinous fossa attached to the ce
inch of the foundation of the root of the spine. This line te
inch and one third from the glenoid cavity, in a third line,
near the inferior edge of the glenoid cavity, more than half wa
angle, tending to isolate the thick edge of the inferior costa."

A well-executed drawing illustrates this case; the
which it is taken being preserved in the museum
There is nothing calling for notice in Mr. Callaway's s
of the acromian and coracoid processes of the scapula.

He has seen a case of that rare accident, displacem
angle of the scapula from under the border of the latiss
and thus speculates on the causes and treatment of the

"I think it is a question worthy of consideration, whether
at all upon the muscle having an origin from the angle of the
think this circumstance is a predisposing cause, and would be
the accident, inasmuch as these fibres, taking their *point d'*
circumstances, from the insertion of the muscle, would act
bone, and draw it upwards and over that portion of the m
dorsal spines. It would occur, I conceive, in weak, delicate
have a case, whose reduction I was unable to effect, I should
by a subcutaneous section, the obstructing fibres of the mu
patient ultimately suffers but little inconvenience from the
deformity would be very apparent; in women who wear stay
be pressed upon and somewhat controlled; still a very trou
form between the bone and the superimposed integuments
trouble and inconvenience." (pp. 98-9.)

Pursuing the same plan, we shall pass in review Mr.
of *Dislocation of the Shoulder.* The principal caus
that exists on this subject, is the want of a definite no
to describe the same accident; but this defect is n
England as on the Continent, where French surge
laboured to complicate instead of simplifying the su
M. Malgaigne, dislocations of the shoulder are exceed
years of age; from 15 to 45 they form nearly one hal
of dislocations of the body; and after 45, during th
proportion is still more increased. They most com
the ages of 45 and 70; are very rare in women befo
prevalent in winter than in summer. These statemen
approximating to the truth, yet they are probably pr
Mr. Callaway divides these dislocations into an
which are thus determined: If a line be taken from
and let fall, so as to bisect the glenoid cavity, from
then continued along the scapular head of the tr
boundary, in relation to which all dislocations
posterior. These are again subdivided as follows:

A. Anterior.	B. Post
α. Axillary.	α. Su
β. Subscapular.	
γ. Subclavicular.	

The axillary and subscapular dislocations are ne
nerally the limb seems lengthened, though measureme
is not always the real state of the case. The followin
to our author, is invariably present, and forms a grea

> fold of the axilla, be measured and compared with the sound one (usually about eighteen inches and a half), a great increase of about two inches will be found. This is an *invariable concomitant;* for the head of the bone, go where it will, must distend and displace parts, augmenting the depth of the shoulder and the breadth of the base of the axilla." (p. 109.)

Mr. Callaway gives an ingenious explanation of the little comparative lengthening that exists in many of these dislocations; but as this cannot be made clear without the aid of a diagram which illustrates it, we must pass it over. The methods of reducing this dislocation are such as are ordinarily recommended, and do not call for further remark. We wish, however, to draw the attention of our readers to the plan advocated by Mr. Vincent, in his late volume, which contains sterling contributions to almost every branch of surgical practice. We can assure them, that they will be surprised with the facility with which it enables us sometimes to reduce the most unpromising dislocations of the shoulder.

As for the time which may have elapsed since a dislocation, Mr. Callaway would not make an attempt to reduce one after *ninety days;* but he observes, that in all cases the surgeon would be guided much by the wishes of the patient. No absolute rule can be laid down; every one must have observed the ease with which some old dislocations are reduced; and the extreme difficulty attending the replacement of very recent ones. In our own experience, chloroform has not facilitated the reduction of old dislocations to the extent that might have been expected; and has thus confirmed an opinion formed long ago, that muscular action is very often but a secondary obstacle.

We are surprised to find Mr. Callaway recommending such a filthy practice, as applying lint dipped in blood to close the external wound in a compound dislocation. Sure we are that blood outside the body is a great deal better removed from the neighbourhood of a wound, where it often putrefies and occasions serious mischief.

The brachial plexus of nerves often suffers injury in dislocation of the humerus. An instructive example is related from the practice of Mr. Cock, but it is too long for transference to these pages.

Injuries to the longer head of the biceps. Mr. Callaway believes that the accident which is sometimes described as partial dislocation of the humerus forwards, is the result either of dislocation or rupture of the long head of the biceps muscle. The latter injury is more common than dislocation of the tendon. It happens in old debilitated persons, usually midway between the glenoid cavity and the anatomical neck of the humerus; and like the dislocation, is accompanied by lateral and posterior flattening of the shoulder, and by undue prominence of the head of the bone anteriorly. With regard to the condition of the tendon on dissection, our author thus expresses himself:

> "The tendon is usually found in one of three conditions: firstly, lying loose in the joint, and presenting a fringed cellular edge; secondly, closely adherent to the capsule of the joint; or thirdly, inseparably attached to the neck of the humerus and its bicipital groove. In both these latter cases the capsule of the joint is much thickened, as though it had become strengthened by the fibres of the tendon. The morbid changes which take place in and about the articulation, as a consequence of the injury, appear to be two—first, a coalescing of the capsule and synovial cavity of the shoulder-joint with the bursa between the capsule and the acromion, or, to speak more correctly, the formation of a false joint between the head

of the humerus and the under surface of the acromion; secoı caseous bones, in which there is a deficiency of phosphatic d(place in the head of the bone, the anatomical neck slides sligh in old people), and assumes a rather more lateral aspect th inner shell of the bone passes upwards and inwards, and pe: This, I think there can be no doubt, is the result of the up the acromion process. In Sir A. Cooper's case, the tendon i: With every possible submission to so high an authority, I meant, that it had become united to the capsule of the joint. is not so much affected by the change in the joint as would b ally it is slightly atrophied, but the muscle soon again recove tone. Should there appear to be much wasting of it, the su to decide upon the probability of its dependence upon some circumflex nerve. In this case, sensation over the posterior over the insertion of the deltoid would be much impaired."

In dislocation of the tendon, it may be thrown in part of the joint, or may be lodged either in the greate of the humerus; and it is to be regarded as a more s rupture.

The treatment of both these accidents is the same, gative. Entire rest to the joint at first, taking care forward; and subsequently passive motion, when a mation has subsided, constitute all that it is in our pc

Fractures of the head of the humerus. This, the book, though sensibly and well written, does not call The usual division into fracture of the anatomical an their occasional complication with dislocation, is ad(He also adds a third, of fractures implicating either or or in any way comminuting the head of the bone; o examples are recorded.

Mr. Callaway concludes his modestly-written and some sensible remarks on the deficiency of an accur reports; and expresses the regret in which all will mass of knowledge that is daily running to waste ii the want of some one to collect it.

Art. XVII.

The Treatment of Rheumatic Diseases by Lemon-J
Cases from Hospital Practice. By G. Owen Rei
London, 1849. 8vo, pp. 38.

In this little pamphlet Dr. Rees has brought b(profession the beneficial effects of lemon-juice in acu Dr. Rees states that in these cases, lemon-juice four or five times a day, appears to be equally effic or even more so; it produces an equally rapid red frequency of the pulse, and ensures an earlier relie of these assertions, eight cases of rheumatism a quently Dr. Rees proceeds to explain the mode in may be supposed to act. The cases seem to have be(in one case only does there appear to have been an in that case (vii) there was only a slight systoli

The relief to the urgent symptoms seems to have been rapid; although, on adding up the number of days in hospital, we find the average duration of the malady to have been about the same as under other modes of treatment, viz. rather more than twenty-four days; the longest time was forty-seven, and the shortest twelve days. The most marked effects were on the pulse, which often was lowered six, eight, or ten beats in twenty-four hours. The influence on the urine was not so decided; the solids excreted in twenty-four hours were in one case augmented during its use, in another diminished; the urine was generally acid, and in one case became acid under the use of lemon-juice, although it had previously been alkaline. The acid perspirations do not seem to have been affected. No examination of the blood, or analyses of the urine, appear to have been made; and the cases are so briefly detailed, that we do not learn much more from them except the main fact that the patients got well while taking lemon-juice. We cannot help remarking, that we are sorry Dr. Rees should not have been a little more minute in his chemical observations; for in regard to the pathology of rheumatism, the examination of the blood, the urine, and the sweat is the great subject for inquiry; and probably there is no man in London who is more competent to undertake this investigation than Dr. Rees. We must confess to no little disappointment when we found those important topics so briefly treated in the pamphlet before us.

Dr. Rees's theory of the action of lemon-juice appears to be founded on the notion of Liebig, that urea may be formed from the oxidation of uric acid. Dr. Rees seems to consider retained uric acid as the *materies morbi* in rheumatism; and thinks that the citric acid (C_4, H_2, O_{14}) of the lemon-juice yields oxygen, which, with the elements of water, converts uric acid into urea and carbonic acid. Nothing, however, is more doubtful than this Liebigian hypothesis; and one of Dr. Rees's own colleagues has pointed out its probable fallacies. We would suggest to Dr. Rees an accurate determination of the absolute quantity of urea and uric acid excreted in acute rheumatism; it is evidently a condition of his hypothesis that the quantity of urea shall be relatively much below that of the uric acid, since the mode in which the uric acid comes to be in excess, is by reason of the failure in the process by which, according to Liebig, it is converted into urea. But we shall not be surprised to find analyses proving the urea to be even in excess in rheumatism, either in the blood or the urine. Again, to prove his position Dr. Rees should have brought forward evidence to show an increase in the quantity of urea after the use of lemon-juice; but this has not been done.—We should, however, say that our author expressly states that he considers his hypothesis extremely doubtful, and attaches no importance to it; while he earnestly calls the attention of the profession to the practical fact of the utility of lemon-juice in rheumatism, from whatever cause this good effect may arise. We also attach much more importance to this fact than to its interpretation; and from some little experience of lemon-juice in rheumatism, we are inclined to agree with Dr. Rees in believing it to be of some efficacy, although the amount of this has not been so great in our cases as in those of Dr. Rees. But with the strong opinion which has been given in favour of lemon-juice by Dr. Rees, we have not the slightest hesitation in urging our professional brethren to give it a fair trial in rheumatism, in the belief that it will be found at any rate a useful auxiliary, if not an absolute and certain specific.

PART SECOND.

Bibliographical Notices.

ART. I.—*An Essay on the Diseases of Young Women.* By WALTER JOHNSON, M.B., Medical Tutor, Guy's Hospital.—*London*, 1849. Post 8vo, pp. 256.

THE title of this work affords by no means a sufficient indication of the nature of its contents; for among all the disorders to which young women are peculiarly liable, it only treats of two, namely, Chlorosis and Hysteria. The author gives us, in his preface, as a reason for the production of this Essay, that "although the peculiarities manifested by young females, as a class, have been frequently dwelt upon by medical authorities, yet that these peculiarities have always been discussed as isolated problems, and never brought together nor systematized;" in consideration of which he has "thought it not unprofitable to collect a variety of cases, illustrative of each affection that presented itself, to form a basis upon which, at some future period, a comprehensive theory may be constructed." We infer from the first part of this statement, that Dr. Johnson is unacquainted with Dr. Laycock's valuable treatise on the 'Nervous Diseases of Females,' which embraces both a practical and a philosophical view of the whole subject; not to mention other works of inferior merit. But even supposing that no monograph of this kind existed, it is scarcely to be expected that a mere collection of cases, brought together from the writings of different authors, could be of much service to the professional reader; either in aiding him in that very difficult task of discriminating between the proteiform manifestations of the hysterical diathesis and the indications of serious disease, which often baffles the most experienced practitioner; or in throwing any new light upon the real nature of the disordered condition of the system whence these manifestations proceed. It might be all very well for Dr. Johnson to form such a collection of cases as a basis for his own study of the subject; but we cannot but regret that he should have been so ill-advised as to put them forth to the world in their present shape, which is such as more resembles a "bait for practice" than a scientific essay. In fact, the whole book has the air of being addressed to the public much more than to the profession. Thus, for example, the description of chlorosis is almost entirely confined to the changes in the external appearance and habits of the patient. To illustrate the perversion of appetite, which is a frequent, though by no means a constant, symptom of this complaint, Dr. Johnson has recourse to the letter wherein "Sabina Green, now Sabina Rentfree," described to the "Spectator" her succession of cravings, which occupies no less than three pages of his book; whilst all that he tells us about the state of the blood is, that it is "poor and watery." Not a word is said of the results of exact analysis; nor do we find any more specific directions in regard to the treatment,—which is noticed, in combination with that of hysteria, at the conclusion of the work, these two diseases being regarded by the author as "varieties of one disease,"—than that "if the patient exhibit marked pallor of countenance, indicative of the absence of iron from the blood, it may not be imprudent to administer a supply of that metal."

The collection of cases of hysteria, which forms the chief part of the work, has not even the merit of completeness or discrimination; and several of those strange aberrations, on which, from the author's preface, we should have expected him more especially to dwell, are altogether unnoticed. In his original matter he frequently displays a singular mixture of bad taste and good judgment; the former being displayed in his manner, the latter in his meaning. As a sample of both we shall quote the following passages, of which the first, relating to the causes of hysteria, is written in a style on which we should hope that none of our readers will look with any favour:

> "But the grand cause of hysteria—that which puts out the eyes and lames the limbs, and distorts the features of the young and beautiful; that which prompts the canine bark, obstructs the breath, and wrings the brow with anguish: that which melts the women of England into powerless babes, lulls them into months of slumber, deforms the moral beauty of their souls, and shatters their intellect; that which stretches them moaning and struggling on the ground, or petrifies them into living statues; that which will sometimes freeze every faculty of soul and sense, and, by destroying reason, level them with those that chew the cud—this grand traitor and foe to humanity is Polite Education. First, the boarding-school, then the saloon, the theatre, the opera—these are the focus of infection, the very den of hysteria. At school, the unhealthy confined life, the premature tasking of the mental powers, that excessive application to music, lay a foundation which is consolidated upon the young lady's entrée into society. What occurs then? Why a constant round of toilsome pleasure: like a squirrel in its wiry box, the maiden makes much motion, though but little progress. The waltz, the polka, a flood of music, enrapturing every faculty, a concourse of people with mirth in their faces, brilliant dresses, rapid vivacious conversation, the thus and frankincense of flattery —oh! it is delightful, ecstatic, divine! Yes, little bewildered one, it is divine, if Jupiter still sits upon Olympus, and pagan deities breathe impurity into our hearts, and receive adoration. Listen to the heart in thy bosom, how it throbs; question thy conscience, how it trembles. Hope, fear, joy, ambition, self-adulation, rebellion against authority, and a thousand conflicting emotions, disturb the serenity of thy mind. 'He is my slave,' says Vanity. 'He is a Crœsus,' adds Avarice. 'He is odious to me,' whispers Truth, 'and I cannot forget another.' 'Can I forego a coronet?' asks Ambition. 'How they will envy me!' sighs Pride. Alas! upon no sybarite-bed lies she whose body is enfeebled by nights of dissipation, and whose mind is polluted by the mercenary schemes and lax morality of the gay world." (pp. 232-4.)

We could quote many pages equally inflated in manner, and commonplace in matter with the preceding. But, in mercy to Dr. Johnson, we shall forbear, and shall substitute a passage which will show how much better he *can* write, when he is thinking of his subject, instead of the attractiveness of the garb in which he desires to present it.

> "Perhaps the most important feature in the treatment of the maladies of young females is that which I fear is most frequently overlooked or neglected, viz. the gentle consideration and tender sympathy of those about the patient. I do not wish to have it inferred that a patient is absolutely to domineer unrestrainedly over

a whole family, nor to display unchecked the wild caprices of her malady. The observance of a due medium between stern coercion and unlimited indulgence constitutes, perhaps, the great difficulty which we have to encounter. It would be as injurious to the patient to allow an hysterical female complete license, as it would be oppressive to her friends. What we have to do is to restrain her from indulging in the weakness or violence of her malady, by a certain moral influence. Remembering that almost ever the disease originates in a preponderance of the impulsive over the reflective faculties, our task appears to be to restore this lost equilibrium. This, in fact is an arduous enterprise; it involves the attempt to re-educate an ill-educated mind; to weed from it that false philosophy in which it has been entangled; to implant clear and just ideas of life and of duty; to elevate and purify the soul. Now, to effect this grand object, it is needless to point out, that a mere series of moral admonitions, come they from whom they may, or abstract disquisitions on the 'whole duty of woman,' are quite unserviceable, and even calculated to disgust those whom they are intended to benefit. It is evident also that the first point which must be gained by any one who imposes on himself the task of healing the afflicted mind—the first point, and the most difficult—is to acquire the esteem and affection of the patient. Unless this first step be satisfactorily made, all is hopeless; but this step once made, the onward path becomes less thorny. The benevolent Mentor has then to study the mental peculiarities of his scholar, and shape his course accordingly. He will probably, by various means, according to various circumstances, endeavour to fill his pupil's mind, to present to it some object or objects which may worthily employ the idle faculties. He will, perhaps, engage them in the contemplation of the wonders of Nature. Thus ennobling her mind, and raising it above the pettinesses by which it was once agitated, he will infuse serenity into her bosom, impart vigour to her intellect, and enable her gradually to master the turbulent passions to which she was lately a prey. However, not to all dispositions are such lofty thoughts adapted; the discerning instructor will regulate his course in accordance with individual temperament. He will seek to occupy the mind in some steady pursuit, and to inspire the soul with an ambition to excel in some province, however lowly. For nothing is more pernicious than to wrap one's self in one's own feelings, to luxuriate without reserve in happiness, or to meditate uninterruptedly on misfortunes. It is this self-involution which constitutes the hysterical mind—it is this Hindoo paradise from which we would liberate the captive. Hence, therefore, we should endeavour to tear the patient from herself, and to absorb her thoughts, were it even by Berlin wool." (pp. 252-55.)

We would earnestly counsel Dr. Johnson to write no more books, until he has acquired, by observation and study, some knowledge which may be communicated to the profession with real advantage.

ART. II.—*Confessions of a Hypochondriac, or the Adventures of a Hyp. in Search of Health.* By M. R. C. S.—*London,* 1849. Post 8vo, pp. 310.

THE conception of this book, which will be readily understood from its title, is excellent. The supposed "Hyp." is a merchant who has acquired an ample fortune by forty years of hard labour, and who then retires from business, "intending to give his spirit a life-long holiday," but without any objects of interest, domestic or social, scientific, literary, or æsthetic. He found the ordinary pleasures of life unattractive as soon as their novelty had worn off; and like the hero of that admirable comedy, 'Used Up,' whether he looked up the dome of St. Peter's, or looked down the crater of Vesuvius, he always found "nothing in it." He consequently falls a victim to ennui, and first has recourse to the "regulars," who send him to

Bath, Cheltenham, Leamington, &c., drugging and dieting him *secundum artem;* until he comes to the conclusion that no good is to be expected from *them.* Of course he next falls into the hands of quacks, who soon find out what a profitable patient they have got hold of, and manage to fleece him accordingly. He goes the whole round of the various forms of empiricism, catching at each new promise of cure as if he had never before been deceived; until at last his easy credulity receives a check from the discovery that he has been made the victim of a regularly organized system of imposture, of which his trusty valet has been the accomplice. Just in the nick of time, when he is in despair of relief, business brings him in contact with one of the intimates of his youth, a rural physician, who invites him to his cheerful home, the influences of which gradually dissipate his ennui, and restore him to health and happiness.

Such is a sketch of the story, from which our readers may judge of its plan; with regard to its execution we must confess that we cannot speak in as favorable terms as we should have been glad to do. Such a *jeu d'esprit* ought to be lively enough to carry the reader along without weariness; and where the scope of the treatment is limited by the constant prominence of one object, it ought not to be too fully amplified. The book would have been more readable if about half its present length.

ART. III.—*Clinica Quirúrjica, ó Consideraciones sobre el Estudio práctico de la Cirujia.* Por el Doctor RAMON FERRER Y GARCES.—*Barcelona,* 1849. 8vo, pp. 258.

Clinical Surgery, or Considerations on the Practical Study of Surgery. By Dr. RAMON FERRER Y GARCES.—*Barcelona,* 1849.

THE work before us is addressed principally to the pupils of the author attending his lectures in the medical school of Barcelona, which is one of the leading provincial medical colleges of Spain. It commences with some observations upon the importance of clinical or bedside study, and a short historical account of the progress of clinical tuition. The merit of establishing the first ward expressly for this purpose is awarded to Padua, a clinical ward having been opened in an hospital of that city in 1578. Le Boe, at Leyden; Boerhaave, and Van Swieten, adopted the system with such success, that it was followed in the schools of Paris, Montpellier, and most of the principal hospitals of Europe. But in Spain, from the time of the Arabs,

> "Seville, Toledo, and Cordova possessed celebrated schools and hospitals, to which many young physicians from other European countries repaired for instruction; yet, in the midst of such copious sources of knowledge, scarcely any clinical study was carried on; since, far from endeavouring to obtain a profound knowledge of diseases, discussions were raised upon the most frivolous subjects, and interminable questions excited about them." (p. 24.)

At present the author laments that clinical tuition in Spain is far behind that of France, England, or Germany; but states that clinical establishments are numerous, and that the royal colleges of medicine and surgery of Madrid, Cadiz, and Barcelona, of medicine, surgery, and pharmacy at Pampeluna, with various other medical schools in different large towns, have given of late years a promise of great good to the profession in the

Peninsula. The causes which diminish the value and utility of clinical instruction, and the means of removing them, are sensibly treated; the author insisting that the number both of patients and students must be limited to prevent the danger of imperfect observations and confused impressions, and that in every medical school professors should be appointed for the express purpose of clinical tuition.

The following chapters are of no great interest; their subjects are diagnosis in general, and the method of examining the different organs of the head, neck, chest, abdomen, and extremities.

In his own classes, the author has been in the habit of assigning certain patients to a party of three or four students, one of whom is called the observer, who records and narrates the case before the others, who are expected to offer their opinion or suggestions upon what the observer has done or stated. The effect is said to have been excellent, a great spirit of emulation having been excited, the little controversies often showing extensive research and close observation. The author also proposes to unite the most diligent clinical students into a species of society of emulation, in order to collect histories of cases treated by different professors. This was done with great success by Corvisart, and except that the members are more advanced in the profession, is the principle of the Medical Society of Observation, which has done so much under the direction of Louis.

A long chapter follows on prognosis in general, and in particular in the cases of wounds, ulcers, fistulæ, dropsy, herniæ, aneurism, fractures and dislocations, and tumours. It is rather common-place.

Among some remarks which follow upon the treatment of surgical disease, the following may not prove inapplicable beyond the Spanish frontier.

> "The practitioner should not be above examining if his prescription be well or ill prepared, both to assure the result of the method he employs, and to avoid the injury which might be produced by a badly-prepared medicine; yet I esteem it very improper and shameful that a physician or surgeon should establish a shop to distribute medicines to his patients, or the drugs which compose them." (p. 240.)

A few observations on the best method of writing histories of cases, concludes this work, which, although it might be considered among us as common-place in the extreme, yet is not without its interest as a sign of the times (the *Medical* Times of course,) in Spain, and will, doubtless, prove useful to those to whom it is addressed. We have noticed it principally to show what is doing by our Spanish friends.

Art. IV.—*Practical Observations on the Prevention, Causes, and Treatment of Curvatures of the Spine; with Engravings and Woodcuts, illustrative of the Cases.* By Samuel Hare, Surgeon. Third Edition, revised and enlarged.—*London,* 1849. pp. 245.

Mr. Hare's book, although sensibly enough written, contains nothing but what every one knows; and in the whole 245 pages it would be hard to find a single new fact, or even a single new suggestion. Who, for example, doubts the necessity of cleanliness both to children and adults? who wants to be told that it is a good thing to sponge a child morning and evening with tepid water, and then to wipe it dry? and who questions that a "handful of salt" dissolved in the water makes it more stimulating? Again, who is ignorant that stays are very often badly made, and as often laced too tight? and whence the necessity of such a paragraph as this: "Cleanliness and temperance are equally essential to the enjoyment and preservation of health, as fresh air and active exercise." (p. 41.)? The wonder to us is, how people can be found willing to undergo the repeated infliction of such a dose of twaddle; one would think the author himself must be tired of it by this time.

With respect to the treatment of distorted spine, it is just the same old story over again. The spine does not become crooked without a cause, and without a considerable impairment of the general health; and if Mr. Hare's beautiful plates—for they *are* beautiful—show nothing else, they at least prove how wonderful is the change that a little rest, combined with a little gentle mechanical treatment, strict attention to diet, and a little judicious physicking, are capable of producing in the worst of these cases. So far so good; but now the question rises in the mind—what is the object of the book? It cannot be to tell the profession what every member of it already knows; and there is but one other solution,—it must be written for the public, and to catch patients. Even the artist has combined in this object with Mr. Hare, and has helped to bait the hook daintily. Witness plates 9 and 10. For a long time we could not make out what had changed a seemingly idiotic boy into an intellectual-looking youth, there being something in the metamorphosis far greater than the difference between a straight and a crooked back; but after much study we discovered how a little shortening of the lips, a little addition to the forehead, and, above all, a *different mode of arranging the hair*, effected a change, in the hands of our skilful limner, that was quite astonishing, and must have been most gratifying to the friends of the young gentleman.

Then, again, sometimes the patients tell their own stories. There is one lady who must be a perfect pathological phenomenon. Case XI.—1816, very ill and confined to bed for some weeks; 1817, bore a child; 1818, another child, and in the most imminent danger; 1820, an inflammation of the lungs; continued very ill until 1822, then had another attack of inflammation of the lungs and bowels; 1825, "a severe complaint in the bowels;" 1826, an inflammation of the brain; 1828, bore a fourth child after a most distressing labour; and now she had the good fortune to come under the care of Mr. Hare, and got quite well.

Besides this, our author does other wonderful things. In caries of the vertebra, he induces the bones to be kind enough to become anchylosed in the *straight* position, instead of the crooked one, which is the result with which most of us are simple enough to be contented.

There is one charming feature in Mr. Hare, and that is, the delicacy with which he tells us of his patients' improvement. It would not do to say they *got fat*—that's vulgar—so Mr. Hare says, "a considerable deposition of adipose tissue took place."

But after all—jesting apart—we are very sorry to see a respectable, and, we believe, a judicious practitioner descending towards the level of the puffing tradesman; and we take leave of Mr. Hare with the same remark as was made by one of our predecessors,—"The work before us has not added anything of the least value to our previous knowledge."

ART. V.—*The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with numerous Drawings in Colour.* By ARTHUR HILL HASSALL, M.B. Part XV.—*London,* 1849. 8vo.

WE are happy to be able to announce the completion of Dr. Hassall's 'Microscopic Anatomy;' the long delay of which has not improbably led to the suspicion, that it would share the fate of too many other works published in parts, and not be brought to a conclusion until the earlier portions of it should be already antiquated. The present is a triple number, containing nine plates, and a considerable quantity of letterpress. The subjects illustrated in it are the ultimate structure of the thyroid and thymus glands, the supra-renal capsules, and the spleen; of the papillæ of the skin and tongue; and of various parts of the olfactive, visual, and auditory apparatus. Some of the delineations are very good, others but mediocre; on the whole, however, this concluding part well sustains the character of those which preceded it. We shall take an early opportunity of passing the entire work under review.

ART. VI.—*An Exposition of the Case of the Assistant-Surgeons of the Royal Navy.* By a Naval Medical Officer. Second Edition.—*London,* 1849. 8vo, pp. 28.

THE author of this temperately-written pamphlet so clearly and cogently sets forth the grievances of the class whose cause he has undertaken to advocate, and so trumphantly meets the objections that have been made to their removal, that we are at a loss to conceive on what ground a system, which is not only palpably unjust, but also (which we apprehend is a consideration of more weight with those in power) demonstrably impolitic, can be any longer persevered in. When the fact is made known, that notwithstanding the overstocked condition of our profession, the temptations of a pay of from seven to ten shillings a day, with the prospect of advancement, and the nominal rank of a lieutenant in the army, are not only insufficient to induce well-qualified young men to seek admission into the service,—Sir W. Burnett having been obliged to go a-begging for candidates even as far as Edinburgh; but are inoperative to retain the best men in it,—many quitting it for the army and the East India Company's service, and others (such as Dr. F. J. Brown, whose little tract on Naval Hygiène we noticed with approbation in our last Number) for private practice; it cannot but be obvious to all, that there must be "something rotten in the state of Denmark."

The main grievance, so far as the assistant-surgeons are concerned, consists in the degradation to which they are subjected in being compelled to herd with the midshipmen in the cockpit, instead of being admitted to an association with the ward-room officers, to which they are entitled by their nominal rank, their education, and the responsible nature of their duties; and in the want of a private cabin in which they can pursue their professional studies, or devote their leisure time to the acquirement of those other branches of knowledge which the assistant-surgeons of the navy are specially directed to cultivate. And we notice it on the present occasion, because the subject will ere long be again brought before the attention of the Legislature: and since the aggrieved parties are debarred by the rules of the service from uttering their own complaints, it is for those who feel, as we do, that the dignity of our profession is insulted, and its claims to public consideration trampled down, by the obstinacy (to use the mildest term) of the Admiralty authorities, to stir up our brethren to exert themselves in behalf of this injured class, that they may bring public and private influence to bear upon those who have the power to remove the grievance.

But we cannot leave this topic without expressing the surprise with which we have learned from the pamphlet before us, that this inferiority of rank and consideration extends into the higher branches of the medical service of the navy; for the whole Naval Medical Staff, from the Director-General to the Deputy-Inspector of Hospitals, have rank a step *beneath* that of their brethren in the army; so that Sir William Burnett does not rank with Sir James Macgregor, but with the subordinate Army-Inspectors. Nor can we omit to notice the mode in which the medical service, even of the army, is degraded by exclusion from the honours and rewards which are bestowed on those engaged in personal combat; as if the exertions of the former were inferior in merit, or less important to the success of military operations. It is surely high time that our public bodies should bestir themselves in these matters, and endeavour to convince the world of our claims on its regard, by showing that we are not ourselves insensible to them.

ART. VII.—*Ristretto Generale di tutti gl' Infermi nel Sacro Archiospedale di S. Spirito per lo scorso Anno* 1847.—*Roma*, 1848.

General Return of all the Sick in the Great Hospital of Rome for the Year 1847.—*Rome*, 1848.

FROM this official return we merely extract the numbers treated in 1847, including those remaining on the 1st of January, and those entered during the year. We also give the proportion of deaths; the difference between the number of deaths and patients treated will give the number of those who left the hospital and of those who remained on the 31st of December together. The daily average of patients treated in the Civil Hospital was 511, the deaths for the whole year 1108, exclusive of 9 medical cases which died on the day of admission. The only classification is as follows:

	Numbers treated.	Deaths.
General medical cases	13,020	899
„ surgical cases	476	81
Surgical cases operated on	34	4
In the ward for phthisis	140	95
„ scurvy	32	2
In the clinical wards	251	10
In the chronic wards	54	7

It must be observed that surgical cases are not in general sent to this hospital, another very large establishment in Rome being specially destined for wounds and diseases treated by the surgeon.

In the Military Hospital the numbers were as follows:

	Cases treated.	Deaths.
Medical diseases	4121	45
Surgical diseases	737	12
Phthisis	54	47
Cases operated on	16	0

The returns of the Foundling Hospital are as follows:

	Males.	Females.	Total.
Remaining from the year 1846 . .	992	955	1947
Received in the year 1847	507	511	1018
	1499	1466	2965

Of these, 52 were restored to their parents, 97 dismissed when they were of age to learn a trade, 788 died, and 2028 remained.

In the Insane Asylum the daily number treated was 392 on the average. There remained in 1846, 389; and 92 were admitted in 1847, making the total number treated 481, of whom 286 were males, and 195 females. Of these 53 left the asylum, 39 died, and 389 remained.

We have given the substance of this return, as it may serve as a comparison with those from other establishments, and would not be likely to meet the eye of many English readers through any other channel.

ART. VIII.—*Medical Chemistry, for the Use of Students and the Profession: being a Manual of the Science, with its Applications to Toxicology, Physiology, Therapeutics, Hygiene, &c.* By D. P. GAIRDNER, M.D., formerly Professor of Chemistry in the Philadelphia College, &c. &c.—*Philadelphia*, 1848. Small 8vo, pp. 396.

WE have expressed on various occasions our conviction that the present mode of teaching Chemistry in Medical schools is fundamentally erroneous. The student is crammed with a vast mass of matter, which never becomes of any practical use to him; whilst those portions of the subject to which he might devote himself most profitably, are comparatively neglected. In universities, and academical institutions for general education, this is not to be wondered at. The medical student sits on the same benches with other pupils of various destinations, and has no ground to expect that *his* specialities shall receive more notice than theirs. We do not see how this can be otherwise, unless a separate course be provided for the medical department; but in schools in which nothing but medicine is taught, we cannot see any difficulty in so adapting the chemical course to the wants of the student (if it be retained at all, see Vol. II, p. 311), as to make it far more subservient than it is at present to his lasting benefit. We have often sketched in our own minds the character of a text-book, which should serve as the foundation of such a system of instruction; and hoped ere long to see our views carried out,—every one being now agreed that the medical student ought to be well grounded in the elements of chemical and physical science, before he enters upon his professional curriculum, and that his attention, during the period of his special education, ought to be devoted chiefly if not exclusively to the acquirement of a truly practical knowledge of the condition of the human body in health and disease.

The work before us has evidently emanated from conclusions akin to our own; and we are well pleased to find that the views we have expressed are in harmony with the convictions of eminent medical teachers on the other side of the Atlantic. The author admits the multiplicity of existing treatises on chemistry, but considers them as not adapted to the requirements of the medical student, for whom is wanting a book which directs the attention of the novice to the intimate connexions between this science and medicine,—something which may interest his mind in the subject of chemistry, by pointing out its relations to physiology, therapeutics, and practice. Such a book Dr. Gairdner has attempted to furnish; and, we think, he has done so with a fair measure of success. "He does not lay claim to originality, but is more ambitious to be useful; and his highest satisfaction will be found in having contributed something towards the introduction of chemical science into medicine." Its general conformity with our own views as to what such a book should be, induces us to ask the attention of our readers to the following enumeration of its contents.

After a short introduction, on the General Properties of Matter, and on Specific Gravity, we come to Part I, under which are grouped together the forces which are contained in chemical action; namely, heat, light, electricity, galvanism, animal-galvanism, and affinity. All these topics are concisely but comprehensively treated; and it is evident that, on most points, the author has made himself familiar with the results of recent investigations, and that he has thought for himself throughout, instead of merely copying and compiling, after the fashion of too many writers of introductory treatises. The section on affinity is particularly good; containing as it does a lucid summary of those general views which are so important to the student, but which are stated with such unaccountable brevity in most larger treatises.

In Part II, the *Simple bodies of greatest importance* are considered, according to their arrangement in isomorphous groups. Thus we have, after Oxygen, Hydrogen, Nitrogen, and Sulphur, the *Haloid* elements, Chlorine, Iodine, Bromine, and Fluorine; and then the *Phosphorus* group, Phosphorus, Arsenic, Antimony, Carbon, Borax, and Silicon. Here the author quits the consideration of the simple bodies and their inorganic combinations, to conduct us, in Part III, to Organic Chemistry. Now we must say that we cannot see the advantage of interrupting the series of simple bodies at this point; the principal *Metals* and their compounds being brought under consideration in the last part of the volume. We suppose that this departure from the usual arrangement was occasioned by the author's desire to give prominence to the Organic division, which is accordingly entered upon as soon as the four principal elements of organic compounds have been discussed. But we do not think that this plan offers any advantage sufficient to compensate for the positive evil of interrupting the series of simple bodies, and of passing on to the most complex division of the subject, before the student has been familiarized with the simpler laws and phenomena of inorganic chemistry, which may be so well illustrated by a general view of the chemical history of the principal metals. These are passed over very cursorily by Dr. Gairdner; who seems to have too much forgotten, in his zeal for organic chemistry, how many chemical phenomena present themselves in the decompositions of the Pharmacopœia, which might be employed as illustrations of the laws of affinity, and of the special properties of the chief acids, bases, &c., with which the medical student ought to be well acquainted.

Part III treats of *Organic Chemistry*, and Part IV of *Animal Chemistry*. We do not see the justice of this method of subdivision; for animal chemistry is surely a part of organic; and has no more title to be separated from it, than has vegetable chemistry, which occupies the greater proportion of the third section. Amidst the multiplicity of materials which are now accumulated on this part of the subject, and which are grievously perplexing to those who do not attempt to master the whole science but wish to become acquainted with the most important parts of it, Dr. Gairdner has made an admirable selection; and we consider these two chapters as on the whole better adapted to the wants of the medical student, than anything else of the kind we have met with. The author is a little behind-hand in some points of physiology; thus he tells us that the bile and mucous secretions are elaborated by the agency of cells; "but that this is not the case with the urine and milk;" and that "the chyle undoubtedly finds entrance without this mechanism." These, however, are trifling blemishes in a work which is on the whole so well executed; and we shall be highly pleased if Dr. Gairdner's example be followed by some one of the accomplished chemical teachers of our own country, who is familiar with the wants of the medical student.

Art. IX.—*A Practical Treatise on Morbus Coxarius, or Hip-joint Disease; showing the Advantages to be derived from a System of Mechanical Management, for the Prevention and Cure of the Contraction of the Limb. With Cases and Illustrations.* By Wm. C. Hugman, Surgeon to the Verral Institution (for the Treatment of Spinal Disease and Distortion, &c.)—*London*, 1849. pp. 82.

This is one of that class of books which contains a certain amount of useful information, that might very well have been communicated to the public through the medium of one of the medical journals. The adoption of such a course would have ensured a much wider circulation of such parts of it as are useful; would have made known more certainly the benefits to be derived from the bed recommended by the author in the treatment of diseased hip-joint; and would have exempted him from the suspicion of "book-making" to which he is at present liable. So modest and reasonable a course would not, however, have answered the purpose. It is necessary, now-a-days, to have written a book; and accordingly this honour is not to be denied to Mr. Hugman. The work has no claim whatever to the rather ambitious title under which it presents itself,—'A Practical Treatise on Morbus Coxarius, or Hip-joint Disease.'

Considered as a "Treatise," it is wholly incomplete—indeed, the author himself does not seem to consider it in any other light; for at page 23 he says, speaking of the "Treatment of Hip-joint Disease:"

> "Few subjects have perhaps occupied a greater share of the attention of eminent surgeons at the present day; and although, during the last eight years, I have had unusual opportunities of making myself acquainted with the disease in every form and variety, I shall not avail myself of the present occasion to offer any new suggestions."

One would have thought a Practical Treatise the very work for new suggestions as to treatment, if the author had any to make. But if he had not, what then? This is a question the reader will answer for himself. The unfortunate paragraph we have quoted, fairly puts Mr. Hugman on the horns of a dilemma. Either he insinuates that he possesses information, which in truth he has not got, or he knowingly and intentionally sends forth a "*Treatise*" in an imperfect state.

Mr. Hugman is surgeon to the Verral Institution for the treatment of spinal disease and distortion; and in that capacity has employed with great advantage a peculiar couch, formerly used by Dr. Verral, and improved upon since his decease. This couch is composed of two planes, the one horizontal, the other inclined at an obtuse angle from it. The horizontal portion extends from the top of the sternum to the bend of the hips; and upon it the patient lies on his abdomen, his legs resting on the other plane which is bent downwards. The latter portion is divided longitudinally in the centre into two parts, one leg resting on each; so that, by means of a sliding framework, extension can be kept up as may be desirable. This description will make it sufficiently clear that the patient lies on his face, with the body bent at such an angle as may be convenient, and the affected limb in such a position that extension can readily be made upon it; and experience shows that this prone position, far from being disagreeable, is exceedingly comfortable either to an affected joint or an affected spine. Mr. Hugman states, and narrates cases to prove the assertion, that a moderate amount of extension exerted upon a diseased hip-joint, not only prevents the shortening, which is almost inevitable if the limb be left to itself, but also effects considerable improvement in such cases as have already undergone this change.

We have had an opportunity quite unknown to the author—whom indeed we are not conscious of ever having seen,—of witnessing the amount of comfort and benefit that his bed is capable of affording to patients with disease of the hip-joint, in a painfully protracted case of the malady, and it is therefore disagreeable to be obliged to speak thus of his book; and we shall on this account abstain from reviewing it in the ordinary sense of the term.

ART. X.—*Portraits of Diseases of the Scalp, with the Safest and most Efficient Modes of Treatment.* By WALTER COOPER DENDY, Senior Surgeon to the Royal Infirmary for Children, &c. &c. Fasciculus II.—*London,* 1849. 4to. Four Plates.

THE plates in this fasciculus are decidedly superior as works of art to those of the preceding; but still, as delineations of disease, we cannot give them any high commendation. Moreover, if the work be *completed,* as we understand it to be, in these two fasciculi, it appears to us very far from *complete;* several important phases of scalp-disease being passed by without illustration.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ANIMAL CHEMISTRY.

On the Structure and Development of the Liver. By C. HANDFIELD JONES, M.B. CANTAB.

OF this interesting paper we gave in our First Volume (p. 535) the abstract furnished by the Proceedings of the Royal Society. Its recent publication in full, enables us to add some further particulars in regard to the author's views. He states that his survey of the various forms of hepatic structure lead to the following conclusions:—1. The liver, in all vertebrated animals, may be regarded as consisting of a secreting parenchyma and excretory ducts. These two portions of the liver are not continuous with each other, but disposed simply in a relation of juxtaposition; the substance of the lobules being made up of secreting parenchyma and of blood-vessels, whilst the ducts are confined to the interlobular spaces. 2. The action of the liver seems to consist in the transmission of the bile, as it is formed, from cell to cell, until it arrives in the neighbourhood of the excretory ducts, by which it is absorbed. This action is probably slow, and very liable to be interfered with, contrasting remarkably with the kidney, where a particular apparatus is added to ensure completeness and rapidity of action. 3. The secretion of the hepatic cells is very liable to be retained within the gland, either in the cells or in a free state. This circumstance, as well as its structural peculiarities, seem to point out the liver as approximating to the class of ductless glands. For the same reason, it seems highly probable that a part of the secretion of the cells is directly absorbed into the blood which traverses the lobules. 4. In a classification of the true glands, the liver seems to occupy the lowest position, the highest being assigned to the permanently tubular, such as the kidney and testis. 5. From the condition of the secreting parenchyma in many instances, we learn that the secretory process by no means requires the formation of perfect cells in order to effect its peculiar changes; these may certainly occur in blastematous matter, if a nucleus only be present. 6. The condition of the liver is in great measure dependent on the intensity of the respiratory process; its products being unused, accumulate in the gland, often to a remarkable extent; its function is therefore not only vicarious of respiration, as formerly supposed, but preparatory, and to some extent subsidiary.

In an appendix to the paper, Dr. H. Jones thus replies to an objection which may be urged against his views, on the ground of the very small extent of contact between the ultimate ducts and the cells of the liver, which makes it difficult to understand how the bile which they secrete should be received into the efferent ducts. "In answer to this I observe:—First, that I believe much of the secretion of the cells is directly absorbed into the blood traversing the lobules. Secondly, that I think it is by no means clearly proved that the secretion of the cells is perfectly-formed bile; in many instances it clearly is not; thus in most fishes, and in the fatty liver of the human subject, it is evident that the gorged parenchyma is full, not of bilious but of oily matter, *out* of which, however, healthy bile is elaborated. I have carefully examined the cells of the human liver, in organs which were quite healthy, without finding any evidence of the presence of bile in their contents, though in congested livers the yellow molecules are often very distinct in the interior of the cells. In the pig, rabbit, and dog, the cells appear as pale granular bodies, and do not, that I can perceive, exhibit the least biliary tinge, even after the addition of nitric acid. In the sheep, there is generally a good deal of oily matter in the cells, but no biliary. I do not of course deny that bile is often found in the cells, especially in states of congestion; but I conceive that in the perfectly healthy state, the complete elaboration of bile is effected principally by the nuclei of the terminal portion of the ducts."—*Philosophical Transactions*, 1849, Part I.

[Some of these statements are rather startling, and we can scarcely reconcile them with our own observations, which have never led us to entertain any doubt that, in perfectly healthy livers, the cells of the hepatic parenchyma contain true biliary matter. Again, we must take leave to question the statement that biliary matter can be directly elaborated from oily matter; since we think that there is neither chemical nor physiological evidence to prove that such transformation is possible. On the contrary, there is much evidence of their entire distinctness of character and function; and if the blood do absorb any of the contents of the secreting cells, in its passage through the parenchyma, as suggested by Dr. H. Jones, we suspect that the matter thus taken up will be the oleaginous, rather than the true biliary.]

On the Minute Structure of the Papillæ and Nerves of the Tongue of the Frog and Toad. By Augustus Waller, M.D.

The attention of physiologists was first directed by Dr. Waller to the peculiar advantages possessed by the tongue of the living frog and other similar animals, for microscopic investigation, in the year 1839; and since that time, he has published various observations made upon its component structures. By the induction of anæsthesia by means of ether or chloroform, he now removes the objections which formerly lay against the employment of the tongue of the living frog, and is able to carry on his observations for several hours without any suffering to the animal, and without any alteration of the natural condition of the organ. We can only notice a few of the more interesting facts which he has in this manner been able to demonstrate.

The *fungiform* papillæ are described as consisting of "a circular zone of epithelian cells, containing a central area filled with coils of capillary vessels, and with nerve-tubules ascending and terminating abruptly amongst them;" the investing membrane of this "gustatory area" being always extremely thin. They are always elevated, and sometimes attached to the tongue merely by a slender footstalk. "Besides blood-vessels and nerves, we invariably discover in the interior of the fungiform papillæ numerous striated muscular fibres. They are derived from the superficial muscular layer, which exists beneath the basement-membrane of the dorsum of the tongue, and appears to be one of the essential elements of the mucous tegument of that region. They run parallel with the vessels and nerves, to which they are external, and form a complete investment. After attaining nearly to the summit of the papilla, they curve inwards, and afterwards disappear in the surrounding tissues, apparently by losing their striæ and sarcolemma, which are their distinctive characters. This mode of termination of the fibres is deserving attention, and is, I believe, the only instance in which the gradual transformation of the muscular element into any other tissue than the fibrous variety composing tendons has been discovered." Dr. Waller has noticed the existence of ciliary motion on these papillæ, and its absence on the conical, which are destitute of muscular fibres. "The action of these fibres is to shorten the papilla, probably at the same time they may compress the vessels, regulating to some extent the current of blood, and produce the turgescence of these papillæ which has been observed in the higher animals." [We do not see how these two functions are compatible, a state of turgescence being one of erection.] "The action of the cilia is very evident while under experiment. It conduces to clear away foreign bodies from the surface; to equalize the distribution of the sapid substance over them, and consequently over the nervous extremities; and to promote the removal of the epithelial scales which are constantly being shed." The nerves of the fungiform papillæ are described and figured by

Dr. Waller as terminating in free extremities; the tubule sometimes ending in irregular points, sometimes in a club-shaped dilatation, sometimes in a spiral form, and sometimes "like small funnels, but most often with a kind of concentric mouth." [We have made many observations, after Dr. Waller's method, upon this point, but have not been able to satisfy ourselves of the accuracy of this description. Many of the appearances were quite compatible with the idea of a return of the nerve-tubes by loops; in other instances it seemed to us as if, though the sheath ceased, the central axis of the fibre was prolonged.]

The *conical* papillæ are described as possessing an opening at their summit, which "is either sharp at the edges, or anal-like, with circular lips. From above we see the commencement of a cavity lined with epithelium, which I have been able to see terminate in an infundibular canal extending towards the base of the papilla. These papillæ generally contain no vascular capillaries within them. When they do exist, they never ascend to the summit of the body, but form a bend or loop at about the half or lower third of the height of the cone." The nerve-tubes are difficult to distinguish in the conical papillæ, owing to the thickness of the epithelium; Dr. Waller has succeeded, however, in demonstrating that, as a general rule, "the nerve-tube runs close to the aperture of the papilla, around which it forms loops, after which it runs away in a wavy direction. Often at each angle of the aperture is a nerve-loop of this kind, formed by separate tubes, besides others which are seen running in a meandering course, and crossing the former in various directions. The space inclosed by these nervous loops is much darker than elsewhere, as if it contained some dark granular matter. The tubes never appear to terminate abruptly in free extremities."—*Philosophical Transactions*, 1849, Part I.

On the Production and Disappearance of Sugar in the Animal Economy.
By M. MAGENDIE.

Production of sugar in the economy. After describing the various modes in which sugar may be detected in the animal economy, M. Magendie observes, that, although it had been long known that sugar was found in the urine, the mode of its formation only became known when the formation of that in vegetable bodies was discovered. If starch, derived from potatoes or other bodies containing it, is brought into contact with acids, sugar is found, and the same takes place if a ferment is added to it; *dextrine*, and then *glucose*, being formed in this latter case. This result is due to an active principle, found by MM. Payen and Persoz, in germinating barley, and called by them *diastase*, on decomposing which we obtain nitrogen, as well as oxygen, hydrogen, and carbon. How does this body separate the dextrine from the starch? How does it convert it into sugar and alcohol? It is a mystery; and to merely state that it is by catalysis, is only to avow our ignorance. We must therefore content ourselves with studying the phenomena. Thus, if we add iodine to a solution of starch, we produce a violet colour. If, in the same solution, we place a little diastase, the colour becomes rose, and, a little later, red—demonstrating first the existence of dextrine, and then that of sugar, which is afterwards produced.

Are there not, in the animal economy, analogous conditions to these, in which diastase is formed. The recent discovery of sugar in the economy by M. Bernard renders the inquiry one of the greatest interest. So many substances of the animal economy possess this power, as well as diastase, of transforming starch into sugar, that the greater difficulty would be, to point out those that do not possess it.

In respect to the action of the *saliva*, there are some distinctions to be observed. First, its quantity varies much in different animals. In the horse, for example, whose food requires prolonged mastication, it exists in very large quantities; there is but little in the dog and most carnivora, and hardly any in the cat. Again, the question whether the three salivary glands all furnish a similar fluid, has not been hitherto determined. The property of converting starch into sugar by the action of the animal diastase, is a means of deciding this. In the horse, a large alimentary bolus is formed, which remains long in the œsophagus, and the transformation of the amylaceous matter of its food may be ascertained if we tie this canal. But if, by an opening into the parotid duct, we remove the fluid of the parotid gland, before it reaches the mouth, and place it in contact with starch, no such conversion into sugar occurs. It is the same, also, with the saliva drawn from the maxillary ducts in the dog. There is, then, in the buccal saliva something peculiar, dependent upon all its constituent principles. In fact, we have the fluid of the three glands united, mucus, atmospheric air, the absorption of oxygen, and a tendency to produce carbonic acid. The *pancreatic fluid* of itself effects the transformation.

Can the *gastric juice* convert alimentary bodies into sugar? It is a singular circumstance, that sometimes starch is promptly thus transformed, and sometimes not at all. The transformation takes place if the juice is alkaline, like the saliva, but not if it is acid. If the acid gastric juice does not transform starch into sugar, it possesses the power of dissolving azotised aliments, such as meat. This difference in the juice, according as it has to act upon amylaceous or azotised substances, is one of M. Bernard's important discoveries, whence therapeutical applications may flow. As the acid of the stomach is the lactic, it is this we should prescribe for individuals whose digestion of animal food is difficult; while alkaline drinks are proper for those who digest vegetable aliment with difficulty.

Starch is also transformed into sugar in the *intestinal canal*, as shown experimentally by M. Magendie. The same change takes place when it is placed in contact with a solution, an infusion, or a decoction of the brain, spleen, pancreas, &c. The urine itself can effect this transformation. So, too, if starch be introduced by injection into the blood, it is converted into sugar. Thus, almost all parts of the body may contribute to transform alimentary starch into sugar; but the *liver alone* has the property of producing sugar *without* starch, as shown by M. Bernard's investigations. This same distinguished chemist has more recently shown that this production of sugar by the liver is under the influence of the nervous system. M. Bernard, after several experiments, discovered that, if the floor of the fourth ventricle is pierced within a very circumscribed space, in less than half an hour a very considerable quantity of sugar was found in the blood and urine, without the regimen of the animal having undergone any change whatever. This curious fact has naturally directed attention to the condition of the floor of the fourth ventricle in diabetic patients; and, in a recent autopsy, two dark spots, at the place where the part must be penetrated in order to produce the sugar, were observed. M. Bernard has been enabled to produce the same phenomenon in another manner, viz. by pricking or gently galvanising the eighth pair in the neck; but the increased secretion so produced is of short duration. Another experiment, by its negative results, affords additional proof that this secretion of sugar is under the influence of the nervous system. The two nerves of the eighth pair are divided in the neck of a rabbit; and, if the animal survives the operation for some hours (which is necessary, in order that the sugar already formed in the liver may pass away into the circulation), the liver no longer, however treated, offers any traces of sugar.

The transformation of amidon into sugar, as also the natural production of sugar in the liver, appears, then, to be one of the great functions of the economy,—one of the true conditions of existence. In late times, animals have been denied the property of producing sugar or fat, these bodies existing ready formed in the aliment. All now stated demonstrates, however, that the animal machine is not only a true sugar-manufactory, but that it may even produce it without requiring even the alimentary starch for the purpose of conversion.

Disappearance of sugar from the economy. In proportion as sugar is formed in the liver, it is carried away by the venous and arterial currents which traverse that organ, and it should therefore be found in the vessels proceeding from it; and M. Bernard has found it in the supra-hepatic veins, in the vena cava superior, and in the right cavities of the heart. But in other parts of the body the blood contains no sugar, or only very feeble traces of it. It is found neither in the veins constituting the vena cava inferior, in this trunk itself, nor in the splenic veins, and scarcely any traces are found in the blood of the veins returning from the head. Nevertheless, for about five hours *after the digestion of amylaceous substances* a notable quantity of sugar may be found in all the veins. This lapse of time shows that the sugar is destroyed only gradually, and that it is necessary for it to pass a great number of times through the lungs before it disappears entirely. Since we do not find the sugar produced by the liver after it has traversed the lungs, it must become destroyed in these organs. Here, then, is an entirely new fact. Something takes place in the respiratory process which was entirely unknown. May not this be the cause of animal heat? In spite of all the ingenuity of the illustrious Lavoisier's theory, he supported it by no direct proofs; for it is not only oxygen, but also nitrogen and hydrogen, which, passing into the blood, produce carbonic acid. There is nothing impossible, then, that from the destruction of sugar in the lungs the carbonic acid of respiration may result. [This doctrine has long been taught in this country, especially since the researches of Dr. Buchanan of Glasgow demonstrated the existence of sugar in the blood, for a short time after the use of amylaceous articles of food.]

Respiration does not act in the same manner in the destruction of all species of sugar. If we inject a solution of cane-sugar, mannite, or the sugar of milk, the whole of this sugar is found in the urine; but if we inject glucose or grape-sugar, unless a large quantity has been thus introduced, we do not find any in the urine. But if the first-mentioned descriptions of sugar are not destroyed in the lungs, this is no longer the case when animals are fed by them. In this case we no longer find cane-sugar in the urine, because, by digestion, it has been transformed into glucose, and decomposed by the lungs. Under these different circumstances, sugar is always tending to disappear from the economy. The first-named species, escaping the respiratory act, are eliminated by the urine; while glucose, which is the sugar of diabetes, is destroyed in the lungs.

The following table exhibits the quantity of the different kinds of sugar it is necessary to inject into the jugular, in order that they may be detected in the urine. Thus, there are physiological differences nowise indicated by chemistry; and it will be especially remarked that the sugar of the liver, i. e. the natural sugar of the economy, is destroyed in the act of respiration with far greater facility than the sugar proceeding from alimentary substances:

Cane-sugar	0·05
Mannite	0·05
Sugar of milk	0·25
Glucose	2·50
Sugar of the liver	12·00

Applications. There are only two diseases known, in which the quantity of sugar in the economy has been proved to be increased, *phthisis* and *glucosuria.* It had been long observed that the urine, as well as the expectoration, of phthisical patients is sometimes saccharine; but a distinction must be made between such cases and those which only become phthisical as a result of diabetes. M. Bernard declares that the urine in true phthisis is not saccharine; but that the quantity found in the blood is much increased. Having observed such increase in the blood of a young girl, who had been bled, and whose urine exhibited no sugar, he predicted, although no other sign of phthisis was present, that she would fall a victim to this disease, which she did. But researches into this subject require to be multiplied. It is certain that diabetes induces as one of its consequences the most serious pulmonary affections; and now that we are aware that one of the functions of the lungs is to destroy the sugar as it is formed, we cannot be surprised at their becoming fatigued in the effort to decompose all that is formed. For this they do not suffice, and an immense quantity escapes by the urine. It is also found in the expectoration, in the matters vomited and passed by stool (M. Magendie has found large quantities in the cholera evacuations), and in the sweat. In this disease, which is characterised by emaciation, we find weakness of sight, loss of the generative faculties, sleeplessness, an excessive thirst, a voracious appetite, and active digestion, as if to furnish material for this incredible activity of fabrication; but under this terrible influence, consumption arrives, and the patient is sometimes carried off with great rapidity.

Two distinguished chemists, MM. Bouchardat and Mialhe, have recently offered their explanations of the mode of production of this affection, and suggestions as to the best means of coping with it. According to M. Bouchardat, the diabetic urine proceeds from the transformation of fecula into sugar, and the quantity of this will be found to be great in proportion to the quantity of bread and feculent aliment consumed; and he advises a modification of the animalized regimen which had already been employed by Rollo, Thenard, Dupuytren, &c. He gives also a glutinous bread, containing only one fifth of flour, and endeavours to regulate the functions of the skin by warm clothing, exercise, and the use of baths. In obstinate cases, he thinks well of the carbonate of ammonia as a medicine, only using the bicarbonate of soda when the glucose is in moderate quantity, and the urine simultaneously contains uric acid. Lime-water and magnesia, by retarding the solution of feculent matters, become useful adjuvants, being, however, of only temporary avail. According to M. Mialhe, the defective decomposition of sugar and its passage into the urine, depends upon the insufficient alkalescence of the humours; it being, in his opinion, by the instrumentality of the alkalies of the blood and of the animal fluids, that digestion of amyloid and saccharine substances is effected. His treatment consists in the administration of the alkaline waters of Vichy, and the re-establishment of the functions of the skin, the alimentation becoming then a secondary consideration. To both these views of the nature of the disease, the natural production of sugar in the liver and the saturation of the blood by it when the eighth pair are excited at their origin, or in their course, present serious objections. To M. Mialhe's view may also be opposed the facts, that the blood, only slightly alkaline in its normal state, does not become acid in diabetes, and that a more alkaline fluid than it has been found, by M. Magendie's experiments, not to decompose the glucose. The serum of the blood of the horse is less alkaline than is that of man, and yet the sugar is just as easily destroyed in the one as the other. In fact, it is not by the alkali of the blood that such destruction is effected, but, as shown by M. Magendie, by the respiratory action. M. Bernard believes the disease to be an affection of the pneumogastric nerves; but we may also ask whether, in some cases, it may not arise from some peculiar alteration in the liver.—*From a Report of M. Magendie's Lectures at the Collége de France; by M. Fauconneau-Dufresne. L'Union Médicale*, Nos. 72, 75, and 79.

On the Medullary Substance of Bones. By MM. Gosselin and Regnauld.

Most anatomists have in general terms described the existence of a medullary membrane supporting and surrounding the medullary substance; but when they enter into particulars concerning it, they declare its tenuity to be such that its existence is demonstrated with difficulty. In the present paper the authors declare that careful examination conducted by the naked eye, the microscope, and chemical tests, in a great variety of cases, have convinced them that in point of fact no such membrane does exist, the doubts already expressed by Ruysch in respect to it becoming thus fully confirmed.

The medullary substance presents very different appearances, not only in different species of animals, but even in different individuals. The modifications may be classed under two principal heads, which for distinction's sake we may term the fatty medulla and the gelatiniform medulla. In the first of these it almost entirely consists of semi-fluid, fatty substance, and of extremely delicate vessels forming a network on its surface and in its substance. In the other there is but a small proportion of fatty matter, the substance then having the appearance and consistence of a red jelly, the vascular network being here still more abundant. Both these varieties are compatible with health, the substance containing more or less fat, according to peculiar predisposition. Disease, however, exerts its influence, and under the operation of inflammation fat is absorbed, that part of the skeleton then presenting the gelatiniform type, even while the other parts present the fatty. Long-continued general disease may similarly affect all the bones; but the differences which result are so great, that no general rule can be laid down. Age, however, has a more marked and constant influence. In early life the substance is very vascular and gelatiniform, and does not present the colour of fat; but as development advances, the fatty type is gradually assumed, though not completely until after the consolidation of the epiphyses. The same appearances present themselves, whether the substance is examined in the medullary canal of a long bone, or in the cellules of the spongy tissue.

Originally the medullary canal is but a canalicule, containing a much larger artery than the other canalicules, which freely ramifies into the substance of the bone. These and the accompanying venous ramifications afterwards become mingled with some adipose cells, and with a matter, which, though not gelatine, much resembles it. As ossification goes on, this canal becomes enlarged by the absorption of its inner layers, for the purpose, as shown by M. Flourens, of affording the long bones a greater power of resistance without increasing the quantity and weight of osseous matter. In proportion as this absorption of the innermost layers of the canal takes place, the adipose or gelatiniform matter is deposited, filling up the empty spaces, supporting the vessels, and keeping an abundant vascular network applied against the internal surface of the canal; its office, though important, being purely mechanical. The fat of the bones, then, differs from that of other parts of the body, in not being divided into lobes or lobules by prolongations of the membranous areolar tissue. The cells are separated only by delicate capillaries, and are mingled with more or less gelatiniform matter. Just as the vascularity is great, the proportion of fatty matter is smaller. It would seem that the deposition of the fat compresses and causes the disappearance of a portion of the vessels. In this way, when active nutrition is required, as in the child, there are abundance of vessels and little fat, while, in the aged, there is a larger proportion of fat and fewer vessels. This connexion is observed also in disease. Thus in osteitis, the portion of the bone which is red with vessels has a less proportion of fat than the other parts, which may possess even a larger proportion than usual. So in that singular change in bones termed their fatty condition, the naked eye can detect no vessels in the medullary substance, sufficient of them only remaining to maintain the slow and incomplete nutrition of the part.

Denying the existence of a medullary membrane, the physiological and pathological actions attributed to it must be otherwise explained. We cannot say, with M. Flourens, that it effects the absorption of the inner layers of the canal, this being in reality accomplished by the capillary vessels supported by the medulla. So, too, a membrane takes no part in the formation of callus, and in accomplishing the various changes supervening upon osteitis; but the material for all these may be amply supplied by the vascular network.—*Archives Générales*, t. xx, pp. 257-74.

[We should be disposed to regard the "gelatiniform matter" of these authors as an organizable *blastema*, the incipient condition of fibrous or fibro-membranous tissue.]

On the Ganglia and Nerves of the Heart. By Robert Lee, M.D., F.R.S., &c.

With a view of confirming by analogical evidence the statements which he has made with regard to the uterine nerves, Dr. R. Lee has made several minute dissections of the nerves of the heart, the results of which are presented in this paper, or rather in the delineations which accompany it. These results confirm the statements of Scarpa, as to the existence of a large number of nervous filaments passing over and into the muscular substance of the heart, quite independently of the coronary arteries, which have their own plexuses; and also as to the existence of numerous fusiform enlargements upon these nerves. "The series of my dissections," says Dr. Lee, "shows that the nerves of the heart, which are distributed over its surface and throughout its walls to the lining membrane and columnæ carneæ, enlarge with the natural growth of the heart before birth and during childhood and youth, until the heart has attained its full size in the adult; that the nervous supply of the left ventricle is greater than that of the right, and that when the walls of the auricles and ventricles are affected with hypertrophy, the ganglia and nerves of the heart are enlarged like those of the gravid uterus."—In his last dissection of the nervous system of a young heifer's heart, he states that he has made distinctly visible to the naked eye no fewer than ninety ganglia or ganglionic enlargements on the nerves of the anterior surface only, which pass obliquely across the arteries and the muscular fibres of the ventricles, from their base to the apex. The ganglia of the posterior surface also are very numerous, but of smaller size than those of the anterior. And besides these, there are very great numbers of ganglia in the walls of the heart itself; so that, according to Dr. Lee, "it can be clearly demonstrated that every artery distributed throughout the walls of the uterus and heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed."—*Philosophical Transactions*, 1849, Part i.

[We must repeat, in regard to the nerves and ganglia affirmed by Dr. Lee to exist in the heart, that which we formerly remarked as to his statements in regard to the uterine nerves and ganglia (p. 9),—that he must not expect histologists to admit their existence, until he shall have given that proof of their nature which microscopic examination alone can yield.]

On the Size of the Eye at different Ages. By M. QUETELET.

Age.	Male.	Female.
0 to 1	19 millim.	18 millim.
1 „ 3	21 „	20 „
3 „ 5	25 „	25 „
5 „ 7	26 „	26 „
7 „ 9	26 „	26 „
9 „ 11	27 „	27 „
11 „ 13	27 „	27 „
13 to 15	28 millim.	27 millim.
15 „ 18	29 „	28 „
18 „ 20	30 „	28 „
20 „ 25	30 „	28 „
25 „ 30	30 „	28 „
30 and above,	30 „	27 „

The size is taken from one palpebral angle to the other; and it would seem that, towards the age of 5, the eye has nearly acquired its entire development.—*Annales d'Oculistique*, tom. xxi, p. 170.

On Luminosity of the Human Body. By DR. SCHNEIDER.

DR. SCHNEIDER is acquainted with a strong, healthy, dark-haired Capuchin monk, who, on removing his head-dress, always induces a number of shining, crackling sparks from the hairy scalp. Fearing that the large quantity of the good beer he was in the habit of taking had something to do with this, he diminished his potations, and the appearance also diminished somewhat, though it did not cease entirely, continuing still perceptible during an illness which prevented his taking beer for three weeks. In another case, luminous sparks were given out whenever the patient passed his urine. Nasse has collected various examples of this preternatural illumination. Marsh relates two cases, in which the heads of phthisical patients were surrounded by phosphoric light. Kaster relates one, in which light was seen in the perspiration and body-linen after violent exertion. Jurine, Guyton, and Driessen observed luminous urine after prior exertion in healthy persons; and Nasse met with it in a phthisical patient. Percy and Stokes saw a bright light emitted from a cancerous ulcer; and the breath of spirit-drinkers, it is well-known, emits it.

In the opinion of Nasse, this appearance chiefly depends upon impeded respiration, and a diminished quantity of oxygen gaining access to the lungs. This is the case with the phthisical, as also with the healthy, when a plethoric state of the lungs is induced by great exertion. Spirit-drinkers, owing to the changes wrought in the blood by alcohol, are in an analogous position to the phthisical; and it is in such subjects that spontaneous combustion generally occurs. The congestion of parts with dark blood, as in the case of ulcers, may give rise to it; as also the sweat of an over-heated skin, the urine of persons suffering from syphilis, or other cause of diminished strength. Animals that normally give rise to the phenomenon of illumination are all low down in the scale, and possess but imperfect respiratory apparatus. Much light-producing material enters into the composition of the body; and although, in the normal conditions, phosphorus, sulphur, and carbon are expelled from the frame, combined with acids, by the kidneys, yet, as in diseased states, we may find carbon in the bronchial glands, and sulphur in the abdominal flatus, so may we suppose phosphorus may sometimes remain uncombined with oxygen. Such escape of unburnt phosphorus would be favoured by whatever limits the respiratory powers and diminishes the quantity of free oxygen in the blood. In experiments upon dogs, it has been found that phosphorus, mixed with oil and injected into the blood, if time be not given it to combine with the oxygen of the the body, escapes unburned from the lungs.—*Casper's Wochenschrift*, No. 15.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the Comparative Pathology of the different Races of Men. By M. BOUDIN.

M. BOUDIN has for some considerable time been engaged in furnishing statistical proof of the erroneousness of the doctrines of acclimatization, which suppose that long residence habituates men to climates otherwise unfitted for them. He has brought forward, in his various communications, of which this forms one, ample proof of the little success and the great mortality that have attended the attempts at the colonization of Algeria. In the same way, the European has always failed in fixing himself in Egypt, and the French cannot propagate their race in Corsica. He pays a well-deserved compliment to the British government for having availed itself of a knowledge of these deleterious influences, in the more judicious distribution of its troops in recent times. Thus, by adding to the British troops auxiliaries recruited among races whose physiological and political aptitudes suited them to the respective climates, by selecting for the European troops serving in warm climates the most elevated regions, and by shortening the time of service abroad, it has effected a remarkable diminution of mortality,—a diminution which, in the most unhealthy possessions, has amounted even to 50 per cent. He contrasts this with the large mortality which still prevails among the French troops serving in analogous regions. The differences of the comparative mortality of the different races placed under different circumstances is seen by examining that of the white and the negro population. Thus, while at Philadelphia the mortality of the whites is 24 per 1000, that of the negroes is 47; that of the whites 10 years old and upwards being, at New York, 15 per 1000, that of the negroes 26. At the Eastern Penitentiary, 20 per 1000 whites and 70 blacks die; while at Weathersfield, the numbers are 28 and 100 respectively; and the same enormous disproportion is observed in other prisons. The great mortality is especially due in the negro tribes to phthisis and typhus; while the negro is so proof against malaria, which carries off so many whites, that a due observation of this fact has enabled the British government to diminish wonderfully the mortality of their troops employed in the East and West Indies. In the West Indies, the mortality of the negro soldier, compared to the white one, is but as 40 to 78 per 1000; and in Sierra Leone, it is 16 times less than that of the white. The latter are 160 times more fatally affected by fevers than the negro; and it is only in diseases of the chest that they enjoy a somewhat greater immunity (4·9 to 6·3 per 1000). Even in the most southern station of Europe, Gibraltar, however, the negro mortality (62) greatly exceeds that of the white soldiers (21·4), though these are also strangers to the soil—the diseases of the chest in the negro amounting to 43 per 1000.

In respect to the *Sepoys*, their mortality at the different stations is three or four times less than that of the English soldier; and they enjoy a remarkable immunity from diseases of the chest and liver, but are more prone to dropsies, owing to the endemic prevalence of the *beriberi*. The mortality of the *Hottentot* soldier at the Cape is very small, half of the deaths arising from diseases of the digestive organs. They, too, are more prone to diseases of the chest than the white soldier. Many facts tend to show that the *Jews*, even amidst wide-spreading pestilences, have enjoyed great immunities; and that they pre-eminently possess the power of acclimatization.

M. Boudin concludes the present paper with some observations on the diseases and mortality of the *war-horse*. In 1845, of an effective force of 33,618 horses, the French cavalry lost by death 2603, and by discharge 2511. Of an effective force of 41,793 in 1846, 2679 died, and 3314 were discharged. While the general loss by death and dismissal in France was 143 per 1000, it was in Algeria 240. In 1836 the mean annual mortality was 197 per 1000; 126 in 1841; 108 in 1842; 71 in 1843; 76 in 1844; 77 in 1845; and 66 in 1846,—a diminution constituting one of the greatest triumphs achieved by the application of sound hygienic and administrative measures. How much yet remains to be done, however, is shown by the fact that the Prussian cavalry only loses 20 per 1000, and the French gendarmerie 14 per 1000. The largest mortality takes place in the finest quarter of the year,—from April to October; while the smallest corresponds to the cold season of the year,—a fact chiefly attributable to the suspension of manœuvres during the latter period. The most frequent causes of death, both in France and Algeria, are glanders and diseases of the respiratory organs. The glanders gives rise to 10 times the mortality in Algeria to that produced in France,—a difference, M. Boudin thinks, that may be in part explained by the fact of so many mules (5695 in a force of 18,538) being employed in the former country,—the disease almost always attacking these animals in the acute form. In 1846, the Minister of War consulted 136 army veterinary surgeons, as to whether this disease is contagious. No answer was returned by 12; 24 were uncertain; 36 were non-contagionists; and 64 contagionists.—*Annales d'Hygiène*, tom. xlii, pp. 38-80.

On Nervous or Convulsive Cough. By M. SANDRAS.

THERE are several varieties of this:—1. The patient can receive no physical or moral impression, without suffering from a cough almost convulsive in its character. In examining the chest of such a person, the physician may be led into grievous error, and the unnecessary fear of incipient phthisis, unless he examines it on various occasions and under different circumstances. Patients with incipient phthisis also cough from the slightest cause; but it will be generally found that in those cases the impression is physical, while in those we are alluding to it is oftener moral.

2. Another form of cough, having some analogy to this, is observed whenever certain functions are brought into play, or when they are more actively exerted than usual. Thus, it is found in some whose meals have been too long delayed, in others as soon as they have eaten, especially if rather fully. Other persons cannot take a little extra muscular exertion without bringing on a tormenting cough of this kind. In both this and the preceding form the cough is dry and capricious, exhibiting very inconstant physical signs; but this latter form is somewhat more fixed in character than the first, inasmuch as in the same person it is always when the same function is fulfilled that it is produced; and it seems, too, to be more dependent upon disorder of the organs in connexion with the exercise of whose functions it appears; and this should be our chief guide for its treatment.

3. Another cough is observed upon the slightest irritation of the bronchi being

produced; so that the least cold brings on a convulsive cough nearly as bad as that of pertussis. Sometimes, and especially in children and very young adults, it takes on this form at the very commencement of the cold, and retains it until coction is produced. Each paroxysm is accompanied by a dry, raucous sound, and attempts at vomiting. Sometimes it is periodical, the disease only gradually assuming the characters of an ordinary ripening catarrh. In other cases the spasmodic character is only observed as the cough is drawing towards an end. Instead, however, of coction taking place, the expectoration continues frothy and transparent, and is only ejected by convulsive efforts and vomiting—the paroxysm being brought on by the slightest cause, and a state of spasmodic suffocation being almost induced, until a little transparent and frothy matter is expectorated, when all becomes quiet and normal until a new paroxysm. In some cases the cough suddenly ceases, without the expectoration having undergone any change; but this is rare. The causes of this pertussoid cough are not of easy appreciation. At the commencement, all is like a common cold; and it is the reiterated catching cold in an eminently neuropathic subject that seems to induce the aggravation. The prognosis, as regards immediate danger, is favorable; but is more serious in respect to future consequences, owing to the various evil consequences which may ensue upon the congestions the paroxysms give rise to. The destruction of sleep and disturbance of digestion which it causes are other important circumstances. Among the more serious results, is the production of hernias and of emphysema pulmonum. The irritation of the glottis and larynx should be relieved by tepid aqueous or narcotic vapours, and by the use of demulcent emulsions with laurel-water. When the expectoration is difficult, syrup of poppies, with small doses of tartar emetic, should be given, the antimony, whether it causes vomiting or not, affording great relief. So, too, small doses of extract of belladonna every night, or night and morning, should be given when the expectoration is somewhat modified, and in a few days the convulsive character of the cough usually abates. When this drug disagrees with the patient, it should be used endermically.

4. This variety may be called *hysterical*, from its occurring in hysterical patients. In a subject whose respiratory organs are habitually in a good condition, all at once an irregularly paroxysmal cough comes on, occurring at frequent intervals, and sometimes almost without intermission. It does not terminate with the expulsion of mucosities, but is either dry and objectless, or is accompanied by a true phlegmorrhagia. Hysterical phenomena sometimes precede or accompany the cough; while at others it ceases instantly that these appear. The cough is found to get worse and worse, in proportion to the development of the hysteria; and this without any physical explanation of its intensity. The pulse is not febrile, but may be irregular, and such a one as is found in nervous subjects. The prognosis is favorable, unless the cough is mistaken for a phlegmasia, and aggravated by maltreatment. The treatment is, in fact, that which is proper for hysteria; but two means are especially indicated—the use of belladonna, and the employment of baths. Belladonna, given in doses of one seventh of a grain every half-hour, is highly efficacious; and it is rare for five or six doses to be given before improvement is visible. Baths at from 84° to 89° act as if by enchantment; but sometimes it is useful to give them at from 75° to 82°; and this is the temperature which will in most cases prove the best, after the patient has already employed the higher.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 385-96.

On Modern Humorism. By M. Tessier.

M. Tessier arrives at the following conclusions from an examination of the results obtained by M. Andral in his investigations of the condition of the blood in different diseases:

"We have not been able to lay hold of any one etiological fact having any value whatever. This suffices to show that in our opinion what is called modern humorism, purified humorism, does not and cannot exist as a medical doctrine, and that, consequently, *organicism*, or the principle of the localization of disease is but a chimera. The title of a celebrated work ('De Sedibus et Causis Morborum') has too easily deceived many. The belief that the lesions of the solids and liquids of the body are the seat and cause of disease has been credulously received, and the phantom of localization has been pursued with a zeal worthy of a better result. While believing that a reform in medical doctrine has been accomplished, we have been carried back to whatever is most false and most hypothetical in Galenism. It would seem as if medicine is never to leave the track marked out for it by Hippocrates and Galen. In the sixteenth century, reform in medicine consisted in reasserting the errors of Hippocrates, while in the eighteenth and nineteenth centuries we have been content with Galenism. It has been thought that, by ornamenting it with the results of modern physics and chemistry, a reform has been accomplished; but we have only complicated a system in whose trammels we are yet kept.

"Nothing is more dangerous in science than to attach importance to mere probabilities, and of this modern humorism furnishes us with a proof. For any man, who has not a well-defined idea of what *disease* really is, or who has not yet passed the abyss of general pathology, nothing seems more probable than the supposition that a certain number of diseases proceed from changes in the blood. The part this fluid plays in the economy renders this so easy to believe; and how many physicians with Galen have believed it! Yet this belief, at first sight so simple and seductive, is a mere medical extravaganza, an absurdity unworthy of a serious thinker.

"The history of the alterations of the blood is and will always be a question in *semeiotics*, and no more, inasmuch as such alterations are but symptoms. Supposing chemistry and the microscope succeed in exposing the minutest modifications of this fluid, that will not convert a symptom into a disease, a sign into a cause, a question of semeiology into one of etiology. He who enters on a study of the alterations of the blood as the cause of disease, seems to me to act very much as a surgeon who would study crepitation as a cause of fracture. It does not follow that he should not study crepitation, nor do I mean to assert that it is not highly advantageous to study the blood in disease, by all the procedures that physics, chemistry, and physiology have placed in our hands. I am happy in adding that the labours of M. Andral abundantly furnish the proof of this. To that physician will always remain the honour of having verified and classified a certain number of the alterations of the blood, and having furnished useful indications in semeiotics. Thanks to his labours, we are now able to say that humorism is an *error*, and that the changes in the blood are *facts*."—*Rev. Méd.-Chir.*, t. v., p. 264.

On the Treatment of Psoriasis and Lepra Vulgaris. By M. EMERY.

M. EMERY states, that when appointed to the St. Louis, several years ago, he tried all the various remedies so warmly recommended by authors, and of all these *arsenic* proved the best internal medicine; but besides the inconveniences which it sometimes gave rise to, its operation was very slow. The external use of *strong tar ointment* (⅓ to ¼ of tar) produces, in fact, a far more rapid cure than any other means. Of from 1500 to 1800 patients who have employed it, five sixths have been rapidly cured, and that without any ill consequence, or any greater frequency of relapse than after internal means. In 228 cases of psoriasis, the arsenic and tar were used conjointly, and 200 were cured within two months. Of all the preparations of *mercury*, the *protiodide* is alone efficacious (⅛); but it excites much irritation of the skin, and if applied to large surfaces, produces salivation. The *iodide of sulphur* is useful in psoriasis of the head; but if applied to large surfaces, produces erysipelas. The conjoined use of the arsenic and tar-ointment constitutes, in fact, the best medication. In a disease so apt to recur, the greatest attention to diet is essential; and on the least symptom of recurrence, resort should be had to medicine, without waiting until it has become very bad. Before commencing, the patient takes a bath, and rubs in the ointment with gentle friction three times a day. In two or three days the strength of the ointment and activity of the friction are increased; and when the disease is of old standing, linen rags smeared with the ointment are to be kept applied. A tepid bath is taken once or twice a week. It is only in very irritable skins that the treatment is interrupted by the appearance of impetiginous pustules or furunculi. In about ten days we perceive, where the squamæ have fallen, a whitish circle circumscribing the patches, and extending from the circumference towards the centre. This is a sign of a decrease of the disease, which usually disappears in two or three months. With respect to the arsenic, we should begin with five drops of Fowler's solution, in four ounces of sugared water, which are to be divided into two doses. Every other day the dose is to be increased a drop, until twelve are reached. If we observe that the patches are becoming thinner, and acquiring a blackish-gray colour, this is a sign of saturation, and the dose is not to be increased; but even when this is not present, it is rarely proper to go beyond twelve or fifteen drops. Sometimes the skin becomes hot and painful around the patches; but this is relieved by tepid lotions, demulcent drinks, and diminution of the dose. After twelve or fifteen drops are reached, a feeling of constriction of the throat, or severe pain of the stomach, is perceived; and then the medicine should be suspended for a day or two, and recommenced *de novo* with the small dose. Pain near the heart, with palpitation, sometimes renders venesection requisite. If any contraction of the extensors of the limbs is observed, the medicine must be at once abandoned; and as soon as the blackish-gray colour of the patches appears, which announces saturation and an approaching cure (though such spots may remain for months), the arsenic must be discontinued.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 481-90.

SURGERY.

On Granular or Egyptian Ophthalmia. By MM. Thiry and Buys.

Two highly interesting papers have recently appeared in the 'Annales d'Oculistique' upon this disease, which, heretofore so destructive in the French and British armies, has of late years especially attacked the Belgian troops. This has given rise to most praiseworthy investigations on the part of the Belgian military surgeons; among the fruits of which is a new theory of the disease advanced by M. Thiry, and a new and efficacious mode of treating it discovered by M. Buys.

M. Thiry's views are detailed at great length by his pupil M. Crocq, but a brief abstract is all we have space for. He considers the disease as a special inflammatory affection, having its peculiar signs, the most characteristic of which are the *granulations*, which have been erroneously regarded by prior authors as a hypertrophied condition of anatomically-normal elements; whereas, in the author's view, they are a new and special element. Their existence may be suspected when the eyelids are swollen, slightly everted, and of a venous tint. On everting the lids, a mass of little red projections is seen upon a very light red surface, especially under the upper lid, and near the oculo-palpebral fold. When a little older, the conjunctiva looks slightly bronzed, and the little red bodies uniting together, present an appearance like the surface of a strawberry or of velvet pile. Little furrows radiate between these in every direction, in each of which a lens detects a streak of purulent matter, which in some places is sufficiently abundant to conceal the granules. Slight pressure on the granules renders them pale, only to become the more red when this is removed, which M. Thiry regards as a pathognomonic sign, due to the great congestion of the parts, owing to which, also, the furrows are then found streaked with blood. The irregularities seen on inspection are not the granulations, but merely their summits. The granulations are imbricated; and to see them completely, the finger must be passed along the reverse way to that in which they are disposed. The apices of the granulations are always directed towards the larger angle of the eye, from which also the pus flows. Examined microscopically, the granulations are found to be composed of a very close vascular network, supported by a fleshy substance. When they are of a somewhat older date, they are less acuminated and more highly organized; some epithelial cells are then met with, and their summits have the bronzed appearance. When very old and numerous, they form large masses, the mucous membrane and submucous tissue also becoming indurated. This granular transformation of the mucous membrane constitutes a true local diathesis, and renders the disease very obstinate. At a line or two from the free edge of the eyelids an infinite number of small capillary vessels are found gorged with blood. They anastomose in various directions, and give rise to the characteristic swelling and appearance of the conjunctiva. It is by this union, under the influence of a special inflammatory action, that what are called granulations are developed; and it is to this remarkable vascularity that we must attribute the activity of the pathogenetic power of the inflammation, the rapidity with which granulations may be reproduced, the frequency of their complication with pannus, and the remarkable variations which take place in the accompanying palpebral and ocular congestions—most surprising changes, in respect to these being effected even in an hour or two.

Although maintaining the granulations to be quite a new structure, entirely distinct from papillæ or conjunctival glands, he admits the frequency of the enlargement of the latter in various forms of ophthalmia, and of their being mistaken for the granular disease. A little attention serves to distinguish the granules from these various hypertrophies. The hypertrophied *papillæ* are rounded or nipple-shaped, instead of being acuminated or conical. Even when assembled in groups, they remain distinct from each other. They are not imbricated, but spring out direct from the mucous membrane, of which they form an integral portion, without modifying its characters. Their vascularity is less; and, being covered with epithelium, they do not bleed on pressure as the granulations do. Moreover, they do not secrete; and if secretory irritation complicates them, this is not purulent, but mucous. They are also developed in greatest abundance near the free edge of the lid. The hypertrophied *glandules* are globular, and exhibit to a lens their excretory openings. They are larger than the papillæ and granulations, equalling or exceeding a pin's head in size. If there is one thing more than another resembling the granulations, it is the granulation of a recent wound, which, like them, is also a cellulo-vascular, acuminated, bright-red production, deprived of epithelium, bleeding, and secreting pus; but however similar in appearance, these two conditions are not identical—the one being reparative, and the other disorganizing.

M. Thiry lays it down as a principle, that the contagious property upon which he believes this disease to depend may vary in intensity, and therefore in the degree of pathological effect it is capable of producing; and that the activity of this contagious principle is reproduced by the granulations in proportion to the acuteness of their inflammatory action. Although a chronic state of the inflammation may have been preceded by an acute one, this is by no means necessarily or usually the case. In this last case the activity of the contagious poison has not proved competent to set up the acute inflammation. In the acute form, the granulations are not immediately formed; the intense character of the conjunctivitis delaying the pathological changes which are to take place; but next day their imbricated asperities may be seen, reflecting the light strongly, and imparting a peculiar brilliancy, that diminishes as they grow more chronic. The isolated conjunctival vessels now disappear, and actively enter into the new pathological modification which is going on; and so much is this the case, that the reappearance of any regularity of the vascularity of a granular conjunctiva is an invariable sign of improvement. In their *chronic* form the granulations enlarge, especially at their base, so as to obliterate the separating furrows, and their summits become rounded, while their colour is bronzed, in place of bright red. The mucous membrane, too, is transformed into a hard mass, traversed by irregular and superficial furrows, although those originally separating the granulations no longer exist. There are occasional attacks of conjunctivitis, and frequent epiphora. Accordingly as these chronic granulations are or are not preceded by an acute form of the disease, M. Thiry designates them as *secondary* or *primary*, the terms chronic and acute being more suitable for describing the degree of accompanying inflammation; and accordingly as this may be excited, either form may become again chronic or acute in its course.

M. Cunier furnishes us with an account of the *treatment* of this disease, found so serviceable by M. Buys. The disease has of late not only made great ravages among the military, but also among the lower orders of the civil population in Belgium; and the most anxious attention has been directed to the discovery of the most appropriate treatment. The *nitrate of silver*, either in substance or in solution, seemed, as the result of innumerable comparative trials, to be the most suitable application; and it was generally resorted to for the soldiers, but the civil population often refused to submit to the frequent painful applications that were necessary. M. Buys, a military surgeon, maintained, however, that the greatest amount of benefit was derivable from the application of the *acetate of lead;* and an official investigation lately instituted seems to have determined the pre-eminent advantages of this substance. Since then, M. Cunier has treated about 100 cases by its employment, and with the best results. A very minute hair-pencil is moistened sufficiently to enable it to take up a grain or a grain and a half of the acetate in impalpable powder, which is to be extended over the conjunctival surface of the eyelid, closing this afterwards until the tears have dissolved the acetate. The solution of the salt takes place more slowly under the upper lid; and it may be expedited by touching the mucous membrane with some of the tears that have accumulated at the angles of the eye. As soon as the acetate has penetrated the tissue, we find that this becomes crisper, and a kind of vascular efflorescence takes place; but the most remarkable phenomenon is, that the granulations, when only of moderate size, with their furrows, disappear, the membrane remaining even and polished as long as it is exposed to the air. The white shining aspect imparted by the lead does not generally occur until after the eyelid has been replaced. When the granulations are larger, and the furrows deeper, the polished appearance is not produced until after the second or third application. The application causes a marked feeling of adstriction and burning, but rarely true pain. This soon disappears, and reappears on closing the lid, when a flow of white, scalding tears takes place for some seconds, or two or three minutes, after which the eye can generally be kept open. A sense of fulness replaces that of gravel in the eye produced by the granulations, and continues for some time. The vascular reaction produced is very slight; but after the first application, there is always a certain amount of swelling or even œdema of the eyelids for twenty-four or forty-eight hours. When both eyelids are affected, the upper should be first treated, and two or three days after, the lower one. In two or three days, too, any portions of the altered conjunctiva that have escaped are retouched; but no new application must be made until the irritation produced by the former has disappeared, which it does with rapidity, in proportion as the disease is acute, and the flow of tears abundant.

The salt is sometimes very long before it becomes detached, and patients treated by Buys in 1839-40, still present traces of it. M. Cunier here refers to one of his former papers cautioning against the application of *lead salts* to the eye, as being liable to leave incrustations that impair the integrity of its functions; but he has employed the *acetate* in ulcerated cornea, without any ill effects, which he attributes to the fact that it is transformed by the tears into a soluble chloride, while the salts usually employed, as the carbonates, meconates, &c. are insoluble. He has found the cure of atonic corneal ulcerations remarkably accelerated by dropping into the eye a solution of neutral acetate of lead and hydrochlorate of soda, no incrustation being produced.

M. Cunier has had as yet no opportunity of treating the *acute* stage of the ophthalmia by the acetate (in the *subacute* it has succeeded); but from his being able to cut short the progress of a case of *gonorrhœal* ophthalmia, he augurs well of it in such. Both in the acute and chronic stages of *catarrhal* ophthalmia, he has used it with the best effects; and in *strumous* and catarrho-strumous ophthalmia, the distressing photophobia has rapidly yielded to it. The comparative painlessness of this application renders it far preferable to the nitrate of silver; while, when we wish to keep a stimulant applied to the eye for a prolonged period, it is thus better accomplished than by the use of collodium recently recommended.—*Annales d'Oculistique*, tom. xxi, pp. 108-29, and 229-48.

Pathological Classification of Military Pensioners.

The in-pensioners at the Hôtel des Invalides of Paris (analogous to our Chelsea Hospital in its purposes) are classed in a manner not devoid of interest as regards pathological physiology and operative medicine:

Blind	154	Lame or unable to walk	133
Amputation of both legs	12	Feet frozen in Russia	28
„ of one leg	313	Mutilated hands	132
Amputation of both arms	2	Various other wounds	1027
„ of one arm	236	Lay brothers	178
Paralytic or impotent	237	Septuagenarians	516
Epileptic	12	Octogenarians	37
Mad	31		
Silver noses or chins	8	Total	3056

The annual admissions generally amount to 200.—*Gazette Médicale*, No. 23.

[M. Malgaigne, whose opinion upon the fatality of amputation of the thigh was so strongly expressed during the discussion on gunshot wounds at the Academy, inquires whether the fact of none of the pensioners exhibiting an example of it arises from no soldier on whom it had been performed having survived.]

On Syphilitic Disease of the Testis. By M. Ricord.

M. Ricord has recently addressed some interesting observations upon this subject to the Paris Surgical Society. He observed that the disease may show itself in three forms,—one in which there is swelling of the tunica vaginalis; another in which the proper substance of the testis is invaded; and a third wherein the scrotum becomes the seat of a gummy tumour. The most frequent cases are those in which the tunica albuginea is affected. When the sarcocele is *general* and left to itself, atrophy almost surely results. It need not even be characterised by swelling, the organ only being observed gradually to waste away; but even here careful examination will detect an induration, produced by the effusion of fibrous or fibro-plastic matter, which may even become transformed into cartilage or bone. In some cases, as this effused matter becomes absorbed, the seminiferous body, which had been compressed by it, reacquires all its normal characters and functions. In *partial* sarcocele, the seminiferous vessels are never completely destroyed, and there may be diminution, but not total loss of virility. Whether partial or general, the pain is generally trifling, and tenderness on pressure is felt only in the partial sarcocele, the seminal vessels then suffering when pressed between the fingers, and the morbid product occupying the centre of the organ.

Sir Astley Cooper's description of the disease is not quite correct. Thus, he speaks of swelling of the epidydimis, whereas the syphilitic disease exclusively occupies the testis, any swelling of the epidydimis that may be present being due to some concomitant cause. He also mentions suppuration, which never takes place in syphilitic testis, and can only arise from an accompanying affection of the scrotum. He has also laid too much stress on the presence of pain, which usually does not exist, and upon the coexistence of cutaneous eruptions, than which tertiary affections of the fibrous and osseous tissues are far more common. Moreover, syphilitic sarcocele may exist in some individuals as a consequence of hereditary syphilis, unpreceded by any primary or secondary symptom. Mercury will cure all forms of the disease; but the iodide of potassium is a more efficacious and rapidly acting remedy.—*Gazette des Hôpitaux*, No. 44.

On Deep-seated Serous Cysts of the Breasts. By M. ROBERT.

THESE tumours are of difficult diagnosis; and, as in a short time five cases of what is considered a rare disease have fallen under M. Robert's notice, he has published a few observations upon it. In all, the cysts were unilocular, developed amidst the cellular tissue which unites the lobules of the gland, and were found (varying in size from a walnut to an apple) nearer the centre than the circumference of the gland. At first, completely covered by it, they distend the organ; but as they increase in size, they project between its lobes, only covered by adipose tissue, not causing much pain or uneasiness until they have reached a considerable size. The fluid they contain much resembles that of hydrocele, being sometimes reddish. Their walls are thin and very adherent to the gland, and as smooth within as any serous membrane. Sometimes the tissue of the mammary gland is unaltered, and at others so indurated as to give rise to the suspicion of cancer. The tumour, indeed, imparts a feeling very like that of scirrhous or fibrous tumours; but at the centre of its projecting part a small elastic point is always discoverable, pressure on which produces a partial and momentary subsidence of the tumour. Very careful examination also succeeds in detecting fluctuation. The treatment consists in widely laying open the cyst, and exciting suppuration within it by means of a tape, cicatrization not taking place for five or six weeks.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 159-63.

On Arthropathia of the Shoulder-joint. By M. VELPEAU.

M. VELPEAU designates the affections of the joints under the general term arthropathia; and in the present paper, a peculiar form of disease affecting the shoulder-joint is illustrated by several examples. Every surgeon meets with cases which are characterised by atrophy of the muscles of the shoulder, especially the deltoid; and numerous autopsies have assured M. Velpeau that the osseous portions of the joint participate in the atrophy. The deltoid may be reduced to almost a membranous condition, scarcely exhibiting any muscular fibres; and not only is the glenoid cavity diminished in size and depth, but the head of the humerus is lessened, and even its body diminished in volume and length. The cartilages disappear early, not as in ordinary caries, but sometimes by a sort of atrophy, and at others by undergoing osseous transformation.

The disease may commence as ordinary arthritis, but is soon found to differ from this by the slowness of its progress (M. Velpeau has known cases occupy more than twelve years before complete), and the accompanying atrophy. The original part affected seems to be the soft parts, especially the deltoid; and the constancy with which this is the case has led M. Velpeau to conjecture, that an affection of the circumflex nerve might be the original one. There are three distinct stages: the period of painful affection of the brachial plexus, or only of the circumflex, which, with the accompanying swelling, has led to the disease being mistaken for rheumatism; the muscular atrophy; and the changes in the bones. In two of the cases cited there was observed a semi-luxation downwards, of easy temporary reduction. This luxation may be due to the elongation of the muscular fibres allowing the escape of the head. In treating these cases, three periods have to be observed. During the early and inflammatory period, repeated leechings, or, better still, small cuppings, friction with mercurial and belladonna ointment, and the application of emollient and resolvent cataplasms, are indicated. This period passed, large blisters, moxas, or the cautery before and behind the joints, baths, with local stimulants and tonic douches, either of plain, or sulphureous, or aromatic waters, are demanded. In the third stage, when the bony parts are affected, the derivative and antiphlogistic treatment is to be laid aside, and frictions, especially with the essential oil of turpentine, resorted to. They rouse the dormant vitality, and produce a cure in many cases apparently hopeless. The sagacity of the surgeon will decide between the two risks of causing anchylosis by leaving the arm too long unmoved, and the re-excitement of inflammation by moving it too soon. Of the two dangers, the first is the greatest; and in order to prevent it, we must not fear exciting a little pain. Cases are detailed in which even subluxation had been produced; and yet treatment has soon restored the plumpness and full use of the arm.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 64-73.

[In reference to the partial dislocations sometimes observed in these cases, we may notice a paper by M. Yvonneau, of Blois, inserted in the same journal (p. 163), in which he draws attention to the fact, that occasionally some days after contusion of the shoulder, unattended with dislocation, the bone becomes spontaneously displaced, the arm becoming elongated and pendant, and a paralysis of the deltoid existing. M. Yvonneau believes that the displacement results from the temporary paralysis of the deltoid, and the elongation of the capsule by the hydarthrosis; the determining cause being a contraction of the pectoral during some voluntary or involuntary movement of the patient.]

MIDWIFERY, &c.

On Embryotomy and the Cæsarean Section. By M. CAPURON.

THE French practitioners do not seem even yet to have very fixed notions concerning the preferability of these two operations, in certain cases in which the induction of premature labour has been neglected. M. Capuron, reporting to the Académie de Médecine upon some cases communicated by M. Guisard, states that the Cæsarean section is by no means so fatal as is generally supposed. The propriety of its performance may become a question under three circumstances. *First.* The pelvis may be so deformed, that it is physically impossible to extract the child even after its destruction. Here the answer is obvious; whether the child be living or dead, the Cæsarean section alone affords any hope for the mother. *Secondly.* The deformity is not sufficiently great to prevent the delivery after lessening the child's head; and here the English practitioner would at once, after full conviction of the impossibility of otherwise extracting it, proceed to embryotomy; but M. Capuron, without saying so distinctly, evidently infers that it would be wrong to do this as long as the child is supposed to be in vigorous life. Still he hesitates in declaring positively in favour of the Cæsarean section, and probably would counsel waiting until perhaps either operation failed to save the mother's life. *Thirdly.* The deformity may be such that, after vain attempts at extracting the child after embryotomy, the Cæsarean section still has to be resorted to. This he partly stigmatises as bad practice, as destroying the child, and conferring additional danger on the mother; but when the pelvis is not so decidedly deformed as in the first class of cases, the difficulty of determining whether a child can or cannot pass after embryotomy is very great, and the practitioner, where there is doubt, is quite justified in first exposing the mother to the lesser danger only. This was the practice in some of the cases here recorded.

The first case occurred in the person of a rickety little woman, æt. 22, who was found in her first labour to have an extreme degree of deformity of the pelvis. Three physicians, after in vain, for thirty-six hours, trying to remove the child by the crotchet, resorted to the Cæsarean section with complete success, as far as the operation was concerned, though the patient died a month after from pneumonia. Another little woman formed the subject of the second and third cases. In her first labour, after repeated manœuvres, and with great difficulty, a mutilated child was extracted; but in her second, after prolonged efforts with the crotchet, the Cæsarean operation was resorted to, and the mother recovered. This woman had refused to allow premature labour to be induced in her second pregnancy; and we must confess that we think in the cases of individuals who, aware of their unfortunate conformation, allow themselves to become pregnant, and yet refuse their offspring the chance of life which the induction of premature labour gives, the obstetrician is not morally justified in preferring embryotomy to the Cæsarean operation. The fourth case occurred in an intelligent primipara, æt. 25, and although the deformity was such, that extraction of the child by embryotomy was barely possible, yet the practitioners in attendance, only called to her after sixteen hours of violent labour, during which convulsions had occurred, and encouraged by the repeated demands of the patient, preferred the performance of the Cæsarean section. A small asphyxiated child was removed, and soon restored to life, and the mother did very well. Thus within the space of five years, the Cæsarean operation was performed three times in one provincial district of France with success, though its results have been found as fatal in the capital as they have hitherto in our own country. That such success is in great measure due to the purer medium in which the patients lived, there can be little doubt; although M. Capuron seems to think otherwise, as he terminates his report by an indirect preference of the section, even to the induction of premature labour. He says, "Who would dare, under this pretext, to refuse the hand of succour to women in large towns, having excessively deformed pelves, and expose them to the chances of an artificial delivery at the end of seven months, to prevent the danger, or even death, that awaits them at the ninth, for want of good air?"—*Bulletin de l'Académie*, t. xiv, pp. 775-85.

On the Pneumonia of Children. By M. VALLEIX.

CONTRARY to formerly entertained opinions, pneumonia is a frequent disease in children; but it is to speak too vaguely to treat of children in the mass, as great differences exist, according to their ages. We may take three periods into account: 1st, from birth to the second year; 2d, from two to six; and 3d, from six to fifteen. And as a general statement, making allowance for even numerous exceptions, it may be said that the disease decreases in severity from the first to the third of these periods. Careful researches have proved that during the *first two years* pneumonia is more frequent, more dangerous, more rapid, and oftener double, than at any other period of life except *extreme old age;* and the similarity of the disease, at these two extreme periods of life, is in many respects very remarkable. During even the *first period*, the danger of pneumonia much depends upon the part of such period it occurs at. Whatever the state of the child's health may be, if attacked during the *first month* it may be regarded as doomed to certain death; and from the first to the sixth month there is little hope of saving it, if the attack be at all severe. From the sixth to the twenty-fourth month the cures become more frequent, but the prognosis is still very bad, and should be most guarded, until convalescence is quite complete. The general symptoms may seem to amend, and the local ones to make little progress, but in from twenty-four to thirty-six hours a recrudescence occurs, which proves rapidly fatal. If the patient continue uninterruptedly to improve during thirty-eight hours, the convalescence is almost always definitive. The *local symptoms* should be especially watched; for it is not here as in adults, in whom we often see local symptoms continue for a considerable time after the general ones have diminished, without any cause for alarm. If in the infant there is not in twelve hours a notable improvement in the local symptoms, a fatal relapse must be feared. Another peculiarity is the *lobular* form of the disease, usually a consequence of an already severe attack of capillary bronchitis. At this period, too, as in advanced age, *double pneumonia* and pneumonia of the *apex* are common. M. Valleix believes that there is some confusion in the statement of MM. Bailly and Legendre, that the anatomical condition of the lungs in these cases is due to a persistence of the fœtal state; for although a condensed state of the pulmonary tissue, disappearing on insufflation, may very often be found in very young infants, yet it is an error to suppose that all the cases usually described as infantile pneumonia are of this nature.

He has, in such cases, met with even a denser hepatization than in the adult, the lung rapidly sinking in water, and being quite impervious to insufflation. The cases described by these writers would not, from their symptoms during life, be considered by good observers as pneumonia.

In proportion as we approach the *second period*, the pneumonia loses its lobular character, and approaches nearer to that of the adult, while its fatality diminishes also. Indeed, especially during the last two or three years of this period, the *benignity* of the disease is remarkable; and little alarm need be excited, except if the child is already an invalid, when the supervention of pneumonia is exceedingly dangerous. By benignity it is not meant that the symptoms are slight, but that the cure is so sure; for, in fact, the symptoms have a very alarming appearance, and yet, in spite of them, amendment takes place in from two to four days, after which time the cure goes on rapidly.

In the *third period*, the disease still more resembles that of the adult, and is still benign. A distinguishing circumstance at some part of this period is the appearance of expectoration.

As a general rule, the younger the child the greater is the difficulty of the diagnosis. For auscultation, very young children should be held, by an assistant placing his hands under the thorax and belly, when examination can be made, especially as in this position the child usually ceases crying for awhile. Older children should be held on the mother's arm. In a case, in which auscultation was very difficult, M. Valleix availed himself with success of the observation of the increased thoracic vibration, indicated by Monneret.

Treatment. During the third period, the child is treated as the adult. In the second, we must be more chary of our means, remembering that there is a natural tendency to cure. One bleeding usually suffices, and antimony should be reserved for only severe cases, and used with great caution. With still greater caution should it be given in the first period. Still if there is great or increasing hepatization, it is to be used in divided and infrequent doses. Small cuppings are very preferable to leeching, and mild opiates are too much dreaded by practitioners. Blisters should be wholly discountenanced, especially in the very young.—*Bulletin de Thérapeutique*, vol. xxxvi, pp. 97-103.

Masturbation in Girls. By M. RENE-VANOYE.

DR. DURR long since affirmed that this may always be suspected when warts are observed on the index or medius finger, especially when they are not present on the other fingers. In support of this opinion, Kretschmar gives the case of a girl, who, wishing to ascertain whether her fowls were about to lay, was in the habit of passing her finger into the cloaca, with the effect of generating an abundant crop of warts upon it. Dr. René-Vanoye has recently met with two cases, in which this sign enabled him to discover the real cause of otherwise inexplicable exhaustion. It is one certainly desirable to bear in mind, though cautiously to be made use of.—*Rev. Méd.-Chir.*, t. v, p. 235.

On Local Bleeding in Displacement of the Uterus. By M. OLLIVIER.

M. C. OLLIVIER, of Angers, observes, that during a fifteen years' practice, in which uterine diseases have especially occupied his attention, he has never met with a case of displacement of the uterus that was not preceded by a more or less considerable degree of engorgement; and that instead of employing pessaries, and the whole variety of palliative bandages, he has been always enabled, by leeching the os uteri, to effect a radical cure. The careless way in which leeching this part has frequently been performed, has been the reason it has so fallen out of favour. In the first place, it should very rarely be undertaken, except in chronic uterine affections, when the congestion has become purely passive. Small leeches should be employed rather than large ones, the suction of the latter being too energetic, and their bites sometimes proving painful. They should never be employed in a large number, it being far preferable to renew their application frequently. In applying them, a small conical speculum should be used, so that the whole of the os uteri may not become inclosed within it; for otherwise there is risk of the leeches entering within the cavity of the cervix. It is from accidents, which have resulted from their doing so, that their application has been thought to be attended with danger. It is evident that considerable *engorgements* will yield only to repeated applications; but these should be made by a small number of small leeches.—*Gazette des Hôpitaux,* No. 103.

Recovery of an Infant after Perforation of its Cranium. By Dr. Lagae.

In July, 1839, the author was called to a woman, æt. 34, in labour with her second child. Two years before, she had been delivered of a still-born child by means of the forceps. He found that the labour had continued more than forty-eight hours, and that the practitioner already in attendance, after having in vain endeavoured to deliver by the forceps, had perforated the cranium, and made ineffectual efforts at extraction. The woman was fatigued, but not exhausted; and Dr. Lagae fearing, owing to the height at which the head was situated, and to the narrowness and obliquity of the pelvis, that greater danger would result to the mother by continuing the attempts at extraction than by performing the Cæsarean section, resorted to the latter. No difficulty attended its performance, the mother getting about in a few weeks, and living for eight years after. A feeble male infant, heaving some sighs, was delivered. There was a large wound in its cranium, situated to the right of the sagittal suture, and a few lines in front of the posterior fontanelle. Through this the brain was visible, looking like a sanguinolent pulp, a small portion escaping by the wound, as did other portions, after the suppurative process was set up. The child recovered; compresses, dipped in cold water, being alone applied to the part. It, now nine years old, was recently exhibited to the West Flanders Medical Society, a loss of substance equal to a two-franc piece in size being still observable in its cranium, notwithstanding that reparation of the loss of the cranial bones occurs in the young.

The child's intellectual faculties are in their normal state. A circumstance worthy of note is, that at the solution of continuity in the bone, where the soft parts alone cover the brain, there sometimes takes place a depression, and then the brain is plainly seen raised up by the arterial pulsations at the bottom of this cup-like depression. When this appearance manifests itself, experience has shown that the child is not well. At other times, the soft parts remain on a level with the cranial bones, and the arterial pulsations are slightly, if at all, observable.—*Revue Médico-Chirurg.*, tom. vi, p. 55.

Vagitus Uterinus.

Dr. Winkel communicates a case which occurred in the person of a rickety woman, in labour with her first child. He and several other persons distinctly heard the infant cry during a quarter of an hour, while still in its mother's womb. Indeed, it was only delivered eight or nine hours after by means of the forceps, and that with such difficulty that its life was sacrificed. The hydrostatic test, applied twenty-four hours after, showed that respiration had occurred.—*Medecin. Zeitung,* 1849, No. 12.

[It is to be regretted that so meagre an account of the phenomenon should have been given; for the mere fact of the child's head requiring to be brought by the forceps through a rickety pelvis several hours after, is no proof that its mouth may not have descended to some distance in the cavity before the cry was heard.]

MATERIA MEDICA AND THERAPEUTICS.

On Nux Vomica in Impotence and Spermatorrhœa. By M. DUCLOS.

INCOMPLETE impotence is of far more common occurrence than would be supposed, until many patients have been questioned respecting it. Erections are almost always possible, especially in the morning; but they are soft, incomplete, and insufficient, a certain amount of tension only continuing, and that for a short time. This state may be met with in men even of the strongest make and most robust constitution, in whom the vascular and muscular systems have attained their highest development. In others, in whom these systems and the nervous system are ill-developed, the generative functions are properly exercised; so that the general physical force is no criterion of the special force of these organs. This imperfect condition is as often found in those who have been excessively continent, as in those who have abused the sexual organs; and it is observed just as often in persons whose nervous system is easily excitable, as in those in whom its lesser irritability allows of a predominance of the muscular and vascular systems. Self-pollution may occur either by night or by day, the discharge being either a true or a pseudo-spermatorrhœa.

Accident first led the author to the employment of nux vomica in this class of affections; and he has since observed several cases in which its efficacy has proved very great. He divides 75 grains of the alcoholic extract into 100 pills. During 5 days he gives 1 every night; then for other 5 days, 1 morning 2 night; for other 5 days, 2 night and morning; and for other 5 days, 2 morning and 3 at night; and so on until 4 are taken night and morning. He has never found any harm result, although some patients have taken 14 pills per diem. In many cases the stomach is rapidly improved by the medicine, the lost appetite returning. The following liniment rubbed into the loins and on the inside of the thighs is a valuable though not an essential auxiliary:—℞. *Træ. Nuc. Vom.*, *Træ. Arnicæ vel Melissæ*, *āā*, 60 *p.* *Tr. Lyttæ* 15 *p.* The regimen should be tonic; and the increased appetite demands a larger supply of food. A very moderate use of coitus is advisable.—*Bull. de Thérap.* tom. xxxvi, pp. 529-33.

Creosote in Mercurial Salivation. By DR. FAULCON.

DR. FAULCON relates a case of profuse mercurial salivation, in which, after the unsuccessful employment of the usual remedies, he employed with great advantage a gargle composed of creosote ʒ½, sage tea a pint, the affection quickly yielding.—*Phil. Med. Ex.*, No. 47.

[Although this is but a solitary case, the remedies generally resorted to in salivation are so commonly altogether useless, that we have thought it right to quote it.]

On the Therapeutical Employment of Manganese. By M. HANNON.

THE author seems to have been led to the employment of manganese by the somewhat fanciful suggestion, that as metallic substances usually found together exhibit many analogous effects, it might be possible, owing to the almost constant natural union of iron and manganese, that in cases in which the former of these did not exhibit its usual medicinal efficacy, this might be due to the artificial separation of the two substances. However this may be, he has found experimentally that in chlorosis, anæmia, and other diseases in which ferruginous preparations are so useful, manganese is so likewise; and markedly so in those cases in which iron has already failed, or only succeeded partially. He has since learned that M. Millon, in his recent researches upon the human blood, has always found it to contain silex, manganese, lead, and copper. In simple anæmia, manganese acts even more rapidly than iron; and it has been found of considerable service in the anæmia resulting from the cachexia of cancer, scrofula, syphilis, and ague; and that without the induction of the injurious stimulant effects often produced by iron when employed under these circumstances. In scrofula, iodide of manganese has proved very superior to iodide of potassium. Notwithstanding that the quantity of manganese required to exert a curative effect is much less than that of iron, the durability of such effect is much more considerable. Another very great advantage is, that it does not give rise to the numerous decompositions which iron dees when united to other tonics. The preparations of this metal are powerfully astringent, and may be employed externally. M. Hannon gives a great variety of formulæ, and recommends that the more insoluble preparations (as the carbonate, phosphate, or oxide) should be commenced with, and then the more soluble, as the tartrate, malate, iodide, and chloride, may, if necessary, be resorted to. The substance being rapidly assimilated, none is found in the excretions, and it need not be so long continued as iron.—*Rev. Méd.-Chir.* tom. v, p. 332.

On the Properties and Administration of Quinine. By Dr. HOLMES.

WHILE some authors advocate the use of quinine as a sedative, others praise it as a stimulant, and the majority seem to regard it as possessing one or other of these properties, according to the dose in which it is given. Dr. Holmes considers that in large doses it acts as a sedative to the vascular, but as an excitant to the nervous system, producing a high degree of nervous erethism, a peculiar irritability of the nerves of hearing and vision, and a distressing vertigo. This tendency to affect the brain has led to the medicine being proscribed in fevers accompanied with cerebral congestion or inflammation, or nervous excitability; and investigation has led Dr. Holmes to the conclusion that *digitaline* modifies the action of quinine in this particular more than any other agent does; combined with this, he has given it in large doses to children of mobile and impressible temperaments with the best effect, and the same with regard to delicate females, who before could not tolerate the drug at all.

He regards quinine as possessed of high antiseptic properties; and states that in malignant and protracted fevers, in which it may be thought desirable to make an impression with mercury on the system, it expedites the effect of calomel upon the mouth, and yet checks its tendency to induce ulceration of the gums or sloughing of the cheeks. In typhus or typhoid fevers it checks the tendency to putrescency. Its abundant employment is the sheet-anchor which rarely fails the practitioner in the American remittent fevers. Its great power as a febrifuge has led to the overlooking, in some measure, of its beneficial agency in long-standing neuropathic affections.—*Philadelphia Med. Examiner,* No. 48, p. 764.

On Valerian in Epilepsy. By M. CHAUFFARD.

M. CHAUFFARD, an able practitioner at Avignon, adds his testimony to that of so many other writers, concerning the great efficacy of this substance in the various disorders of the nervous system; and in truth in many of the functional disturbances of this it is invaluable. M. Chauffard likewise confirms the opinions of many of the older and some of the modern writers on the materia medica, as to its utility in epilepsy, and he attributes the failures in very many cases in which it has been given to the small quantities employed. To be of use in this formidable affection, very large quantities must be perseveringly taken for some months. In the cases given in illustration, as much as from four to sixteen drachms of the powder were taken in divided doses, daily. Great repugnance is at first felt to both its taste and smell, but this is soon got over, and neither nausea, oppression, nor other disagreeable sensation results; nor does the drug with any certainty produce diuretic or other marked effect. Dr. Chauffard generally premised a bleeding with good effect; and several cases remained under observation for months and years after the cure was effected.—*Rev.-Med. Chir.* t. v, pp. 265-72.

[We are almost tired of chronicling remedies for epilepsy, so fallacious do they all sooner or later prove; yet in so terrible an affection, and one in which patients are usually so eager to try every resource, it is satisfactory to have the testimony of so old a practitioner as M. Chauffard, that in some forms of the disease, valerian has, to all appearances, operated a cure.]

On the Treatment of Chilblains. By M. Ossieur.

In the earliest stage, friction, either employed dry or with brandy or sp. camphor, is the simplest and best means; but when the parts have become red, swollen, shining, and even covered with phlyctenæ, but prior to ulceration, the formula recommended by M. Goffin may be used with the greatest advantage:

Camphor, 4 parts;
Ess. Oil Turpentine, 30 parts.

When the practitioner is only consulted after ulceration has for some time taken place, M. Devergie's ointment is then the best application:

Lard, 1 oz.;
Liq. Plumb. Subac., 12 drops;
Thebaic Extract, 3 grains;
Creosote, 10 drops.

Bulletin de Thérapeutique, vol. xxxvi, p. 38.

On the Pharmaceutical and Toxicological Applications of Carbon. By M. Esprit.

M. Esprit believes that the property possessed by carbon of appropriating saline matters held in solution, is not sufficiently known. Payen, twenty-five years since, announced that such is its separating power in respect to lime and the calcareous salts, that if 100 parts of distilled water, saturated with lime, be boiled with 10 parts of charcoal, the filtered liquor will show no traces of the lime. Some years later, this was shown to apply, to a certain degree, to the alkalies. In 1829, Graham studied the action of carbon on nitrate of lead, arsenious acid, nitrate of silver, sulphate of copper, sulphate of ammoniated copper, the hydrate of lead dissolved in potass, solution of iodine, and the chlorides of soda and lime. He was unable to precipitate the arsenious acid, or sulphate of copper, but succeeded with all the others. Dupasquier showed that it energetically absorbs a large proportion of the alkaline sulphurets. These facts had made but little impression, when Chevalier announced, in 1845, that the acetate and nitrate of lead, held in solution in water, wine, or vinegar, are removable by carbon, with or without the aid of heat. Garrod having thrown some doubts upon Graham's experiments, M. Esprit resolved to repeat and extend them, and now communicates the results at which he has arrived. He found that the following salts were energetically absorbed: the acetate and sulphate of copper, the acetate, sulphate, and chlorate of zinc, oxide of zinc dissolved in potass, the acetate and nitrate of lead, acetate and nitrate of iron, tartar emetic, nitrate and sulphate of silver, chloride of silver in ammonia, corrosive sublimate, nitrate of cobalt, sulphate of cadmium, and the chloride of barium. He found that the sulphate of soda, potass, and magnesia, arsenious acid, and the nitrate of copper, were very sparingly separable. Five parts of the carbon of the blood, calcined with potass, and washed, produced such complete precipitation of the following salts, that no trace could be found in the filtered liquor, viz. sulphate of ammoniated copper, acet. and nitr. of lead, sulph. and nitr. of silver, chlor. of silver in ammonia, chlor. of zinc, and oxide of zinc in potass. Twenty parts were required for the following, viz. sulph. and acet. copper, corros. subl., tart. emet., nitr. cobalt, sulph. zinc, sulph. cadmium, and chlor. barium.

M. Esprit's experiments agree with those of Mandrel and Bussy, in denying the great absorbability of arsenious acid declared by Garrod to occur; and so small is the proportion separated, that the utility of carbon as an antidote in poisoning from arsenic may be considered as *nil.* The general conclusions are the following:

1. Carbon, as regards prescribing, is incompatible with the substances which it appropriates and renders insoluble. 2. Its use in chemico-legal researches may give rise to great errors; for, as Chevalier has remarked, authors frequently direct us to discharge, by means of carbon, the colour of liquids, in which we have afterwards to seek for the presence of metallic salts removable by carbon. Thus, even in modern works, we are told to decolorize, by its use, liquids in which we have to declare whether a salt of lead, &c. is present! 3. In some cases, the property which carbon has of fixing certain bodies around its particles much facilitates their detection; thus, for the separation of corrosive sublimate, and, as shown by M. Lebourdais, of vegetable alkalies, it suffices, after treating the liquids containing them with carbon, to remove them from this body by alcohol or ether. 4. If a better is not at hand, charcoal may be given as an *antidote* against all the poisons it thus has the power of fixing. For this end it should be given, after vomiting, in large quantities of fine powder suspended in water.—*Bulletin de Thérapeutique*, tom. xxxvii, pp. 72-5.

FORENSIC MEDICINE AND STATISTICS.

On the Medico-Legal Appreciation of Ether and Chloroform. By M. Bayard.

In the present paper, M. Bayard protests against the employment of these substances for the detection of simulated diseases, or, rather, against the soundness of some of the conclusions arrived at by those who have so employed them. Thus, for example, because certain contractions or rigidity of limbs have disappeared during its employment, these have been set down as feigned, while every practitioner knows that a deformity produced by a dislocation, accompanied with violent muscular contraction, may cease on this becoming relaxed. Two cases which have fallen under the author's notice, incidentally illustrate the point. In the one, a man, suffering from diastasis, with swelling and chronic retraction of the right foot, was etherized during the removal of a strumous tumour, and then complete resolution and mobility of the foot were produced. A young girl, æt. 17, was, while suffering from white swelling of the knee, etherized, in order that an operation for hare-lip might be performed; and, during the inhalation, flexion and extension of the limb became quite easy to produce, these ceasing with the cessation of the etherization.

But, supposing this difficulty not in the way, M. Bayard denies that the practitioner has a moral right, even for the detection of any simulated disease, to induce the "ethereal drunkenness," and continue it until the individual makes the necessary revelations. It would be an abuse of his position, on the part of any medical man, if, in proposing this means of verification, he did not apprize the suspected individual of the consequences of inhalation. Any declarations obtained from an accused person, to be valid, should be made voluntarily and with full consciousness. The extracting replies by the means of the induction of drunkenness or narcotism would be just as justifiable as by etherization; and to proceed in any such way is certainly not to revive the *torture*, but the consequences of the *ordinary* and *extraordinary question*. By the production of pain, in former times, avowals were wrung from the innocent as well as from the guilty; and have we any guarantee of the veracity of statements made under etherization? The following are M. Bayard's conclusions:

1. Etherization, which has been proposed for the detection of the simulation of certain diseases, by inducing an intoxication that renders the maintenance of a fixed idea of simulation impossible, and which induces actions or replies capable of revealing such simulation, should never be employed but with the consent of the suspected person previously informed of the consequences he is exposing himself to. 2. The administration of ether or chloroform should be compared to that of narcotics or spirituous liquors, which have never been employed by physicians to induce

involuntary revelations. 3. Even if the individual suspected consents to submit to etherization, this should not shield the medical man from any responsibility that may accrue from its toxical effects. 4. In the present state of science, we cannot say that simulated muscular contractions can always thus be detected. 5. If, in some cases, individuals submitted to etherization, have been able to render an exact account of all that has happened around them, frequently other individuals have had dreams, hallucinations, and illusions, which they relate with all the conviction due to real facts. Declarations of this kind can be received in a court of justice only with the most wary circumspection.—*Annales d'Hygiène*, tom. xlii, pp. 165-201.

[Seeing the occasional fatal results which follow upon the use of chloroform, the uncertainty of the revelations obtained by its aid, and the unsoundness of the principle upon which the experiment is based, we entirely agree with M. Bayard, that this substance should not be employed for the detection of simulated disease. We have often thought that, in some of the procedures of army and naval surgeons, instituted for the same purpose, neither the dignity of the profession, the indications of science, or the rights of humanity, have been sufficiently consulted.]

On Wounds of the Internal Mammary Artery in a Medico-Legal Point of View.

By M. TOURDES.

M. TOURDES, having met with a fatal case of wound of the internal mammary artery which gave rise to a medico-legal investigation, was induced to collect all the cases he could find recorded, and endeavour to deduce an appreciation of their importance, which has scarcely been attempted by writers upon surgery and legal medicine. So little has been recorded on the subject, that he has only been able to find 10 cases detailed besides his own, and of these he furnishes analyses.

Of the 11 cases, one was an accident, another a suicide, and 9 were homicides. At its origin, this artery is completely protected by the clavicle and first rib; but is exposed to injury in its course between the first and seventh ribs. Its distance from the sternum varies during this course, chiefly on account of the unequal breadth of the different portions of the bone. Thus it is found 10 millimetres from it in the first intercostal space, 16 in the second, 11 in the third, 12 in the fourth, 8 in the fifth, and 15 in the sixth. The cartilages of the ribs, too, increasing in length as they descend, the artery passes under the middle of the second, under the inner third of the third and fourth, and crosses the fifth and sixth in such a manner as to leave but a fifth of their length on the inner side. It equals a large crowquill in size, gives out in its course both external and internal branches, and bifurcates in the sixth intercostal space, its external branch coursing between the sixth and seventh cartilage. We may be certain that it is implicated in any wound which divides these two cartilages transversely at 15 millimetres at least from the sternum. Separated from the pleura only by a thin cellular layer, and by some fibres of the triangularis sterni, it is protected in front by the parietes of the thorax, the larger size of the superior intercostal spaces leaving it there most exposed to injury. Of the 11 recorded cases, in 7 it has been wounded through a cartilage, and in 4 in one of the three upper intercostal spaces. The accident is by no means always easy of detection, for external hemorrhage is often absent, when the wound is narrow and sinuous, or when the closure of the two divided portions of the cartilage prevent the issue of the blood. When the blood is poured into the pleura, the absence of any other lesion, to account for an effusion found to be present, is the chief guide; but many circumstances here obscure the diagnosis; as, e. g. the coexistence of a serious lesion of the lungs. In 6 out of the 11 cases, death has directly or indirectly resulted from the wound; and of the remaining 5, in only 1 was the recovery prompt. Four ran the greatest risks; and in 3 of these, paracentesis thoracis was required for a consecutive empyema. Hemorrhage is the cause of this great amount of danger, and the blood may either appear externally, burrow between the muscles, infiltrate into the anterior mediastinum, or fall into the pericardium, pleura, or even the abdomen. In most cases it is into the pleura the effusion takes place. This was so in 9 out of the 11 cases, and 5 of the 9 died. The effusion, which, when in large quantities, may cause death by inducing asphyxia, generally acts by producing severe pleurisy. In the 9 cases, one patient alone survived without undergoing paracentesis, and he only after running all the risk of a chronic pleurisy.

In treating these cases, we have to consider the lesion of the artery and the effusion of the blood. Larrey applies to wounds of this artery the rule he lays down for the treatment of all wounds of the chest producing internal hemorrhage, viz. to close the external wound, and leave the case to nature, the accumulated blood itself making compression on the bleeding vessel. The results he obtained from this practice hold out little encouragement for others to follow it. Therefore other surgeons, as Delpech, Marjolin, and Velpeau, provided a reasonable certainty of a lesion of this artery can be obtained, sanction the application of a ligature. Objections to this have been offered, on the score of the difficulties and danger of such an operation; but if the vessel has been wounded in the second, third, or even fourth intercostal space, its two ends might be tied without any great difficulty, while the danger of cutting through a cartilage, and attempting to get at it lower down, is not comparable with that attendant upon a large effusion of blood into the pleura. If the hemorrhage seems to be arrested, or its precise seat cannot be ascertained, the ordinary expectant treatment must be followed. In respect to the effused blood, the usual surgical doctrine is contained in two precepts, viz. give issue to the effused blood, if it menaces the patient with suffocation; and wait, if the symptoms are not urgent, until the hemorrhage has ceased and resorption proves insufficient. But to wait for this is to await the development of violent or fatal inflammatory action; and therefore when the quantity of blood is great, and the hemorrhage arrested, a portion of the effused blood may be evacuated by the wound, either by so adjusting the position of the patient, or by the employment of aspiration. If such evacuation by the wound be impossible, we may suppose a case in which immediate paracentesis is advisable; but the circumstances will rarely justify this. When the effusion is inconsiderable, or we are uncertain as to the simplicity of the lesion, we should follow the ordinary practice; and such will form the majority of cases. The consecutive treatment must be such as favours the resorption of the fluid; and where this does not take place, it is usually desirable, about the twelfth or fourteenth day, to resort to paracentesis, that is, before the pleura has undergone sufficient alteration to render the success of the operation but little probable. In the 5 cases of the 11 in which it was employed, in 3 it succeeded, and in 2 was without effect. Of 4 patients who were cured, 3 underwent it; of 5 who died, 1 only was operated upon, 2 of these, however, dying early. As a general rule, it adds to the chances of recovery.

"Such are the considerations which may aid us in appreciating the treatment of this accident; but we must never forget the reserve with which such inductions should be introduced into medico-legal practice."

It may be useful to subjoin the references to the 10 cases quoted by M. Tourdes. *Chopart*, 'Dict. des Sciences Médicales,' tom. xliv, p. 12; *Larrey*, 'Clinique Chirurgicale,' tom. ii, pp. 181, 261, 291; *Valentin*, 'Journ. de Chirurgie de Désault,' tom. iv, p. 108; *Sancerotte*, 'Mélanges de Chirurgie,' tom. ii, p. 369; *Raybard*, 'Gaz. Méd. de Paris,' 1841, p. 55; *Bonnet*, 'Velpeau, Méd. Opér.' 2me édit. tom. ii, p. 252; *De Montègre*, 'Dissertation sur les Plaies pénétrantes de la Poitrine,' 1836, No. vi, p. 19; *Simeons*, 'Henke, Zeitschrift für die Staatsarzneikunde,' Jan. 1849, p. 123.—*Annales d'Hygiène*, tom. xlii, pp. 165-201.

Statistics of the Medical Establishments in France.

There are in France three Faculties of Medicine, and three Schools of Pharmacy,—those of Paris, Montpellier, and Strasburg. There are, too, 21 preparatory schools of medicine and pharmacy in the most important provincial towns. In

Paris, besides the Académie Nationale de Médecine and the Académie des Sciences, there are 8 medical societies for the promotion of medical science; each of the 12 arondissements of the town having its medical society also; but these last are chiefly devoted to the consideration of professional interests. In the provinces there are more than 24 medical societies. The Académie de Médecine is composed of resident or titular members, and foreign and domestic correspondents. The resident members are in future to consist of 100, thus distributed into 11 sections: anatomy and physiology, 10; medical pathology, 13; surgical pathology, 10; therapeutics and medical natural history, 10; operative medicine, 7; pathological anatomy, 7; obstetrics, 7; public hygiene, legal medicine, and medical police, 10; veterinary medicine, 6; physics and medical chemistry, 10; pharmacy, 10.

Hospitals. In Paris there are 17 hospitals, besides various hospices for the infirm poor, and special establishments for the blind, dumb, &c. There is a central Bureau d'Admission, where all cases not immediately urgent present themselves, and whence, after examination, they are distributed to the various hospitals, according to the nature of their cases. The budget of the hospitals for the present year will amount to little less than 18,000,000 francs. In France there are 1164 administrative commissions, managing 1338 hospitals and hospices, whose ordinary revenues amount to 53,632,992 francs. After Paris, the ordinary revenue of whose hospital administration amounts to between 14 and 15 millions, comes Lyons, with a revenue of 3,147,454; Bordeaux, 995,877; Rouen, 995,000; and Marseilles, 985,278; some of the administrations descending to such miserable sums as from 382 to 213 francs. During half a century there have not been 30 new hospitals founded; and, while rich legacies have been frequently left to establishments not requiring them, other localities are destitute of hospital accommodation. Of the 1164 hospital administrations, 80 possess 38,000,000 of revenue, while 669 have but 3,000,000, furnishing the insufficient mean of 4500 francs.

Insane Establishments. There are in France 12,286 indigent insane persons, supported at the public charge,—5935 men, and 6351 women; and of these, 2536, or more than a fifth, belong to the Department of the Seine. As a general rule, the departments considered poor have fewest insane, and most foundlings. For the reception of the 12,286 patients, there are 73 establishments, 6060 of these persons being in public asylums, 4621 in separate parts of the hospices, and 1605 in private establishments.

Medical Press. In Paris there are 24 medical journals, of which 3 are published quarterly, 17 monthly, 2 weekly, and 2 three times a week. Some few of the provincial medical societies publish bulletins, and a medical journal is published at Bordeaux, Lyons, Marseilles, Montpellier, and Strasburg.—*Roubaud's Annuaire Médical*, 1849.

Medical Students in 1849.—It is a singular fact, and not a very encouraging one for the already over-crowded profession in France, that, in spite of the perturbed condition of that country, the number of inscriptions at the faculty of Paris, for the year 1849, very considerably exceeds that of any of the prior five years, being 272, while it has risen from 216 to 251 in those years. The total number of pupils now in course of education in Paris amounts to 950, while in 1845 it was but 800. The other faculties do not, however, participate in the increase; as the number of students this year at Strasburg is but 109, being just the same as in 1845; and at Montpellier 174, *vice* 173. The *Ecoles préparatoires* at the different large towns vary much in respect to the number of pupils educating at them. Thus at Lyons there are 81, Toulouse 76, Marseilles 48, Rouen 45, Bordeaux 38, at other places from 24 to 26, while Rheims only musters 15—rather a small number for a staff of *eight* professors. The total number of the pupils at these schools amounts to 792, and was 751 in 1845.—*Rev. Méd.-Chir.* t. v, p. 316.

[However it may be in France, a Leipsic correspondent furnishes a different account of the effect of revolutions in Germany. In the nineteen universities of that heterogeneous country, the number of students amounted, in 1847, to 22,327, was reduced in 1848 to 17,089, and is at present but 11,465; Munich possesses the greater number (1732), and then comes Berlin (1182), while Rostock (Mecklenburg-Schwerin) has but 103.—*Gazette des Hôpitaux*, 1849, No. 53.]

Foundling Hospitals.—M. de Watteville has recently presented a Report to the Minister of the Interior, on the condition of the foundlings and exposed children in France. It is curious to observe from this the influence of local circumstances in regulating their numbers. We may divide France into four compartments: the departments on the land frontier; those on the sea frontier; the manufacturing and the agricultural departments. Ten departments of the land frontier, with a population of 4,606,983 persons, count only 6224 foundlings (1 in 740 inhabitants), and 1197 exposures in 141,925 births (1 in 119). Twelve departments on the sea-board furnish, in a population of 6,364,123 individuals, 12,142 foundlings (1 in 371), and in 184,156 births, 5098 exposures (1 in 36). In the agricultural departments, the foundlings are 1 in 650, and the exposures 1 in 50. In the manufacturing departments, the foundlings are also 1 in 650, but the exposures are only 1 in 58. Those departments which possess large centres of population, have one foundling in 204 inhabitants, and 1 exposure in 25 births; and in the departments considered poor, there is found to be 1 in 255 inhabitants, and 1 exposure in 35 births.

In 1784, according to Necker, the number of foundlings below 12 years of age were 40,000, and in 1811 it reached 69,000; but then France consisted of a population of 40 millions, inhabiting 130 departments. When, after the peace, the country resumed its natural limits, the number of foundlings below 12 years of age was found to be—

In 1819	99,346
1825	117,305
1830	118,073
1833	129,699

The rapidly increasing numbers, and consequent expense, alarmed the government, who proposed to the provincial authorities, that the baskets for the clandestine reception of the children should be suppressed, and that a system of change of residence of those living in the hospitals should be organized, to discourage those persons who, aware of the residences of their children, and of their consequent power to withdraw them, did not hesitate to place them in the hospitals, though quite able to support them. These measures were only partially adopted, but yet with the effect of reducing the numbers in 1839 to 95,624 foundlings, and the exposures to 1 in 36 births, at which numbers they have continued nearly stationary. The mortality of these unfortunate children has much diminished since 1839, when a medical inspectorship for each department was established. By law, too, a guardianship is exercised by the hospital commissions over the children after their twelfth year, but, according to M. de Watteville's report, only effectually in 20 out of 86 departments. The consequence is, that these children furnish 15 per cent. of the convict population, 13 per cent. of the prison population, and a fifth part of the entire number of prostitutes. It sufficiently indicates the present mortality to state that 1 child is exposed for every 39 births; while, some years after, the proportion of foundlings to the inhabitants is 1 to 353.—*L'Union Médicale*, 1849, No. 55.

Obituary.

THE LATE MR. CHARLES ASTON KEY.

Among the thousands who have been struck down by the fearful pestilence under which we have been suffering since the early part of July, it may be most truly said, that there is not one whose death is more deeply and sincerely deplored than that of Mr. Key; cut off as he was, most suddenly and unexpectedly, in the prime of life, in the deserved estimation of his professional brethren and the public, and in the midst of a brilliant career, which, in the course of a very few years, must have placed him at the very head of his profession, as it had already advanced him to its foremost rank.

Charles Aston Key, born the 6th of October, 1793, in Southwark, was the eldest and only son, by his first wife Margaret, of Thomas Key. She was daughter of Charles Barry, paper merchant, and great uncle of the present celebrated architect of that name; and on her death, at or soon after the birth of Charles, her husband subsequently married her sister, Mary Lawe Barry, by whom he had five other children, of whom are still living Thomas H. Key, Professor of Comparative Grammar in University College, London, and two daughters. The elder Key was for some years a general practitioner in considerable practice in the Borough, and afterwards in Fenchurch street. He subsequently obtained a diploma, and practised as an obstetric physician in Lombard street. He died in March, 1837, when only thirteen days short of eighty years of age.

Charles Aston Key, when a very young child, was placed for a short time at a preparatory school at Kennington, kept by Mrs. Lancaster. From thence he was removed to Buntingford Grammar School, Hertfordshire, of which the Rev. Samuel Dewe, M.A., of Baliol College, Oxford, was Master. He passed through this school from the lowest to the highest classes, receiving a very excellent classical education; and after leaving it to enter upon the more active pursuits of life, he ever retained for his old preceptor the highest regard and esteem. During Key's sojourn at Buntingford, an affectionate relative and schoolfellow mentions the occurrence of the following circumstance, interesting when taken in connexion with his after pursuits:—One of the boys, now a well-known bookseller, undertook to eat a large apple; but on putting the whole of it at once into his mouth, his jaws, from their too wide distension, could not be closed, and he was very nearly suffocated. To relieve this awkward accident, Key, taking a penknife from his pocket, scooped out a large portion of the apple, and then pressing the jaws together, crushed the remaining walls of the apple, to the great relief of the sufferer.

Mr. Key's professional life commenced by apprenticeship, on the 16th of March, 1810, to his father, who probably then intended introducing his son into his own excellent connexion. He entered as a pupil to the lectures in the Medical School of the united Hospitals of St. Thomas and Guy, in the autumn of 1812; and in February, 1814, became a pupil to the surgical and medical practice of the latter hospital, to which he always considered himself as specially belonging. During this period, Mr. Key became acquainted and closely intimate with Mr. Astley Cooper's amiable nephew and apprentice Henry Charles, whose early death, in January, 1817, was most sincerely regretted by all who knew him. This intimacy had a very important influence on Mr. Key's subsequent career, as it led to his apprenticeship to Mr. Cooper on the 7th of April, 1815, his indentures to his father having been for that purpose cancelled, and also to an introduction to Mr. Cooper's family, which terminated in a closer connexion.

The glorious though sanguinary battle of Waterloo, in June following, having, by the enormous number of wounded, both victors and vanquished, rendered their proper attendance by the medical officers of the army utterly impossible, the want, so soon as felt, was readily supplied by the older English students and others, who hastened over to Brussels, where their services were gladly accepted. Among those who went from the Borough school were all (except one) of Mr. Astley Cooper's then apprentices, Mr. Callaway, Mr. Tyrrell, Mr. Henry C. Cooper, and Mr. Key. To the latter was intrusted the care of some French officers; and after several weeks' sojourn in Belgium in attendance on them, Key went to Paris, where he remained for a while enjoying the friendship of some families, who gratefully acknowledged the attention he had shown to their suffering relatives.

Soon after the death of Henry C. Cooper, in 1817, Mr. Key, who had become a great favorite of Mr. Astley Cooper, went to reside with him in Spring gardens, being also at that time engaged to Anne, his niece, and sister of Henry. Here he continued to live till the autumn of 1818, when he was married to Miss Cooper, at Yarmouth, in Norfolk; and, on returning to London, took a house in Lime-street square, where he lived for about three years.

About this time it was determined by Mr. Key and Mr. Morgan to open a summer dissecting-room in Maze-pond, where the pupils of the United Hospital School might continue their anatomical pursuits; the governors not allowing summer dissection within the hospital walls It was kept open for two years, and then given up.

On Mr. Henry Cline's death, in 1820, and Mr. Green's association, in his stead, with Mr. Cooper, in the Anatomical Lectures, Mr. Key was appointed Demonstrator of Anatomy at St. Thomas's Hospital, in Mr. Green's place; an office which he filled with great ability, and to the entire satisfaction of the students, as he was not only a very good anatomist, but also well able to impart the knowledge he possessed, with clearness and precision. At Midsummer in this year, he came to live in St. Thomas's street, in the Borough, with the view, probably, of connecting himself more closely with the hospital.

Mr. Aston Key, as he about this time began to be called, passed his examination, and became a Member of the Royal College of Surgeons of London, on the 5th of January, 1821; and on the 26th of September of the same year, was appointed Assistant-Surgeon to Guy's Hospital, an office specially made for him, although ostensibly for the better attendance of the surgical out-patients. His popularity with and estimation among the pupils, together with the steady support of Mr. Harrison, the Treasurer of Guy's, and of Sir Astley Cooper, who took every opportunity of bringing Key forwards, by giving him operations, carried him onwards; and his own neatness and dexterity in operating, very quickly laid the foundation of his repute as an operator, and fully justified the high opinion Astley Cooper had entertained of his professional ability.

In the early part of February, 1823, Mr. Key resigned the Demonstratorship of Anatomy at St. Thomas's, which he had held till that time, and devoted himself more closely to practice. To further this view, Sir A. Cooper allowed him, towards the latter end of the surgical course in this spring, to give a few lectures on wounds of arteries, and some other subjects. These he again delivered in the following year.

In January, 1824, the two senior surgeons of Guy's Hospital, Mr. Forster and Mr. Lucas, having resigned, Mr. Aston Key and Mr. John Morgan were elected in their stead, Sir Astley Cooper having thus become senior surgeon.

In this year Mr. Key took the bold step of introducing to the profession the operation of lithotomy with the straight staff, and a single knife, with which only all the incisions were made. His account of this method is given in his first publication, entitled 'A Short Treatise on the Section of the Prostate Gland in Lithotomy,' 4to, 1824; but is too well known to require description. The novelty of going through the operation without change of knife, and the dexterity and rapidity with which it was conducted, highly delighted and interested the pupils, and added much to the professional reputation which the newly-elected Surgeon was rapidly acquiring. In connexion with this new method of lithotomy, the following anecdote may not be uninteresting: when the celebrated Baron Dupuytren visited England, he was brought down by Sir Astley Cooper to see Guy's Hospital, and witnessed Mr. Key's performance of this operation, with which he appeared highly delighted, and observed—"Je n'ai jamais vu une opération plus prompte, plus facile, et plus heureuse."—In the course of this year Mr. Key left St. Thomas's street, and settled in St. Helen's Place, where he ever after remained.

The year 1825 was a very important one in Mr. Key's professional life. The separation of the long-united Hospitals of St. Thomas and Guy, and the breaking-up of the joint School, necessarily led to the construction of two new and complete medical schools. In that of Guy's the Surgical Teachership was intrusted to Mr. Key, who in that department did all and more than his warmest friends had anticipated; and although Sir A. Cooper did give, according to promise, for a year or two, a very few Surgical Lectures, yet, beyond dispute, the foundation of the Surgical Class must be attributed wholly to Key's ability and industry, by which, till his resignation in 1844, he retained one of the largest assemblages of students in London.

In 1827 Mr. Key published a second edition of Astley Cooper's great work on 'Hernia,' with additional notes. This did not much increase his professional reputation, as the celebrated author had left little more to be said on the subject. To hernia, however, Mr. Key seems to have devoted special attention; and hence he was led to the revival of Petit's operation of dividing the stricture without opening the sac, of which he gave an account in his 'Memoir on the advantages and practicability of dividing the Stricture in Strangulated Hernia on the outside of the Sac,' 4to, 1838. His recommendation of this operation has been followed as their ordinary proceeding by many able surgeons with success.

Mr. Key contributed several papers of more or less value to Guy's Hospital Reports, one of which especially—"A practical view of Lithotrity, with Remarks on the Operation of Lithotomy," vol. ii, 1837, is a most admirable paper;—and a few also to the Medico-Chirurgical Transactions; of which those on the "Ulcerative Process in Joints," vol. xviii, and "Further Remarks on the Ulcerative Process," vol. xix,—are very excellent and truthful, and deserve the greatest attention.

When the supplementary charter was granted to the College of Surgeons, instituting the Fellowship, and conferring on the Fellows the power of electing a portion of the Council at their annual meeting, Mr. Key was amongst the number of Fellows nominated by the council; and on election day, July 30th, 1845, he was chosen by the fellows as Member of Council. On the arrangement of the medical officers connected with the Court, Mr. Key had the honour of being appointed Surgeon to His Royal Highness Prince Albert.

Whilst thus year after year honours and increased emoluments flowed in upon him, Mr. Key laboured as earnestly as ever in his profession, which was to him as much pleasure as business; his relaxations, excepting his weekly visit from Saturday evening till Sunday night to his country residence, the Lawn, near Hemel Hempstead, were exceeding rare; and even on these occasions he was always ready to attend professional calls in the neighbourhood.

This unremitting attention to practice can scarcely have failed to injure Mr. Key's constitution; especially as he had from early life been a slightly-made and not very strong person. Years before his death he had been the subject of a very severe attack of jaundice, from which he recovered only after a long while. He was rather whimsical in his diet, and would at one time confine himself to one kind of food, and at another time to some other; and immediately after taking his dinner, would employ himself in making his arrangements for the following day. To this negligent mode of proceeding may probably be ascribed the fits of indigestion and disturbed state of bowels, to which he was occasionally subject; so that he would be compelled to lie down almost fainting, and remain in this condition for some time before he could be moved to his bed. Mental anxiety also with him produced the same effect; and it was only a few months since that he was completely knocked up and obliged to have the assistance of his friend Dr. Cobb, in consequence of the anxiety he had suffered from the reported loss of his son Commander Astley Cooper Key's ship Bulldog, which, however, he was speedily informed from the Admiralty was untrue.

The circumstances which belong to the closing hours of Mr. Key's life remain now to be told; and the deep interest and sorrow which the profession have felt in consequence of his sudden removal from the active and honorable career which, humanly speaking, he was expected to run for many years, must be apology for the writer of this short notice detailing scenes, from which in but few instances is it permissible to raise the veil of privacy.

Four days previous to his last illness Mr. Key had dined out; and on meeting his friend Dr. Cobb, on the 20th of August, he jokingly mentioned this, saying, at the same time, he had eaten everything which had come before him, and found himself all the better for it. He was engaged as usual on this and the next day (Tuesday), and neither appeared to have, nor complained of having, anything the matter with him.

On the morning of Wednesday, the 22d of August, Mr. Key rose in his ordinary state of health, breakfasted, and, as was his wont, repaired to the closet about eight o'clock. At nine he was called a second time, which was unusual, to the same place, and had a second and loose dejection. He was engaged in his ordinary occupation of receiving patients from this time till about ten minutes before noon, when he was again obliged to resort to the closet, and had a most enormous discharge. After this he staggered back to his consulting-room, rang his bell, threw himself exhausted on his sofa, and on his servant coming in, said: "It is all up with me; telegraph your mistress." His neighbour, Dr. Cobb, was immediately sent for, and found him with a scarcely perceptible pulse, and sub-fuscous corrugated skin, and merging towards a state of collapse. Dr. Cobb urged on him the propriety of being at once carried up to bed, and his wish that he should have further counsel. To the former Mr. Key replied that he would not be carried, as he was able to walk up stairs; and to the latter, that Dr. Cobb having carried him through a like attack some time before, it was needless to have any one else, but that he expected Dr. Gull very shortly, and if he liked, they might have some talk together. Dr. Cobb left him to procure the medicines which he thought necessary, and on his return found that Mr. Key had, with the assistance of his servant, scrambled up stairs, and having taken off coat and waistcoat, but being too much exhausted to undress farther, was laid on his bed. Dr. Cobb gave him a mixture consisting of chlorate of potassa, hydrochloric acid, and water, which was also freely sprinkled over the bed, so that he might inhale the fumes of the chlorine. Dr. Gull, who had a professional appointment at Mr. Key's house a little before two, arrived about that time, and to his great dismay found his friend lying on the bed in a state of collapse, with a pulse feebly flickering at the wrist, and cold surface. On the Doctor's taking his hand, Mr. Key just opened his eyes, gave him a look of recognition, but said nothing. He continued declining till near three, when an enema of brandy-and-water was thrown up, and at his own request he was allowed to have some tea. He suffered very severely with cramps in his legs, which, however, were relieved by small doses of chloroform in the mixture, and by friction. He also complained of great oppression at the præcordia and towards the left side, for which Dr. Cobb applied a mustard poultice, which gave him exceeding uneasiness, as the sensibility of the surface was so great, that he complained of the heat of a friend's finger, who was feeling his pulse, and begged him to take it away. For the most part he lay on his side, as if sleeping, except when seized with cramp, of which he apprized his attendants by placing his hand upon the affected muscle, and speaking in a low, faint whisper. Towards five o'clock he became restless, and being persuaded to be completely undressed and put to bed, yet, exceedingly weak as he was, he assisted in getting off his clothes, and soon felt more comfortable. Small quantities of beef-tea were given, and the medicine continued, with or without chloroform, according to the presence or absence of cramp. From three up to near seven o'clock both Dr. Cobb and Dr. Gull thought him manifestly improved, but he then threw up everything he had taken, no attempt seeming to have been made at its conveyance beyond the stomach. A second enema of brandy and water was administered, and Dr. Gull, a little after, finding his pulse more distinct, said, "I think you are better;" to which he replied, "I think so too; tell Mrs. Key, it will cheer her."

At about half-past nine, Dr. Babington and Dr. Addison, who had been sent for, arrived, and found their poor friend in a state of extreme prostration, his eyes closed, countenance collapsed and livid, skin icy cold and clammy, tongue moist and cold, and a small, weak, thready pulse, only just perceptible. In consultation with Dr.

Cobb, it was determined that attempts should be made to excite reaction by large and frequent doses of compound spirit of sulphuric ether in moderately strong brandy and water. The medicine was given before Dr. Babington left, and was retained. Dr. Cobb left him at ten o'clock, and during the remaining hours of his life he was carefully tended by Mrs. Key, who had arrived early in the evening, and by Dr. Gull.

About half-past eleven, whilst he was being raised to give the ether, he asked in a scarcely audible whisper, what it was; and on being informed, said, "I should like to have some stout, for Addison wished me to have some." It was soon procured, and he drank freely of it. For nearly three hours after, he remained sensible, but never spoke, excepting that when anything was offered he would ask, in a faint whisper, "What is it?" His surface never recovered warmth, the pulse could not be felt, the respirations became more frequent, and, towards three o'clock of Thursday morning, were rather stertorous and greatly embarrassed. A pattering on the floor, caused by the dripping of fluid through the bed, showed that a watery discharge from the bowels had been going on continually and largely. From this time he was completely insensible, and although the respiratory oppression diminished, the coma continued till his death, at seven o'clock in the morning, which was so easy and gradual that his last breath could not be told. Thus closed, in the zenith of his reputation, the life of one who had been most actively and usefully engaged in his profession, to his advancement in which he had incessantly devoted his every energy. His remains are deposited in his father's family vault at St. Dionis Backchurch, Fenchurch street.

Of Mr. Key's high professional reputation, not merely with the public but among his fellows, there can be no doubt. From his very first entrance into the profession, he made it his great end and object; his whole energies were devoted to it, and the entire course of his life was made subservient to the place he was determined to hold in it. How well he succeeded in achieving his object, is too well known to require further observation. His professional knowledge was almost entirely the result of his own experience, and his surgical lectures an exposition of the facts he had personally noticed. He was possessed of great self-reliance even in early life, as was well known to his fellow-pupils; he needed no leaning-post, and soon showed he was aware of it. He was habitually far-sighted, extremely cautious, well and long thought over the course he would pursue, and steadily followed it. This caution also influenced him in deciding on the performance of operations, and often led him to defer, or even to decline, such as seemed to him of doubtful success. It is not by this statement intended to imply that he usually hesitated either in giving an opinion or in acting upon it; but, as every surgeon knows, cases are not few in which the chance of successful issue of an operation is not great; and upon such, Mr. Key did not enter without much consideration, and thereby gave best proof of his judgment. As an operator he was very expert and smart; his operations for the stone especially were performed with great quickness. And, indeed, on his first assumption of hospital practice, and when closely watched by many of his own standing, to the style of his operations must be mainly attributed his early attainment of a good start in his profession, an advantage which he never after lost.

Mr. Key had the happy power of warmly attaching patients to himself, by impressing on them his thorough knowledge of their ailments, and by throwing himself so entirely into their case as to seem to be completely engaged in it alone. Indeed, he has observed to a friend, "many people endeavour to impress on the minds of their patients the numerous cases to which their attention is directed; my method, however, is, as far as possible, to withdraw my attention from all others, and to give it entirely to that one alone, as if I had none other." When a surgeon acts on such principle, which his patient soon perceives, the speedy attachment of the one to the other follows as matter of course. In addition to this, Mr. Key's liberality towards those who sought his professional assistance, but wanted the means of remunerating it, was great, deserving of all praise, and worthy of emulation. He met his reward in the ample professional income which, for many years he enjoyed, and was one among many instances of the adage, too often forgotten in the sordid pursuit of gain, that "the liberal deviseth liberal things, and by liberal things shall he stand."

It was, however, in his own immediate family relations that Mr. Key especially shone; his attachment to and reliance upon his wife, at whose sole disposal he left his property, and his doating affection for his children, were the greatest enjoyment of his later life. The noble conduct of his son, Commander Astley Cooper Key, at Genoa, within the present eventful year, and the deservedly high position he has thereby most justly acquired, was to him a source of unmixed satisfaction.

Mr. Key has left the following children. Rev. Henry Key, Rector of Stretton, Herefordshire, and formerly of Christ Church, Oxford. Commander Astley Cooper Key, in command of the Bulldog, and at present with the French army at Rome, in a sort of diplomatic station. Anne, married to Henry Heale, Esq., of Highfield House, near Hemel Hempstead. And three other sons, and as many daughters.

THE LATE DR. JOHN REID.

Our readers have all probably been made aware, through earlier channels of information, of the death of this distinguished physiologist and most estimable man, which took place on the 30th of July last, after a protracted and most painful illness. We had the satisfaction not long since (Vol. III, p. 318) of bearing our testimony to the sterling value of his scientific labours, and to his distinguishing excellencies as an experimental physiologist; and to this we shall only now add our conviction, that the results of these labours will continue to be regarded as among the most valuable contributions which experiment has furnished to the advance of physiological knowledge, when others obtained with a hundred times the expense of animal suffering, and announced with infinitely greater pretension, shall have passed into oblivion. It is not our intention to enter at present into any details as to our friend's prematurely interrupted career; nor shall we attempt a full delineation of his character; having been led to expect that such a biography will ere long be given to the public, as will comprise everything that could be desired. But we cannot forego the melancholy satisfaction of paying our own tribute of affectionate regard to one, in whom the virtues of the man were not less eminent than the abilities of the philosopher, and who presented in the closing scenes of his life an example of religious fortitude and submission, that cannot, we should hope, be without its beneficial effect upon every one before whom it is placed.

The writer's acquaintance with Dr. Reid commenced during the Session of 1835-6, in which he was one of the four Presidents of the Royal Medical Society of Edinburgh; at that time Dr. Reid occupied the position of Demonstrator in Dr. Knox's School of Anatomy. In the debates of the Society, especially when they involved questions of Anatomy, Physiology, and Pathology, Dr. Reid took a prominent part; and the writer would not be doing justice to his own feelings or to his friend's memory, if he did not bear tribute to the accuracy of information, clearness of perception, and logical precision of reasoning, displayed by Dr. Reid upon every subject he grappled with; which were almost invariably such as to bear down all his opponents (among whom the writer, trained as he had been under different teachers, was not unfrequently ranked), yet with such perfect *bonhommie*, and with such obvious desire rather for the attainment of truth than for any personal victory, that it was impossible for those most discomfited by his arguments to have any other feeling towards him than that of respect for his abilities and high regard for his mode of using them.

The death of Dr. Fletcher, which occurred at this period, left a vacancy in the extra-academical Lectureship on the Institutes of Medicine, which Dr. Reid was pre-eminently adapted to fill; and the conviction of his qualifications was so strong in the minds of his professional friends, that a requisition was got up, and numerously signed, urging him to take upon himself the duties of the vacant post, and promising him energetic support. On receiving this requisition, it is within the remembrance of the writer that Dr. Reid immediately consulted Dr. Alison, the Academical Professor of the same subject, who advised him by all means to come forwards,—in effect, as his own rival; it is scarcely necessary to point out how strongly this little fact bears testimony to the single-heartedness of *both* the parties concerned. After lecturing for five sessions, in the extra-academical school of Edinburgh with great success, Dr. Reid received the appointment of Chandos Professor of Medicine in the University of St. Andrews; which, whilst it afforded him a moderate income, left him very much at liberty to devote himself to the anatomical and physiological investigations in which he felt most interested. This appointment he received, the writer believes, without any solicitation of his own, but through the strong recommendation of his friend Dr. Alison. Shortly afterwards he married; and having subsequently come into the possession, by the death of his father, of an acceptable addition to his income,—having a young family growing up around him, who received from him a full measure of parental attachment,—being established in a position which afforded him every opportunity of prosecuting the inquiries most congenial to his taste, and of contributing to the advancement of his favorite science,—and enjoying the respect and affection of a large circle of friends,—he seemed in the possession of every earthly blessing. It pleased the All-wise, however, to order differently; and however mysterious the dispensation, none can question it; least of all those who have seen its benefits in the development of that exalted religious sentiment, which was superadded as the crowning excellence upon all that was otherwise estimable in the character of their departed friend.

The frequency of the writer's communications with Dr. Reid having depended rather upon the occurrence of topics upon which they desired to exchange their thoughts, than upon any settled plan of correspondence, it sometimes happened that months elapsed without any direct intercourse. This was the case during the first part of last year; and it was consequently with the deepest pain that the writer received from him the following account of his condition,—a pain, however, not unmingled with satisfaction at the effect of his affliction upon his friend's mental state.

St. Andrews, July 15th, 1848.

"I have lately returned to St. Andrews, after an absence of more than two months in search of health; but instead of being better, there is every probability that my career on earth is drawing to a close. About eight months ago, an ulcer formed on my tongue, to which, for two or three months after its appearance, I paid but little attention. In the beginning of February last, I went to Edinburgh, and consulted some of my surgical friends there regarding it; but they appeared to consider it one of those ulcers that depend upon deranged stomach, as my stomach was very much out of order at the time of its appearance. I took various remedies at the suggestion of Dr. Duncan and Mr. Syme; and it appeared to remain for some time stationary, and even showed some symptoms of amendment. At the close of the winter session, in the beginning of May, I left this to wander about the lakes of Cumberland. Finding this doing me no good, I went up to London for a single day, to consult Sir B. Brodie and Mr. Syme, and when I also saw my old friend Fergusson. Since then I have passed a month at a retired watering-place in the south of Scotland; and about a fortnight ago returned home under the impression that within a short time I shall be called upon to give an account of the deeds done in the body.

"I have been for the last three months disciplining my mind for taking an eternal farewell of earthly things, and endeavouring to make my peace with my Heavenly Father. I have to bless and praise my Maker that he has not cut me off suddenly in the midst of my carelessness and indifference about religion, but has given me timely warning that my sojourn on earth is probably drawing to a close, and has enabled me to take advantage of that warning. It is no doubt a hard and trying thought to leave for ever,—no, I hope, not for ever, for I praise God there is another state of existence where we may again meet,—still it is very hard to make up my mind to leave wife, children, kind relations, and the pursuits in which I have taken so much delight; still it is my duty to bow with submission and resignation to the chastening rod, knowing that it is for my eternal welfare that I have thus been afflicted. My worldly circumstances were assuming a more comfortable aspect;

my constitution, until lately was robust; my age still in its prime (within some months of 40 years); I had formed plans for carrying on investigations into the structure and vital actions of the lower organized bodies, which can be so readily procured from this coast; little thinking that disease was so soon to overtake me. I had my dreams of being able to add something of importance to the deeply attractive and instructive matters embraced in such investigations; and I was looking forward to the time when I should be able to say that I have done something which will prevent me from being readily forgotten. You may say that these were indeed *dreams*, but I believe most people build castles in the air..........If my disease should present any symptoms of amendment, you shall hear from me again."

On the 24th of the same month the writer again heard from Dr. Reid, in reply to a letter of his own; and the following extract will show how well he was continuing to discipline his mind for what still seemed the inevitable though deferred result. After referring to other more personal topics, he continues:

"Your belief that we physiologists will in a future state of happiness have the veil raised from those mysteries of organized bodies into which we cannot at present penetrate, and that those things which we 'now see through a glass darkly' will be revealed to us in all their beauty, is one which is well calculated to yield me comfort and consolation at present. And I sincerely pray that God in his providence may bring us both in due time into his heavenly kingdom, and that we in company may gaze upon those wonders and mysteries in his works here below, into which our feeble intellectual powers cannot *now* penetrate, but which *then* will be fully revealed.

"When I last wrote to you I was in a very feeble state, both mentally and bodily. I had scarcely eaten anything for eight days, and my strength was rapidly giving way; so that I thought it possible I might not have it in my power to write to you again. Since that time my strength has rallied wonderfully; I am again able to take a good deal of exercise in the open air; my appetite is excellent; I am evidently gaining flesh, and I am now fit for some mental labour. This does not, however, inspire me with any hopes of *recovery;* for the disease remains as yet as bad as ever, and I have at times a good deal of pain in that side of the face.

"I often sincerely regret that my mind was not earlier impressed deeply with the paramount importance of a religious life. It strikes me *now* with the utmost surprise, that man should continue to fix his affections entirely upon the fleeting and perishing things of earth; and that he should turn a deaf ear to the kind entreaties, the warnings and the commands of his Heavenly Father, who made him, has supported him, bestowed upon him all that he possesses, and who will one day exact a rigorous account of the things done in the body, unless he turn and repent him of his sins. What a lamentable proof this is of the debasing nature of sin, and of the soul-destroying bondage under which it has placed man.—I find that there is a satisfaction, a deep-felt and soul-satisfying pleasure, in a religious life, which the pleasures of sin cannot afford; so that, apart altogether from the life that is to come, it would be much for man's advantage to lead a life of righteousness."

Not long after this date, when the suffering occasioned by his disease was so much increased as to render it necessary to have recourse to the frequent administration of opium and chloroform in order to procure an occasional and temporary respite, Dr. Reid again sought the advice and assistance of his old friend and colleague, Mr. Fergusson, who was at that time visiting Edinburgh; and was so urgent with Mr. Fergusson to give him the *chance* of benefit from the removal of the disease by operation, that Mr. Fergusson consented; and the excision of about a third of the tongue was performed. The immediate effect of this measure was most satisfactory. The wound healed rapidly, with no other unfavorable result than a deficiency in the power of articulation; the pain entirely ceased; and the flesh and strength gradually returned. Some of the glands of the neck, however, soon began to enlarge; but it was hoped that this might be merely the result of the irritation of the operation. Dr. Reid visited London in October, that he might be for a time under Mr. Fergusson's observation; and during the month which he spent in town, he seemed quite to recover his usual vigour both of body and mind.

He never lost sight, however, of the very doubtful nature of the cure; and his ordinary animation of spirits was evidently chastened by the religious sense of the uncertainty of his tenure of life. Not long after his return to St. Andrews, he experienced a severe affliction in the loss of his only son; of which he spoke as occasioning him more mental suffering than his own illness had given him. It was thought right, also, that the two enlarged glands in his neck should be excised; which operation was performed by his friend Dr. Duncan, of Edinburgh. Shortly before Christmas, he was able to complete the collection of his 'Anatomical, Physiological, and Pathological Researches,' upon the editing of which he had been some months engaged, and the publication of which he had earnestly desired to live to witness. Of the value of this volume there has been but one opinion expressed by those competent to appreciate it.—Having been made aware that indications of the return of the disease had presented themselves, and fearing that Dr. Reid might possibly not live to see the review of his book in the April number of this Journal, the writer sent him a copy of the article as soon as it was in print; at the same time expressing his fear lest the desire to avoid being biassed by private friendship had made the review *less* eulogistic than the work deserved. The following was Dr. Reid's reply, dated March 17, 1849.

"Instead of your notice of my book being less eulogistic than I might fairly expect, I assure you, most sincerely, that I am convinced that you have spoken more favorably of my endeavours to advance the scientific branches of medicine than they deserve, and that your estimate of them has been biassed by personal friendship for the author. Be this as it may, it has afforded me unfeigned satisfaction to know that you entertain so high an opinion of the contents of my book. I know that I have laboured zealously and honestly to arrive at the truth on those points I set myself to examine and investigate; and it is a great satisfaction to find that my labours have not been in vain; for very few of the conclusions at which I arrived, in the different investigations I have entered upon, have been called in question on satisfactory grounds. I do not lay claim to any merit except that of patience and caution.

"I was glad to learn that you and your family have got over the winter so well, and above all, that you have not had the same cause for sorrow as C—— and I have had since I had the pleasure of seeing you in London, four months ago. Both he and I have had to deplore the loss of one of our innocent darlings, who twine themselves so firmly around our hearts that parting from them by their death is like tearing our heartstrings asunder. May it please your Heavenly Father to spare you this misery.

"That my former disease has returned in the back part of my tongue, and isthmus of the fauces, I entertain no doubt. The pain and uneasiness, though sufficiently annoying at times, especially within the last fortnight, are not yet so severe as to prevent me from reading and studying as usual. My general health is remarkably good, my appetite excellent, and I have not yet had recourse to opiates. Part of each week-day I spend in professional study, part in preparing myself for that great and solemn occasion when I shall be called upon to give an account of the deeds done in the body, and part in recreation. My mind is perfectly calm, and I can play with my child, or enter into the enjoyment of innocent mirth, with as much relish as ever. My mind is made up for the worst; and when once this is done, all the fluctuations of hope and doubt are at an end, and the mind settles down into comparative calmness. I still, however, feel that I have not obtained that inward assurance of my Heavenly Father's favour, which some experience under circumstances similar to mine; but I pray earnestly for forgiveness for the sins of my past life, and leave with hope the issue in his hands."

After referring to some of the anatomical inquiries in which he had been engaged, Dr. Reid continues: "Perhaps you may think that I am here exhibiting an instance of the 'ruling passion strong in death;' and I not unfrequently ask myself if it be right in one who will soon bid farewell to this earthly scene, to occupy his mind so much with these matters; but having been the subject of daily thought for so many years, and become almost necessaries of my existence, in fact my mental food and raiment, it is a difficult matter to lay them entirely aside."—The last communication which the writer had from him, was dated June 1st, and had reference chiefly to his researches on the anatomy of the Vaagmar, a remarkable fish of the northern seas, of which he published an account in the 'Annals of Natural History' for June, and which was his last professional labour. In this letter he says: "I have for the last two months suffered severely at different times, from inflammation of the throat, which more than once threatened to suffocate me. I am at present very weak, being confined to bed the greater part of the day. I suffer a good deal of uneasiness and pain at times; but on the whole I have great reason to thank God for his mercies to me in this respect; and I can generally command ease and comparative comfort by a dose of morphia." The letter was completed by Mrs. Reid, who simply and touchingly says of him: "It is painful to him to speak, and he seldom does so; but with all his suffering he is always patient and submissive, and waits the Lord's will for his removal from all his trials."

The period of suffering was protracted for some weeks; but never did the sufferer's faith, hope, or submission fail him; and he remained to the last an example of the same calm heroism, which was manifested in the whole of his deportment from the time when the fatal nature of the disease first declared itself. In the words of one who knew him well, and who truly and lovingly appreciated him,—

"Thou wert a daily lesson
Of Courage, Hope, and Faith;
We wondered at thee living,
And envy thee thy death.

Thou wert so meek and reverent,
So resolute of will—
So bold to bear the uttermost,
And yet so calm and still."*

W. B. C.

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* For the whole of the beautiful little poem from which these verses are extracted, see the 'Edinburgh Monthly Journal,' Sept., 1849.

MEDICAL INTELLIGENCE.

MICROSCOPICAL RESEARCHES ON CHOLERA.

We have been favoured by Dr. Frederick Brittan, Lecturer on Physiology at the Bristol Medical School, with the opportunity of examining the results of his microscopical researches upon certain bodies which appear to be characteristic constituents of cholera evacuations, and to be present also in the atmosphere and atmospheric moisture of districts infected with cholera. These bodies we have no hesitation in pronouncing to be of a *fungous* nature; the atmospheric particles being apparently the sporules, which develope themselves within the body into large aggregations of cells. These aggregate masses have been noticed by many observers who have microscopically examined the evacuations of cholera; but their true nature appears not to have been appreciated by any one save Dr. Brittan and his colleague, Mr. J. C. Swayne, for want of the employment of sufficiently high powers; since the individual cells are so minute, that, when isolated, they would not attract attention, except under an amplification of at least 500 diameters. The researches of these two gentlemen have been prosecuted through a considerable series of cases, with a very uniform result; and the atmospheric moisture has been condensed in several distinct localities. In the last experiment of this kind, carried on in a room in which one patient had died of cholera, and in which two others subsequently died, the water condensed around a decanter of ice was quite discoloured by the quantity of sporules of fungi which it contained. Dr. Brittan appears to have taken every precaution that time and opportunity permitted, to determine, by parallel experiments, whether these bodies are discoverable in the dejections of other patients, and in the atmosphere of uninfected localities; and hitherto with a decidedly negative result.—We would not be understood as committing ourselves, in the present stage of the inquiry, to Dr. Cowdell's Hypothesis of the Fungous Origin of Cholera (see Vol. II, p. 97); but it is obvious that this hypothesis is greatly strengthened by the facts discovered by Dr. Brittan, of which a fuller account will be found in the 'Medical Gazette' for Sept. 28th.

BOOKS RECEIVED FOR REVIEW.

Contributions to the Pathology of Cholera, embracing its History, Modifications, Stages, and Treatment; as the Disease appeared in the Bombay Presidency from 1818 to 1842. By James Bird, A.M., M.D. London, 1849. 8vo, pp. 48.

The History of the Cholera in Exeter in 1832. By Thomas Shapter, M.D. London, 1849. 8vo, pp. 298.

A Treatise on Cholera. By Nathl. Alcock, A.B., M.B. London, 1849. 8vo, pp. 190.

On the Mode of Communication of Cholera. By John Snow, M.D. London, 1849. 8vo, pp. 31.

Mémoire sur le Développement, les Causes, et le Traitement du Choléra. Par Ch. Dvorjak. Saint-Petersbourg, 1848. 8vo, pp. 32.

Ueber die Heilung der jetzt epidemisch herrschenden Cholera, gegründet auf die am Krankenbette gemachtem Beobachtungen und Erfahrungen, von Dr. L. Grünberg, Militärarzte. Warschau, 1848. 8vo, pp. 36.

The above Works will be reviewed in our next.

First Principles of Medicine. By Archibald Billing, M.D., A.M., F.R.S., &c. &c. Fifth Edition. London, 1849. 8vo, pp. 332.

On Gout; its History, its Causes, and its Cure. By William Gairdner, M.D. London, 1849. Post 8vo, pp. 232.

Clinical Lectures on the Physical Diagnosis of Phthisis. By Richard Payne Cotton, M.D. London, 1849. 8vo, pp. 24.

Annotations Cliniques sur la Phthisie Pulmonaire. Par Dr. G. P. F. Groshans. Gand, 1849. 8vo, pp. 58.

Om Spedalskhed, ved D. C. Danielssen og C. W. Boeck. Hermed et Atlas hvori 24 Lithographerede Plancher. Christiania, 1847. 8vo, pp. 516.

Die neure Medicin in Frankreich, nach Theorie und Praxis. Mid vergleichenden Blicken aus Deutschland. Von Dr. Emil Kratzmann. Erste Abtheilung. Leipzig, 1846. 8vo, pp. 264.

Handbuch der Pathologie und Therapie. Von Dr. C. A. Wunderlich. In Drei Bänden. Dritter Band,—III und IV Abtheilung. Erster Band,—I und II Abtheilung. Stuttgart, 1848.

Contributions to the Practice of Medicine: on the Revulsive Agency of the Skin, with Cases. By L. F. Crummey. London, 1849. Fcap. 8vo, pp. 92.

A Dissertation upon Dislocations and Fractures of the Clavicle and Shoulder-joint. Being the Jacksonian Prize Essay for 1846. By Thomas Callaway, F.R.C.S. London, 1849. 8vo, pp. 178. With Three Plates.

Stricture of the Urethra, its Pathology and Treatment; comprising Observations on the Curative Powers of the Potassa Fusa in that Disease; with Cases. By Robert Wade, F.R.C.S. Second Edition.

Portraits of Diseases of the Scalp. By Walter C. Dendy. Fascic. II. London, 1849. 4to, Four Plates.

The Three Kinds of Cod-liver Oil, comparatively considered, with Reference to their Chemical and Therapeutic Properties. By L. J. De Jongh, M.D. Translated from the German, with an Appendix and Cases, by Edward Carey, M.D. London, 1849. 8vo, pp. 176.

The Treatment of Rheumatic Diseases by Lemon Juice; with Illustrative Cases from Hospital Practice. By G. Owen Rees, M.D., F.R.S. London, 1848. 8vo, pp. 38.

A Treatise on the Public Health, Climate, Hygiene, and Prevailing Diseases, of Bengal and the North-west Provinces. By Kenneth Mackinnon, M.D. Cawnpore, 1848. 8vo, pp. 396.

Sketches of the Medical Topography and Native Diseases of the Gulf of Guinea, Western Africa. By William F. Daniell, M.D., Assistant-Surgeon to the Forces. London, 1849. 8vo, pp. 200.

On Parthenogenesis, or the Successive Production of Procreating Individuals from a Single Ovum. By Richard Owen, F.R.S., &c. With a Lithographic Plate. London, 1849. 8vo, pp. 76.

Researches on the Development, Structure, and Diseases of the Teeth. By Alexander Nasmyth, F.L.S., F.G.S., F.R.C.S. London, 1849. 8vo, pp. 230. With Ten Plates.

The Physiology of Digestion, considered with relation to the Principles of Dietetics. By Andrew Combe, M.D. Ninth Edition, edited by James Coxe, M.D. Edinburgh, 1849. Fcap. 8vo, pp. 192.

Microscopic Anatomy of the Human Body in Health and Disease. By Arthur Hill Hassall, M.B. Part XV. London, 1849. 8vo. With Nine Plates.

Fruits and Farinacea the proper Food of Man; being an Attempt to prove from History, Anatomy, Physiology, and Chemistry, that the original, natural, and best Diet of Man is derived from the Vegetable Kingdom. By John Smith. Second Edition. London, 1849. Fcap. 8vo, pp. 342.

The Elements of Botany. By M. Adrien de Jussieu. Translated by James H. Wilson, F.L.S., &c. London, 1849. Fcap. 8vo, pp. 750. With numerous Woodcuts.

A Manual of Botany; being an Introduction to the Study of the Structure, Physiology, and Classification of Plants. By John H. Balfour, M.D., F.L.S., F.R.S.E., Professor of Botany in the University of Edinburgh. London, 1849. Post 8vo, pp. 642. With numerous Woodcuts.

Rudiments of Botany: a Familiar Introduction to the Study of Plants. By Arthur Henfrey, F.L.S. With illustrative Woodcuts. London, 1849. 18mo, pp. 250.

Om Fodselsstiftelsen i Christiania, Læreanstalt for Universetetets Studerende og Jordemoderelever i Aarene 1818—1846, 1847, 1849. Mortalitet, Barselsygdomme, Börnesygdomme med et Foredrag om Puerperalaffectioner. Af Overlæge Dr. F. C. Faye. Christiania, 1849. 8vo, pp. 160.

Confessions of a Hypochondriac, or the Adventures of a Hyp. in Search of Health. By M.R.C.S. London, 1849. Post 8vo, pp. 310.

Der Geist des Menschen in der Natur, oder die Psychologie in Uebereinstimmung mit der Naturfunde. Von Dr. Joseph Ennemoser. Stuttgart, 1849. 8vo, pp. 774.

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